Controllability of Risk and the Design of Incentive-Compensation Contracts

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Abstract: We examine how the ability to control firm exposure to risk affects the design of executive compensation contracts. To do so, we use the introduction of exchanged-traded weather derivatives, which significantly increased executives' ability to control their firms' exposure to weather risk, as a natural experiment. We find that executives for whom weather derivatives have the greatest impact on the ability to control firm exposure to weather risk experience relative declines in total compensation and equity incentives. The former finding is consistent with a reduction in the risk premium that executives receive for their firms' exposure to weather risk. The latter finding suggests that risk and incentives are complements when executives can control their firms' exposure to risk. Collectively, our results show that the executives' ability to control their firms' exposure to risk alters the nature of agency conflicts and influences the design of incentive-compensation contracts.

Keywords: executive compensation; contract design; equity incentives; risk-taking incentives; stock options; derivatives; hedging; natural experiment

JEL Classification: G32, J33, J41

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1. Introduction

The theoretical agency literature highlights the costs of agency conflicts that result when executives have information that principals do not (Lambert, 2001). Although providing executives with incentives tied to stock price can sometimes alleviate these agency conflicts, doing so exposes executives to risk that requires payment of a commensurate risk premium. Consequently, firms trade off the benefit of providing incentives against the cost of exposing executives to the associated risk. While this tradeoff leads to relatively straightforward predictions about the effect of risk on executives' *compensation*, the effect of risk on executives' *incentives* is theoretically ambiguous and depends on the extent to which executives' can influence (or "control") their firms' exposure to risk (Jenter, 2002; Prendergast, 2002; Hemmer, 2006, 2012; Edmans and Gabaix, 2011a). Therefore, we examine whether executives' ability to control their firms' exposure to risk affects the design of their incentive-compensation contracts.

However, empirically identifying the effect of executives' ability to control risk on the design of their incentive-compensation contracts is challenging for two reasons. First, risk and incentives are endogenously related (Demsetz and Lehn, 1985; Aggarwal and Samwick, 1999; Core and Guay, 2002a). A correlation between risk and features of executives' compensation contracts does not necessarily reflect the *causal* effect of risk because boards are likely to design contracts based on factors unobservable to the researcher that also affect firm risk (e.g., executives' risk tolerance). Second, it is difficult to decompose observed risk into controllable and uncontrollable portions. To overcome these challenges, we use the introduction of exchange-traded weather derivative contracts as a natural experiment that provided an arguably exogenous shock to executives' ability to control their firms' exposure to weather risk (Perez-Gonzalez and Yun, 2013).

Our study is premised on the widely accepted notion that risk-averse executives—who are undiversified by virtue of their firm-specific equity holdings and human capital—are potentially exposed to a variety of uncontrollable risks that can give rise to agency problems. Prior to the introduction of weather derivatives, it was difficult (i.e., costly, if at all feasible) for these executives to alter their firms' exposure to weather risk. Weather derivatives were a financial innovation that allowed executives to hedge for the first time, or, at a minimum, significantly reduced the cost of hedging, weather risk (Perez-Gonzalez and Yun, 2013). Therefore, by bringing firms' exposure to particular risks—at least in part—under executives' control, hedging makes uncontrollable risks controllable from an agency-theoretic perspective.[1] Consequently, a change in executives' ability to hedge should affect their incentive-compensation contracts in several important ways.

First, the ability to hedge risk should affect the amount of executives' annual compensation. A portion of executives' annual compensation represents a risk-premium to compensate them for bearing the risk associated with their performance-based incentives and firm-specific human capital (see Core, Guay, and Larcker, 2003 for a review; see also Core and Guay, 2010 and Armstrong, Core, and Guay, 2017). If hedging allows executives to eliminate some of this risk, then they will be exposed to less firm-specific risk and, consequently, should demand and receive less of a risk premium in their annual pay. Accordingly, we expect the introduction of weather derivatives to lead to a relative reduction in the annual compensation of more affected executives.

Second, executives' ability to hedge risk should also affect the design of their equity incentives. Although the prevailing view in the empirical literature is that risk should have a

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[1] In other words, executives' ability to hedge their firms' exposure to a particular risk whose realization is "uncontrollable" in the sense of not being susceptible to executives' actions essentially makes that risk "controllable" from shareholders' perspective and, in turn, "controllable" from an agency-theoretic contract design perspective.

negative relation with the level of executives' equity incentives, several theoretical studies show that the relation is theoretically ambiguous (Holmstrom, 1979, 1982; Jenter, 2002; Hemmer, 2006, 2012; Edmans and Gabaix, 2011a; Guo and Ou-Yang, 2015).[2] The intuition for the theoretical ambiguity of the relation between risk and incentives is simple: similar to the mean of a performance measure, the variance (or risk) may be determined by the executive's actions.[3] The stylized models that predict a negative relation between risk and incentives ignore this possibility. These models feature an additive error structure in which the executive's action shifts the mean of a normally distributed performance measure, but has no effect on its exogenously specified variance. In contrast, Edmans and Gabaix (2011a) allow the executives' actions to affect risk and find that doing so leads to a *complementary* relation between risk and incentives. Therefore, we expect that the introduction of weather derivatives, which increased the ability of executives to affect firm risk, will lead to a relative reduction in the equity incentives of more affected executives.

A summary of our research design and empirical findings is as follows. Throughout our analyses, we use both traditional and "fuzzy" difference-in-differences designs that accommodate executives' presumably endogenous hedging choices. We find that the CEOs of firms with greater historical exposure to weather risk are more likely to use weather derivatives to hedge this risk. In particular, these firms experience a statistically significant and economically meaningful relative

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[2] Even though theoretical (e.g., Jenter, 2002; Hemmer, 2006, 2012) and empirical studies (e.g., Demsetz and Lehn, 1985; Core and Guay, 1999, 2002a) have made this point, there is still a widespread belief that risk should have a negative relation with incentives.

[3] The technical reasons largely relate to the validity of the so-called first-order approach (FOA), which is frequently invoked as a way to solve the bi-level optimization that characterizes principal-agent models. The FOA replaces the first-order condition for the optimum of the agent's incentive compatibility (IC) constraint in the principal's objective function to produce a "relaxed" and more tractable formulation of the problem. Several authors (e.g., Mirlees, 1974; Rogerson, 1984; Jewitt, 1988) have characterized the restrictive conditions that are necessary to ensure the validity of the FOA. Two of the more well-known conditions are the Convexity of the Distribution Function Condition (CDFC) and the Monotone Likelihood Ratio Condition (MLRC). However, as Hemmer (2006) notes, distributions that satisfy these conditions typically do not yield tractable solutions or capture the empirical properties of the parameters of interests (e.g., stock price); nor are they easily ranked in terms of riskiness based on simple summary statistics.

reduction in the covariance between their stock returns and weather outcomes following the introduction of weather derivatives. We corroborate this indirect evidence of increased hedging by searching our sample firms' 10-K filings for references to weather derivative contracts and find that these firms are significantly more likely to discuss the use of these contracts.

Second, we find that CEOs who use weather derivatives to reduce their firms' exposure to weather risk receive relatively less total annual compensation—including both the cash and equity grant components. This relative reduction in annual compensation is consistent with a decrease in the risk premium that these CEOs receive for having their wealth and human capital exposed to weather risk.

Third, we find that CEOs who use weather derivatives to hedge their firms' exposure to weather risk experience relative declines in their equity incentives. This finding, coupled with our evidence that their firms experienced a significant relative reduction in exposure to risk, is evidence of a *positive*, rather than a negative relation, between risk and incentives when executives can affect their firms' exposure to risk. Although this finding is entirely consistent with theoretical predictions from many prevalent agency models (e.g., Holmstron, 1979; Holmstrom and Milgrom, 1987; Edmans and Gabaix, 2011a), it is at odds with empirical studies that predict a negative relation between risk and incentives (e.g., Aggarwal and Samwick, 1999; Gao, 2010).

We contribute to the incentive-compensation and corporate hedging literatures in several ways. First, we identify how CEOs' ability to alter their firms' exposure to an uncontrollable risk, effectively making this risk controllable from an agency-theoretic perspective, affects the design of their incentive-compensation contracts. Much of the prior empirical research in this area has focused on how CEOs' incentives influence their corporate risk-taking decisions (e.g., Agrawal and Mandelker, 1987; DeFusco et al., 1990; Guay, 1999; Rajgopal and Shevlin, 2002; Coles et al., 2006; Low, 2009; Armstrong and Vashishtha, 2012). We examine the converse: how firm risk influences the design of CEOs' incentive-compensation contracts.

Prior empirical studies that examine how risk affects the design of incentive-compensation contracts provide mixed evidence about the direction of the relation between risk and CEOs' incentives (e.g., Demsetz and Lehn, 1985; Lambert and Larcker, 1987; Garen, 1994; Bushman et al., 1996; Aggarwal and Samwick, 1999; Core and Guay, 1999; Gormley, Matsa, and Milbourn, 2013; Armstrong, 2013). To the best of our knowledge, none of these prior studies considers how the controllability of risk affects the relation between risk and incentives. Moreover, some studies attribute the mixed evidence to the endogenous design of CEOs' incentive-compensation contracts (Aggarwal and Samwick, 1999; Core and Guay, 2002a). We acknowledge this important concern and contribute to this literature by examining an exogenous shock to CEOs' ability to hedge firm risk, which, in turn, allows us to estimate the effect of controlling risk on CEOs' compensation and incentives.

Second, we contribute to the literature on corporate hedging. Prior studies examine whether hedging affects firm value and, more generally, why hedging is done at the corporate level rather than by shareholders directly (e.g., Modigliani and Miller, 1958; Mayers and Smith, 1982; Perez-Gonzalez and Yun, 2013; Gilje and Taillard, 2016). Our finding that corporate hedging leads to a reduction in the risk premium that undiversified executives receive for being exposed to firm risk highlights an important channel through which hedging can mitigate agency conflicts and increase firm value (Mayers and Smith, 1982; Stulz, 1984; Smith and Stulz, 1985).

Third, we clearly present the requirements, identifying assumptions, and limitations of traditional difference-in-differences, which has become an increasingly popular technique for estimating causal effects. Not only are these assumptions often unstated, but they are also frequently violated—particularly the perfect compliance assumption. Perfect compliance requires that no firms received the treatment in the pre-treatment period and that all firms in the treatment group—and only those firms—received the treatment in the post-treatment period (Angrist, Imbens, and Rubin, 1996; Blundell and Dias, 2009). In our setting, some treatment firms did not hedge and some control firms did hedge, both of which are violations of perfect compliance. We show how to address imperfect compliance by developing a fuzzy difference-in-differences design. As we explain, this approach can estimate treatment effects that traditional difference-in-differences cannot and can "salvage" certain natural experiments that might otherwise be unusable due to imperfect compliance. Therefore, we also make an important methodological contribution by showing how imperfect compliance affects estimates from traditional difference-in-differences designs, how to use fuzzy difference-in-differences when compliance is imperfect, and articulate and contrast the different assumptions behind these two alternative designs.

The remainder of our paper is organized as follows. We provide background information on weather derivatives and discuss related studies in Section 2. We describe our research design in Section 3 and discuss our sample, data sources, and variable measurement in Section 4. We present our primary results in Section 5 and the results of several supplemental analyses in Section 6. We provide concluding remarks in Section 7.

2. Background

2.1. Weather derivatives

Weather derivatives are financial contracts whose payoffs is determined by reference to the realization of certain climatic conditions such as temperature and precipitation (e.g., rainfall and snowfall), or the occurrence of extreme events (e.g., hurricanes). A typical weather derivative contract specifies the following parameters: (1) an underlying weather measure (e.g., temperature or cumulative precipitation); (2) the location at which the weather is measured (e.g., a weather measurement station); (3) the contract period; (4) the exercise or "strike" price; and (5) a function that maps the realized weather measure to the contract's monetary payout (Considine, 2000).

The most common type of weather derivatives are temperature-based futures that come in one of two varieties: Heating Degree Day and Cooling Degree Day contracts (hereafter referred to as HDD and CDD, respectively). HDD and CDD capture—and can therefore be used to hedge—the energy demand for heating and cooling services, respectively.[4] The payoff of these contracts is based on the cumulative difference between the daily temperature and 65 degrees Fahrenheit (18 degrees Celsius) during a certain period of time (e.g., one month). The baseline temperature (i.e., 65 degrees Fahrenheit) is set at a level at which there is relatively little demand for heating and cooling. HDD contracts pay off if the cumulative temperature is relatively *low* and, conversely, CDD contracts pay off if the cumulative temperature is relatively *high*.[5]

The following excerpt from Washington Gas Light Co.'s 2007 Annual Report (Form 10K) provides an example of a weather derivative contract used to hedge weather risk.

> *On October 5, 2006, Washington Gas purchased a new HDD derivative designed to provide full protection from warmer-than-normal weather in Virginia during the upcoming 2006-2007 winter heating season. Washington Gas will receive $25,500 for every HDD below 3,735 during the period October 15, 2006 through April 30, 2007.*

This contract was based on the number of HHDs, which is the contractual measure of the underlying weather outcome. The contract covered the period October 15, 2006 through April 30, 2007 (essentially the winter of 2006-07) and had an exercise (or "strike") price of 3,735. If the

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[4] According to the Chicago Mercantile Exchange, the trading volume of CME weather futures during 2003 more than quadrupled from the previous year and equaled roughly $1.6 billion in notional value.

[5] $CDD = Max\{0, 1/2*(T_{max}+T_{min})-65\}$ and $HDD = Max\{0, 65-1/2*(T_{max}+T_{min})\}$, where T_{max} and T_{min} are the maximum and minimum temperature, respectively, measured in degrees Fahrenheit over a specific period.

winter had been warmer than usual, Washington Gas would have received $25,500 for each HDD below the strike price. The winter of 2006-07 turned out to be colder than usual, and the actual HDD was 3,955, which exceed the contract's strike price. Accordingly, Washington Gas was not entitled to any payment from this particular weather derivative, and the contract expired worthless.

Prior to the introduction of weather derivatives, firms with significant exposure to the weather had only a limited number of financial instruments with which they could hedge this risk. Moreover, those instruments that were available (e.g., individual contracts with large property and casualty insurers acting as counterparties) often provided an imperfect hedge and were potentially very costly. For example, firms could potentially use agriculture commodity futures to hedge weather risk because weather conditions also affect commodity prices and demand. However, agricultural commodity futures provide imperfect hedges and are subject to basis risk. Alternatively, firms could purchase a weather insurance contract from a property and casualty insurer. However, like most insurance contracts, weather insurance contracts only provide protection against catastrophic damage and do nothing to protect against the reduction in demand that a utility might experience when the weather is warmer or colder than expected.

Weather derivatives also differ from conventional insurance contracts in several other important respects. First, weather derivatives are financial instruments with payoffs that are tied to objective, measurable weather events such as hours of sunshine, amount of precipitation, snow depth, temperature, or wind speed. Weather stations around the country measure these realizations, and the contracting parties cannot influence these realizations. Consequently, the contractual payoffs are difficult, if not impossible to manipulate. In contrast, insured parties can manipulate loss payments from conventional insurance contracts, giving rise to potentially significant moral hazard problems.

Second, the loss settlement process for weather derivatives depends on measurements (e.g., temperature or hours of sunshine) that are collected for other purposes and therefore represent a negligible marginal cost of contract settlement. In contrast, the settlement process for conventional insurance contracts usually entails costly investigation and verification at the loss site, and can even involve litigation before reaching a final resolution of the claim. Third, credit risk is present with insurance contracts, although this risk is somewhat limited by monitoring from insurance regulators, external audits, and credit and claims-paying rating agencies. In contrast, many weather derivatives trade on exchanges, which virtually eliminates any credit risk.[6] Fourth, exchange-traded weather derivatives incur relatively low transaction costs, making it feasible for firms to dynamically hedge their exposure. In contrast, insurance contracts cannot be traded and premature cancellation typically involves significant penalties and other transaction costs.

Absent suitable financial instruments with which to hedge, executives can also engage in "real actions" to hedge their firms' risk. For example, a firm could diversify its operations across either product lines or geographic regions to reduce its total exposure to the weather. However, implementing these and other diversification strategies can be costly and prior studies question their efficacy (Berger and Ofek, 1995; Lamont and Polk, 2002). Moreover, these and other types of "real actions" can also introduce additional agency conflicts between executives and shareholders.

Utilities may use regulatory measures to minimize the impact of weather. Specifically, a weather normalization adjustment (WNA) is a method of adjusting customers' bills to reflect normal, rather than actual, weather conditions, which effectively allows utilities to reduce their exposure to weather risk. However, WNAs do not cover the unregulated portion of utilities'

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[6] Although credit risk remains with over-the-counter weather risk trading, protection is provided by the International Securities and Derivatives Association and external audits of financial records.

business and are not available in every state. Moreover, the cash flow recovery from WNAs lags weather shocks, particularly in extreme cases, and their use is potentially subject to moral hazard on the part of consumers, as well as regulatory and political risk. To summarize, although there were ways that executives could reduce their firms' exposure to weather risk prior to the introduction of weather derivatives, they were imperfect and costlier than are weather derivatives.

The first over-the-counter (OTC) weather derivative contract was introduced in 1997, primarily in response to severe and unexpected weather conditions caused by the 1997-98 El Nino-Southern Oscillation (ENSO). According to the Weather Risk Management Association, the total value of weather derivative contracts traded on the Chicago Mercantile Exchange was about $8 billion in 2003 and increased to roughly $45.2 billion by 2006.[7] Not surprisingly, 70% of the end-users of weather derivatives are members of the energy industry (WMRA, 2005).

2.2. Risk and incentives

Empirical contracting studies that predict a negative relation between risk and incentives typically appeal to Holmstrom (1979) and Holmstrom and Milgrom (1987).[8] Holmstrom (1979) and Holmstrom and Milgrom (1987) model the principal's problem as one of designing a contract that balances the benefit of increasing the sensitivity of the agent's pay to the performance measure (i.e., improved incentive alignment) against the cost of imposing more risk on a risk-averse and undiversified agent. A more restrictive version of these models features a linear compensation contract, negative exponential utility, and a normally distributed performance measure with a mean

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[7] http://usatoday30.usatoday.com/weather/forecast/2008-06-09-weather-derivative_N.htm.

[8] Prendergast (2002) surveys the empirical incentive-contracting literature and documents the widespread prevalence of the belief that risk and incentives should share a negative relation. He also develops a model that predicts a positive, rather than a negative relation between risk and incentives. His model highlights the tradeoff between incentives and monitoring and shows how a principal might want to rely more on incentives when there is greater uncertainty in the operating environment (i.e., risk) and monitoring the executive's inputs (e.g., effort) is relatively costlier than observing output (e.g., firm performance). Although our results are largely consistent with Prendergast's (2002) predictions, we do not explicitly test for a substitution from incentives to monitoring following a decrease in risk.

that is a deterministic function of the agent's (personally costly) action and an exogenous variance (or "noise"). The benefit of this so-called LEN framework is that its highly-stylized—but arguably unrealistic—assumptions are sufficiently tractable to produce a closed-form solution for the optimal (second-best) contract. However, the model is tractable because it places severe restrictions on the contracting environment, the implications of which empirical contracting studies often ignore (Lambert, 2001).

For example, the stylized model assumes that the performance measure (e.g., stock price) equals the agent's action plus a normally distributed error that is mean zero and has a constant, exogenous variance. This additive error structure implies that the executive's action affects only the mean of the performance measure and has no effect on higher moments. However, Hemmer (2002, 2006) highlights that the mean and variance of distributions with a lower bound (e.g., the price of a stock with limited liability) are usually positively correlated. Consequently, actions that increase the mean of the performance measure also increase its variance. By allowing *both* the mean *and* the variance (or risk) of performance to endogenously depend on the executive's action, Hemmer (2006) shows that risk and incentives will have a *positive* relation if stronger incentives are required to elicit greater effort.

Another unattractive implication of assuming that the performance measures' mean and variance are uncorrelated is that the agent's action has a *deterministic* effect on the performance measure. However, it is unrealistic to assume that the agent knows *for certain* how his actions affect expected performance because this link is likely subject to at least some—if not substantial—uncertainty. For example, it is more difficult to disentangle the effects of an agent's actions on firm performance from the effects of other factors that are beyond the agent's control in more uncertain environments. As Meulbroek (2000) explains, just as "rowing does not affect [a] boat's progress very much relative to the effect of a hurricane," risk that is beyond an agent's control reduces the agent's willingness to exert effort for a given level of incentives. Consequently, more incentives are required to offset a weaker link between effort and performance, leading to a *positive* relation between risk and incentives.[9]

Edmans and Gabaix (2011b) model the principal-agent problem in continuous time with simultaneously determined risk and actions, and in discrete time with risk realized before the action. The authors explicitly highlight how their model builds on the stylized model (pg. 2,896): "Our framework develops a quite different set of sufficient conditions, which may be satisfied in many settings in which the [stylized model's] assumptions do not hold and tractability was previously believed to be unattainable. In addition, while the [stylized model's] setup delivers linear contracts, our setting also accommodates convex and concave contracts." Edmans and Gabaix (2011a) extend this framework by adding a talent assignment problem and show that when the agent can affect risk (e.g., via hedging), incentives and risk are positively related.

The theoretical ambiguity of the relation between risk and incentives is one potential explanation for the mixed evidence in prior empirical studies. For example, Demsetz and Lehn (1985), Core and Guay (2002a), and Oyer and Shaefer (2005) present evidence of a positive relation between risk and incentives, while Lambert and Larcker (1987), Aggarwal and Samwick (1999), and Jin (2002) find evidence of a negative relation. Further complicating inferences, studies such as Garen (1994), Yermack (1995), Bushman, Indejikian, and Smith (1996), and Ittner, Larcker, and Rajan (1997) find no significant relation between risk and incentives.[10] In summary,

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[9] Conversely, reducing the amount of risk that is beyond an executive's control can result in the need for less incentives because each "unit" of incentives reflect the executive's actions with greater precision, so fewer "units" of incentives are required to elicit the desired level of effort.

[10] Studies in other settings that involve sharecroppers and franchisees generally find either a positive or no significant relation between risk and incentives (see Prendergast, 1999 and 2002 for a review of these related literatures).

a closer inspection of the theoretical literature and a survey of prior empirical studies provides little support for the widespread belief that risk should have a negative relation with incentives.

2.3. Corporate hedging

Under a restrictive set of assumptions, Modigliani and Miller (1958) demonstrate that corporate hedging is, at best, a value-neutral activity.[11] However, the prevalence of corporate hedging and insurance is striking (Mayers and Smith, 1982). Motivated by the widespread incidence of corporate hedging and insurance, subsequent authors have relaxed the Modigliani-Miller assumptions and have offered several potential explanations for corporate hedging, including (i) reducing the cost of financial distress and bankruptcy (Smith and Stulz, 1985; Mayers and Smith, 1990; Bessembinder, 1991; Géczy, Minton, and Schrand, 1997; Haushalter, 2000), (ii) reducing underinvestment (Froot, Scharfstein, and Stein, 1993; Gay and Nam, 1998), (iii) reducing tax expenses (Mayers and Smith, 1982; Smith and Stulz, 1985; Graham and Rogers, 2002), (iv) speculating (Géczy, Minton, and Schrand, 2007), (v) rent extraction by entrenched executives (Kumar and Rabinovitch, 2013), and (vi) reducing the risk premium that undiversified employees demand for their exposure to firm–specific idiosyncratic risk (Stulz, 1984; Smith and Stulz, 1985). Our study adds to this literature by using the introduction of weather derivatives to examine how the ability to hedge risk affects the design of executives' incentive-compensation contracts.

3. Research Design

The introduction of weather derivatives in 1997 provided firms with an efficient way to manage (i.e., hedge) their exposure to weather risk. The introduction of weather derivatives was arguably exogenous with respect to any particular firm and with respect to executives' expectations

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[11] The Modigliani-Miller assumptions include frictionless markets, no taxes, no information asymmetries, no bankruptcy costs, no agency costs, and equal costs of borrowing and hedging for firms and individuals.

of the outcomes that we examine. Further, we expect that the executives of firms historically more affected by local weather conditions to disproportionately benefit from weather derivatives.

3.1. The effect of weather hedging on the design of incentive-compensation contracts

3.1.1. Traditional difference-in-differences

We use the introduction of weather derivatives as an arguably exogenous source of variation in executives' ability to control their firms' exposure to risk. To do so, we first estimate the following traditional difference-in-differences specification:

$$Compensation_{it} = \beta_{0,it} + \beta_{1,it} After_t \times Treatment_i + \gamma' X_{it} + FirmFE + YearFE + \varepsilon_{it} \qquad (1)$$

where i and t index firms and time, respectively. X represents a vector of control variables, which we discuss in more detail below. *FirmFE* denotes firm fixed effects, which are included to abstract away from (i.e., "control for") time-invariant features of firms and firms' contracting environments (e.g., industry membership). Similarly, *YearFE* denotes year fixed effects, which are included to abstract away from systematic temporal effects (e.g., ENSO).[12] *After* is an indicator that equals one from 1998 onwards and zero otherwise, and delineates the post-introduction period.

Treatment measures the degree to which the introduction of weather derivatives affected firm i. Weather derivatives likely had the greatest impact on firms that were more exposed to weather risk prior to their introduction. Intuitively, more historical risk means more risk exposure that weather derivatives can potentially control. Therefore, we define *Treatment* as several different measures of firms' historical (i.e., pre-1997) exposure to variation in weather conditions.

The β_1 coefficient in Eq. (1) provides an estimate of the average treatment effect (ATE) of the introduction of weather derivatives on *Compensation* so long as the four difference-in-differences assumptions are satisfied. Although these assumptions are crucial to estimate causal

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[12] Note that the main effects of *After* and *Treatment* are absorbed by the time- and firm-fixed effects, respectively.

effects and although difference-in-differences research designs are becoming increasingly common, these assumptions are frequently unstated. Given the importance of these assumptions for credible causal inference, we now discuss them in the context of our research setting. Note that the following discussion refers to a binary treatment for ease of exposition. The case of a continuous treatment relies on analogous assumptions (Blundell and Dias, 2009).

Assumption 1: Common (or "parallel") trends in outcomes. This assumption implies that treated firms would have had the same change in outcomes as untreated firms had they not received the treatment.[13] In our setting, the parallel trends assumption is satisfied as long as *changes* in *Treatment* are otherwise exogenous with respect to *changes* in CEOs' compensation contracts after the introduction of weather derivatives. Because treatment firms' outcomes in the absence of treatment are counterfactual (and therefore unobservable), the parallel trends assumption is inherently untestable. However, we explicitly test whether the treatment and control firms had different trends in the pre-treatment period in Section 6. This falsification test provides assurance that the parallel trends assumption is not violated in our setting by demonstrating that our results are not an artefact of differential pre-treatment trends, and by demonstrating that our results only obtain around the introduction of weather derivatives (e.g., if we define treatment as occurring in 1995, we do not find similar results).

Assumption 2: The stable unit treatment value assumption ("SUTVA"). SUTVA requires that the treatment status of one firm does not affect other firms' potential outcomes. In our setting, SUTVA implies that the decision by some firms to hedge their weather risk does not affect the incentive-compensation contracts of executives at other firms. Like the parallel trends assumption,

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[13] The parallel trends assumption in difference-in-differences replaces the exclusion restriction in instrumental variables, which requires that the treatment is (conditionally) mean independent of the *level* of the potential outcomes, with the weaker assumption that the treatment is (conditionally) mean independent of the *change* in potential outcomes.

SUTVA is inherently untestable because treated and untreated firms' counterfactual outcomes are unobservable. However, SUTVA is unlikely to be violated in our setting because our sample firms do not directly affect the realized weather, but rather adjust their exposure to the weather.

Assumption 3: No effect of treatment on the pre-treatment populations ("NEPT"). NEPT requires that firms did not adjust their pre-treatment outcome in anticipation of receiving the treatment. In our setting, NEPT implies that firms did not adjust their CEO's compensation contracts prior to the introduction of weather derivatives in anticipation of being able to use weather derivatives in the future. Because the weather derivative market developed largely in response to the unexpected severity of the 1997-98 ENSO event, it is unlikely that firms foresaw the advent of this market. Moreover, even if firms had foreseen the development of weather derivatives, they are unlikely to have altered their CEOs' incentive-compensation contracts in anticipation. Nevertheless, we explicitly test for differential changes in outcomes prior to the introduction of weather derivatives in Section 6 and find no evidence to suggest that this assumption is violated in our setting.

Assumption 4: Perfect compliance. Perfect compliance requires that no firms received the treatment in the pre-treatment period and that all firms in the treatment group—and only those firms—received the treatment in the post-treatment period. In the case of linear treatments, perfect compliance requires that treatment is a deterministic function of the treatment variable. In our setting, perfect compliance would be violated if some firms in the treatment group *did not* hedge or if some firms in the control group *did* hedge. This assumption may not be satisfied in our setting because firms in the treatment group were not required to hedge their weather risk and firms in the control group were not precluded from hedging their weather risk using weather derivatives.

When there is imperfect compliance, the β_1 coefficient in Eq. (1) captures a weighted average of (i) zero effect for firms that do not comply with treatment, and (ii) the effect of the treatment on firms that do comply (Blundell and Dias, 2009). Consequently, the treatment effect estimated by β_1 will not correspond to the local average treatment effect (LATE) or the ATE. Instead, β_1 will capture the relative effect of introducing weather derivatives on the treated. Therefore, β_1 is analogous to an intention to treat (ITT) estimate in the epidemiology literature because it captures both firms that hedge (i.e., took the medicine) because the introduction of weather derivatives "nudged them" into doing so, and firms that did not change their behavior. In light of the possibility—and indeed, the likelihood—of imperfect compliance in our setting, we develop an alternative identification strategy based on "fuzzy" difference-in-differences to estimate the LATE that corresponds to the treatment effect of controlling risk on the subsample of compliers.

3.1.2. Fuzzy difference-in-differences

Many natural experiments are not amenable to the traditional difference-in-differences framework because either the treatment or control groups (or both) exhibit imperfect compliance. In this case, there are no "sharp" treatment and control groups, but only "fuzzy" treatment and control groups for which the members' *probability* of compliances differs.[14] In our setting, firms can be classified into four categories that correspond to those in Angrist, Imbens, and Rubin's (1996) framework for causal inference when compliance is imperfect:

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[14] It is likely the case that very few natural experiments involve perfect compliance. For example, two widely cited natural experiments are the Vietnam military draft lottery and the mandatory adoption of IFRS. However, in both settings, compliance is likely imperfect. Individuals may be able to avoid military service even when drafted and firms may use loopholes to avoid an IFRS mandate. Similarly, undrafted individuals may still voluntarily enlist in the military and drafted individuals may enlist prior to being drafted. Firms in non-IFRS countries could still voluntarily prepare IFRS-compliant reports, and firms in IFRS countries could voluntarily prepare IFRS-compliant reports prior to the mandate. Any of these behaviors is sufficient to violate the perfect compliance assumption.

(i) firms that reduce their exposure to (i.e., hedge) weather risk because of the introduction of weather derivatives ("compliers"),

(ii) firms that never reduce their exposure to (i.e., hedge) weather risk either before or after the introduction of weather derivatives ("never-takers"),

(iii) firms that reduce their exposure to (i.e., hedge) weather risk both before and after the introduction of weather derivatives ("always-takers"), and

(iv) firms that *increase* their exposure to (i.e., speculate on) weather risk because of the introduction of weather derivatives ("defiers").

Traditional difference-in-differences requires the treatment and control groups to be composed entirely of compliers and never-takers, respectively (the presence of always-takers or defiers are sufficient to prevent traditional difference-in-differences from estimating an ATE). In other words, traditional difference-in-differences entails comparing pre- versus post-treatment outcomes (i.e., the first difference) between the treatment and control groups (i.e., the second differences) for which the pre- and post-treatment status are strictly (0,1) and (0,0). In the case of linear treatment effects, traditional difference-in-differences requires that treatment status is a *deterministic* function of the treatment. The presence of never-takers always-takers in our sample implies the absence of the "sharp" treatment and control groups.

To address this issue, we modify the difference-in-differences specification given by Eq. (1) to model the differential probability of treatment using the following two-stage estimation:

$$\textit{Weather Risk}_{it} = \alpha_{0,\mathrm{it}} + \alpha_{1,\mathrm{it}}\textit{After}_t \times \textit{Treatment}_i + \lambda'X_{it} + \textit{FirmFE} + \textit{YearFE} + \varepsilon_{it} \tag{2a}$$

$$\textit{Compensation}_{it} = \theta_{0,\mathrm{it}} + \theta_{1,\mathrm{it}}\textit{Predicted Weather Risk}_{it} + \mu'X_{it} + \textit{FirmFE} + \textit{YearFE} + u_{it} \tag{2b}$$

Eq. (2a) uses $\textit{After}_t \times \textit{Treatment}_i$ as an instrument for the endogenous variable, $\textit{Weather Risk}_{it}$. Eq. (2b) uses the fitted values of $\textit{Weather Risk}_{it}$ from Eq. (2a) to instrument for treatment status in a difference-in-differences specification. Eqs. (2a) and (2b) essentially combine instrumental variables and traditional difference-in-differences estimators to produce a "fuzzy" difference-in-differences estimator.

The coefficient θ_1 in the second-stage Eq. (2b) is the Wald-DID estimator, and demonstrates the equivalence between fuzzy difference-in-differences and traditional difference-in-differences coupled with instrumental variables:

$$\text{Wald-DID} = \frac{DID_{Outcome}}{DID_{Treatment}} \tag{2c}$$

where the DID of a random variable Z is given by $DID_Z = [\mathrm{E}(Z_{t_2,g_t}) - E(Z_{t_1,g_t})] - [\mathrm{E}(Z_{t_2,g_c}) - E(Z_{t_1,g_c})]$; t_0 and t_1 denote the pre- and post-treatment periods; g_t and g_c denote the treatment and control groups; and E(.) is the expectation operator. The numerator of the Wald-DID in Eq. (2c) captures the effect of the instrument (i.e., $After_t \times Treatment_i$) on the average outcome and is equivalent to the β_1 coefficient from the traditional (or "sharp") difference-in-differences specification given by Eq. (1). The denominator captures the proportion of the population that responds to the treatment (i.e., the proportion of compliers), and is equivalent to the α_1 coefficient in Eq. (2a).

Fuzzy difference-in-differences entails three significant departures from traditional (or "sharp") difference-in-differences (Hudson, Hull, and Liebersohn, 2017). First, fuzzy difference-in-differences relaxes the perfect compliance assumption. Second, fuzzy difference-in-differences estimates a LATE rather than the ATE. In our setting, the LATE corresponds to the causal effect of controlling weather risk on compliers (Angrist et al., 1996; Blundell and Dias, 2009).[15] In other

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[15] The LATE may not correspond to the treatment effect for either the always-takers or the never-takers if there are heterogeneous treatment effects. Typically, always-takers voluntarily choose treatment because they expect to benefit from doing so and, conversely, never-takers avoid treatment because they do not expect to benefit (e.g., Heckman, Urzua, and Vytlacil, 2006). Hence, the LATE may be the most informative treatment effect since it is an estimate of the effect of treatment on firms that are on the margin between choosing and avoiding the treatment. In other words,

words, Eq. (1) estimates the effect of the introduction of weather derivatives (the "intention to treat" estimate), while Eq. (2) estimates the effect of using weather derivatives to hedge risk (the "treatment effect" estimate).

The third significant departure is that fuzzy difference-in-differences requires two additional identifying assumptions:

Assumption 5: Monotonicity. In our setting, monotonicity implies that the introduction of weather derivatives did not make firms increase their exposure to weather risk (i.e., that there are no defiers). Similar to the parallel trends and SUTVA assumptions, monotonicity relates to how firms would have behaved in the absence of treatment. Because we cannot observe the counterfactual, the assumption is untestable. However, it is unlikely that any utility firms used weather derivatives to *increase* their exposure to weather risk because these are the firms that are most exposed to this source of risk.[16] Therefore, monotonicity is likely to be valid in our setting. This is also an important benefit of focusing on utilities, as the monotonicity assumption would likely be violated in a larger, more heterogeneous sample of firms. Nonetheless, even if monotonicity is violated estimates of the treatment effect will be attenuated as long as the effect of increasing and decreasing hedging is symmetric (Heckman et al., 2006).

Assumption 6: Instrument relevance. This assumption requires that the differential probability of treatment for the treated group relative to the control group is significant enough to avoid "weak instrument" problems. An instrument's relevance can be assessed by examining the test statistics on the α_1 coefficient from the first-stage given by Eq. (2a). In our fuzzy difference-

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it is rarely informative to know the effect of treatment on never-takers since they would be expected to take actions to avoid doing so. Similarly, understanding the effect of treatment on always-takers is rarely informative because always-takers are unlikely to change their behavior.

[16] Note that the presence of some firms that increase their exposure to weather risk following the introduction of weather derivatives does not imply a violation of the monotonicity assumption. As long as these firms did not increase their risk *because of the introduction of weather derivatives*, monotonicity is not violated. A violation of monotonicity requires that these firms would not have increased their risk absent the introduction of weather derivatives.

in-differences tests, we follow Stock and Yogo (2005) to assess the relevance of $After_t$ × $Treatment_i$ as an instrument.[17]

4. Variable Measurement and Sample Selection

4.1. Sample selection

The sample period for our primary tests runs from 1993 to 2002, spanning the five years prior to and the five years following the introduction of weather derivatives. We start with 370 unique utilities that engaged in the generation or distribution of electricity or natural gas (Standard Industrial Classification Codes 4911, 4923, 4924, 4931 and 4932). We then require the following information for each firm: (i) the location of the firm's headquarters (we lose 49 firms), (ii) at least ten years of quarterly data prior to 1997 to estimate the firm's historical weather exposure (we lose 68 firms), (iii) valid historical temperature measurements in the firm's county from the North America Land Data Assimilation System available from Center for Disease Control and Prevention (CDC),[18] (iv) Execucomp data to calculate incentive-compensation measures (we lose 45 firms), and (v) financial information from Compustat and CRSP. We also require that the firm has at least one year of data before and after the introduction of weather derivatives for the difference-in-differences specification (we lose 96 firms). Our final sample consists of 112 unique utility firms and 899 firm-year observations for which we have the required data for all of our analyses.

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[17] The monotonicity and instrument relevance assumptions are necessary because fuzzy difference-in-differences relies on instrumental variables to model relative compliance. Instrumental variables also requires that the exclusion restriction be satisfied. In our setting, the exclusion restriction requires that $After_t$ × $Treatment_i$ only affects *changes* in executive compensation through its effect on changes in firms' exposure to weather risk. This assumption is equivalent to the parallel trends assumption.

[18] http://wonder.cdc.gov/nasa-nldas.html.

4.2. Treatment firms

We use a continuous treatment variable, defined as the sensitivity of firms' revenue to weather fluctuations prior to the introduction of weather derivatives. The introduction of weather derivatives had a greater effect on firms with greater historical sensitivity to weather fluctuations because weather derivatives allowed these firms to control a greater share of firm risk.

Following Perez-Gonzalez and Yun (2013), we estimate the following specification:

$$Rev/Assets_{it} = \beta_{0,i} + \beta_{1,i} EDD_{it} + \gamma_i \ln(Assets_{it}) + \varepsilon_{it} \tag{3}$$

where $Rev/Assets_{it}$ is quarterly revenue scaled by ending total assets. *EDD* proxies for total energy demand and is the sum of daily CDD and HDD in each quarter. CDD and HDD are calculated as $\text{Max}\{0, 65\text{-}½*(T_{max}+T_{min})\}$ and $\text{Max}\{0, ½*(T_{max}+T_{min})\text{-}65\}$, respectively.[19] *EDD* is measured at the location of the firm's headquarters.[20] T_{max} and T_{min} are the maximum and minimum daily temperature measured in degrees Fahrenheit, respectively. We also include the natural logarithm of total assets as a measure of firm size as a way to control for fluctuations in revenue attributable to sources other than the weather.

We estimate Eq. (3) separately for each firm in our sample using quarterly Compustat data from 1980 to 1997, and we require each firm to have at least 40 quarterly observations. The estimated coefficient β_1 captures the sensitivity of revenue to variation in energy demand. *Treatment* measures treatment intensity and is defined as the product of the absolute value of the estimated beta ($|\widehat{\beta_1}|$) and the historical standard deviation of EDD (σ_{EDD}) during the 1980-1997

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[19] We obtain similar results when we use CDD or HDD as a measure of energy demand.

[20] Compustat reports the address of a firm's current principal executive office, which could be different from its historical address if the firm has changed the location of its headquarters. To address potential errors in headquarter locations, we extract historical headquarter locations from the firm's historical 10-K filings available on the SEC's Edgar database. If the historical 10-K is not available for a particular year, we use the 10-K from the closest available year.

estimation period, multiplied by 100 to ease interpretation.[21] *Treatment* captures the historical revenue volatility that is attributable to weather fluctuations.

4.3. Measurement of compensation and incentives

We examine attributes of CEOs' incentive-compensation contracts using data from the Execucomp database. The first four measures are related to the composition (or "mix") and magnitude (or "level") of CEOs' annual compensation and are (i) *CashComp*, the natural logarithm of the sum of the CEO's annual salary and bonus payments, (ii) *EquityComp*, the natural logarithm of the adjusted Black-Scholes value of the CEO's option and restricted stock grants received during the year, (iii) *TotalComp*, the natural logarithm of the value of the CEO's total annual compensation (i.e., salary, bonus, restricted stock and option grants, and long-term incentive plan payouts), and (iv) *EquityMix*, defined as the unlogged value of *EquityComp* divided by the unlogged value of *TotalComp*.

In addition to these four measures of CEOs' annual (or "flow") compensation, we also examine two common measures of the incentives provided by CEOs' equity portfolio (i.e., stock and option holdings). The first measure is *Portfolio Delta*, which captures the sensitivity of a CEO's equity portfolio value to changes in stock price. The second measure is *Portfolio Vega*, which captures the sensitivity of a CEO's equity portfolio value to changes in volatility of stock returns. We follow prior literature (e.g., Core and Guay, 1999; Coles, Daniel, and Naveen, 2006; Burns and Kedia, 2006) and measure *Portfolio Delta* as the natural logarithm of the change in the risk-neutral (Black-Scholes) value of the CEO's equity portfolio for a 1% change in the firm's stock price and *Portfolio Vega* as the natural logarithm of the change in the risk-neutral (Black-

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[21] Since utilities can benefit from hedging weather risk irrespective of the sign of these betas, the absolute value of the beta is informative about firms' hedging opportunities. We consider alternative measures of *Treatment* in Section 6.

Scholes) value of the CEO's equity portfolio for a 1% change in the risk of the company's stock (measured by standard deviation of the firm's return).[22,23]

We include the following control variables identified by prior research (e.g., Core, Holthausen, and Larcker, 1999; Core, Guay, and Larcker, 2008): *CEO Tenure* measured as the natural logarithm of one plus the number of years the executive has held the CEO title; *Firm Size* measured as the natural logarithm of the firm's total assets; *Firm Age* measured as the natural logarithm of one plus number of years since stock price data for the firm becomes available from CRSP; the *Book-to-Market* ratio is included to capture growth opportunities; and *ROA* and *Stock Return* to measure firms' accounting and stock market performance, respectively.

Perez-Gonzalez and Yun (2013) find that firms with greater historical exposure to weather risk increase their debt capacity and investment following the introduction of weather derivatives. Therefore, we also control for (i) *Leverage,* which is measured as the sum of short- and long-term debt minus cash holdings, scaled by total assets, and (ii) capital expenditures (*CAPEX),* which is measured as annual capital expenditures scaled by total assets, to ensure that any changes in executives' compensation and incentives that we document are attributable to the change in firms'

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[22] We calculate the parameters of the Black-Scholes formula as follows. Annualized volatility is calculated using continuously compounded monthly returns over the previous 60 months, with a minimum of twelve months of returns, and winsorized at the 5th and 95th percentiles. If the stock has traded for less than one year, we use the imputed average volatility of the firms in the Standard and Poor's (S&P) 1500. The risk-free rate is calculated using the interpolated interest rate on a Treasury Note with the same maturity (to the closest month) as the remaining life of the option, multiplied by 0.70 to account for the prevalence of early exercise. Dividend yield is calculated as the dividends paid during the previous twelve months scaled by the stock price at the beginning of the month. This is essentially the method described by Core and Guay (2002b).

[23] An alternative to the dollar-holdings measure of the incentive to increase stock price is the fractional-holdings measure, calculated as the change in the (risk-neutral) value of the executive's equity portfolio for a $1,000 change in firm value (Jensen and Murphy, 1990). Baker and Hall (2004) and Core, Guay, and Larcker (2003) discuss how the suitability of each measure is context-specific and depends on how the CEO's actions affect firm value. When the CEO's actions affect the dollar returns of the firm (e.g., consuming perquisites), fractional holdings is a more appropriate measure of incentives. When the CEO's actions affect the percentage returns of the firm (e.g., strategic decisions), dollar holdings are a more appropriate measure of incentives. Since we are concerned about strategic actions that affect the firm's risk profile, we rely on the dollar-holdings measure of incentives.

ability to hedge rather than changes in these corporate attributes. A more detailed description of the variables can be found in the Appendix.

4.4. Measurement of weather derivative usage and weather risk

Our difference-in-differences tests rely on the assumption that firms with greater historical weather exposure engaged in more hedging following the introduction of weather derivatives. We assess the validity of this assumption in two ways. First, we hand collect information on whether firms use weather derivatives after 1997 to gauge the extent of derivative hedging. We use a web crawling program to search for weather derivative keywords in every quarterly and annual report filed by our sample firms during the 1997 to 2002 period. We use the following keywords that are unique to weather derivative hedging to infer weather derivative usage: "Weather Derivative", "Cooling Degree Day", "Heating Degree Day", "CDD", and "HDD." If a firm-year's reports do not contain any of these hedging keywords, we classify that firm-year as nonuser.[24]

Second, we assess whether the sensitivity of firms' equity returns to weather realizations declined following the introduction of weather derivatives. To obtain an annual measure of firms' exposure to weather risk, we estimate the following model of each firm's daily stock returns over a one-year period as a function of the three Fama-French factors and a measure of daily weather realizations:

$$Ret_{i,t} = \beta_0 + \beta_1 Size_t + \beta_2 Hml_t + \beta_3 Mkt_t + \beta_4 EDD_t + \varepsilon_{i,t} \tag{4}$$

Where i indexes firms and t indexes time. *EDD* is the sum of HDD and CDD. We only estimate Eq. (4) for firm-years with at least 60 daily observations. We refer to the estimated coefficient β_4 as a firm's "weather beta," or *Beta-FF*. We also estimate a variant of Eq. (4) that includes a

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[24] We did not use the notional value of hedging instruments because SFAS 133, which requires firms to recognize all derivatives as either assets or liabilities in the statement of financial position and measure those instruments at their fair value rather than their notional value, was introduced in late 2000.

momentum factor to obtain an alternative measure of weather beta, which we refer to as *Beta-FFM*.

It is important to note that utilities can potentially benefit from hedging weather risk irrespective of the sign of their weather beta. For example, some firms may benefit from abnormally cold weather, whereas others may be adversely affected by cold weather conditions. Therefore, the absolute value of the estimated coefficient β_4 captures the sensitivity of the firm's equity returns to weather fluctuations. We also multiply the absolute value of the estimated weather betas by the annualized volatility of EDD to obtain an alternative measure of weather risk that captures the proportion of a firm's stock return volatility that is attributable to weather exposure. We refer to these alternative measures as *Risk-FF* and *Risk-FFM*. We use each of the four measures of weather risk and the measure of derivate usage as dependent variables in the first-stage regression given by Eq. (2a).

4.5. Descriptive statistics

Table 1 presents descriptive statistics for our sample. All continuous variables are winsorized at the 0.5% percentile in each tail. Panel A reports descriptive statistics for different measures of weather risk. The Fama-French three-factor model and the Carhart (1997) four-factor model both produce similar estimates. In particular, Panel A shows that the average return sensitivity to weather is 0.75 and that weather betas exhibit substantial dispersion (standard deviations of 0.86 and 0.90 when calculated with the three- and four-factor models, respectively). These estimates indicate that the utilities in our sample have relatively large average exposure to the weather and exhibit substantial variation in their exposures. Our measure of historical revenue volatility attributable to weather fluctuations, *Treatment*, also exhibits substantial dispersion: its standard deviation is 2.94 compared to its mean of 2.22.

Panel B of Table 1 reports descriptive statistics for the various compensation and incentive variables. The mean (median) of our sample CEOs' annual cash compensation is \$849,000 (\$738,000) and the average *Equity Mix* is 22%. The mean (median) sensitivity of their equity holdings to stock price and stock return volatility, *Portfolio Delta* and *Portfolio Vega*, are 3.44 (3.45) and 2.27 (2.61), respectively. Because our sample firms are drawn from a relatively unique industry, we also report the average values of the incentive-compensation measures for non-utilities in the Execucomp database. Panel B shows that the CEOs in our sample receive less total compensation and have lower levels of equity incentives than their counterparts in other industries.

Panel C of Table 1 reports descriptive statistics for various firm and CEO characteristics. The average (i.e., mean) tenure of the CEOs in our sample is 6.4 years and the average firm has total assets of \$7,543 million. The average stock market and accounting returns of our sample firms are 10% and 3%, respectively. In addition, our sample firms have an average book-to-market ratio of 0.67 and leverage ratio of 0.37. We also report descriptive statistics for the non-utility firms in the Compustat database for comparative purposes.[25] These descriptive statistics indicate that the firms in our sample tend to be larger and more levered, and have fewer growth opportunities than their counterparts in other industries. These differences are not surprising because utilities are more heavily regulated and asset intensive, which explains their larger size and the differences in their capital structure. The differences that we document are also consistent with prior studies that examine utilities (e.g., Rajgopal and Shevlin, 2002; Jin and Jorion, 2006; Perez-Gonzalez and Yun, 2013).

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[25] The mean ROA of -0.18 reported in Panel C of Table 2 is partially due to "penny stocks." If we exclude firms with share price of \$5 or less, the mean (median) ROA is -0.06 (0.03).

5. Results

5.1. Sensitivity of stock returns to weather

Our first set of tests examine whether firms' adjusted their exposure to weather risk following the introduction of weather derivatives. The results in column (1) of Table 2 show that firms' tendency to use weather derivatives is increasing in their treatment intensity.[26] Though it is unlikely that utility firms used weather derivatives to speculate as opposed to hedge their exposure to weather risk, we address this potential issue by examining the impact of the introduction of weather derivatives on weather-related risk exposures and overall return volatilities.

Columns (2) through (5) of Table 2 presents results from estimating the sensitivity of our sample firms' equity returns to weather realizations. The two sets of columns report estimates for weather risk based on the Fama-French three factor model and the four factor model that also includes a momentum factor (modified Fama-French model), respectively. The results from both specifications indicate that firms' relative exposure to (i.e., co-movement with) weather fluctuations following the introduction of weather derivatives is decreasing in their treatment intensity. Moreover, the economic magnitude of the relative reduction in risk for a one standard deviation increase in treatment intensity is large: when weather risk is calculated using the modified Fama-French model, the relative reduction in exposure to weather equates to 16% of the sample mean.

In Column (6) and (7) of Table 2, we present further evidence that firms' risk is decreasing in their treatment intensity. Column 6 presents results when measuring risk using the natural logarithm of firms' total stock return volatility and Column 7 presents results when measuring risk

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[26] Column (1) presents estimates from a linear probability model. We obtain similar results when we estimate a logit model with industry and year fixed effects. Due to the "incidental parameters problem," the logit specification does not allow us to include firm fixed effects.

using the log of firms' idiosyncratic return volatility. We find that both total stock return volatility and idiosyncratic return volatility are decreasing in their treatment intensity following the introduction of weather derivatives. Overall, our evidence suggests that our sample firms used weather derivatives to hedge at least some of their weather risk and experienced a meaningful reduction in their exposure to weather risk.

5.2. Traditional difference-in-differences

5.2.1. CEO compensation

Our next set of tests examines whether several aspects of CEOs' annual compensation changed following the introduction of weather derivatives. The results reported in column (1) of Table 3 indicate that for one standard deviation increase in treatment intensity, total annual compensation declined by roughly 11.1% (*t*-statistic of -4.13) following the introduction of weather derivatives. Columns (2) and (3) indicate that the decline in total annual compensation comes from a reduction in both its cash and equity components.[27] This decline in total annual compensation is consistent with our prediction that weather derivatives allow executives to hedge risk that they would have otherwise had to bear and, consequently, they receive less of a risk premium in their annual compensation (Core and Guay, 2010; Conyon, Core, and Guay, 2011).

Column (4) reports estimates for *EquityMix*. The coefficient on *After*Treatment* shows that the proportion of CEOs' compensation paid in the form of stock and options is decreasing in their treatment intensity. Together with the results in the first three columns, this finding indicates that the more treated CEOs in our sample not only receive relatively less total annual compensation following the introduction of weather derivatives, but that they also receive relatively less of their

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[27] We obtain similar results when we jointly estimate the two equations for cash and equity compensation using seemingly unrelated regression (SUR) (Zellner, 1962), which accommodates correlation between the residuals of the two equations. We estimate SUR using the Stata command SUREG. Since this Stata routine does not allow for clustering of standard errors, we use bootstrapped standard errors.

compensation in the form of equity (i.e., restricted stock and options). This finding is also consistent with firms intentionally substituting away from equity incentives following a reduction in firm risk.

5.2.2. CEO equity portfolio incentives

Table 4 presents the results of estimating our models of CEOs' equity portfolio incentives. The first column examines how the introduction of weather derivatives affected the sensitivity of CEOs' equity portfolio values to changes in stock price, or *Portfolio Delta*. The coefficient on *After*Treatment* is negative and statistically significant (*t*-statistic of -4.30), indicating that the magnitude of CEOs' equity incentives is decreasing in the intensity of their treatment following the introduction of weather derivatives. We find similar results for *Portfolio Vega*: the coefficient on *After*Treatment* is negative and significant (*t*-statistic of -3.05). The coefficient estimates in columns (1) and (2) suggest that a one standard deviation increase in treatment intensity results in an 18.6% decline in CEO equity incentives following the introduction of weather derivatives.

An auxiliary prediction is that risk-averse executives should be willing to hold their options longer following the introduction of weather derivatives because of the reduction in their exposure to firm risk (Hemmer, Matsunaga, and Shevlin, 1996). We construct a variable, *Unex/Total*, defined as the ratio of the (Black-Scholes) value of vested (i.e., exercisable) in-the-money options to the value of all vested options, to measure the timeliness of CEOs' option exercise. Consistent with our prediction, we find that CEOs' tendency to hold a larger relative proportion of vested in-the-money options following the introduction of weather derivatives is increasing in their treatment intensity. Combined with the change in granting behavior by the board, this result suggests that the decrease in CEOs' *Portfolio Vega* for higher levels of treatment intensity is attributable to boards re-optimizing the executives' compensation contracts in light of the changes to firm-specific risk. Moreover, coupled with our finding that firms with greater historical exposure to weather risk experience significant reductions in risk following the introduction of weather derivatives, our finding that their executives' equity incentives also declined is evidence of a *positive*, rather than a negative relation between risk and incentives.

5.3. Fuzzy difference-in-differences

To the extent that firms imperfectly comply with the treatment, traditional (or "sharp") difference-in-differences may not accurately capture the magnitude of the relation between controlling weather risk and CEO compensation and incentives. Instead, difference-in-differences provides an intention to treat estimate that does not match the treatment effect. Therefore, we estimate the effect of controlling weather risk on compensation contracts using fuzzy difference-in-differences.

We use *Beta-FF* as our primary measure of firms' exposure to weather risk and note that we obtain similar results when we use the other measures of weather risk exposure from Table 2. We report the estimates of the first-stage regression given by Eq. (2a) in Column (1) of Table 5. The coefficient on *After*Treatment* is negative and significant at the 1% level, suggesting that the instrument relevance assumption (*Assumption 6*) is satisfied. In addition, the first-stage *F*-statistic of 27.25 is well above the recommended minimum value of ten (Stock and Yogo, 2005).[28] The results from estimating Eq. (2b) presented in Column (2) to (8) of Table 5 continue to show a positive relation between controllable risk and executives' incentives and compensation.

The coefficient estimates from the fuzzy difference-in-differences specification are equivalent to those from the traditional difference-in-differences specification scaled by the

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[28] More precisely, Stock and Yogo (2005) show that if the first-stage *F*-statistic for all instruments is greater than ten, the maximum bias of the instrumental variables estimator will be less than 10%. Subsequent work has adopted the "rule of thumb" that first-stage *F*-statistics greater than ten are acceptable (Roberts and Whited, 2010, 516).

relative proportion of compliers in the sample (see Eq. (2c)). The local average treatment effects imply that a one standard deviation increase in treatment intensity results in a 12% relative decrease in total compensation, a 25.6% relative decrease in equity compensation, a 20.5% relative decrease in *Portfolio Delta*, and 21.6% relative decrease in *Portfolio Vega* following the introduction of weather derivatives. The test statistics associated with the fuzzy difference-in-differences estimates are similar to their counterparts from the traditional difference-in-differences because fuzzy difference-in-differences scales (i.e., multiplies) both the coefficient estimates and the standard errors by the same constant—namely the relative proportion of compliers from the first-stage.[29]

5.4. Can CEOs increase utility by hedging?

Using both traditional and fuzzy difference-in-differences models, we find that the CEOs of firms with greater historical exposure to weather risk choose to hedge weather risk using weather derivatives and, consequently, receive less total annual compensation and equity incentives. It may seem puzzling that CEOs would make a choice that results in lower compensation. However, undiversified and risk-averse executives trade off the risk of their compensation and wealth against the amount of compensation when maximizing their expected utility. Hedging allows CEOs to reduce their exposure to uncontrollable risk, the benefit of which can offset the associated loss of compensation. In this section, we develop a numerical model that examines the effects of hedging on CEOs' utility.

Our model specification follows Lambert et al. (1991). We assume that the CEO is risk averse and his preferences can be represented by the power utility function, $U(w)=w^{(1-a)}/(1-a)$,

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[29] Note that the standard errors of the fuzzy difference-in-differences estimator are adjusted to reflect the use of predicted rather than observed variables in the second-stage. This adjustment has a modest effect on the standard errors.

where w is the CEO’s terminal wealth and a is the CEO’s coefficient of relative risk aversion. w_0 denotes the CEO’s initial wealth, n denotes the number of stock options, q denotes the number of shares, and δ denotes firm risk. We can write the CEO’s end of period wealth as:

$$w(\delta, n, q) = w_0 + n * \mathrm{Max}(s(\delta) - k, 0) + q * s(\delta)$$

Where s(δ) is the firm’s end of period stock price, which we assume follows a lognormal distribution with mean μ and variance δ. k is the exercise price of the stock options. The CEO chooses to vary the firm risk (δ) to maximize expected utility from his terminal wealth. The CEO’s maximum expected utility can be expressed as follow

$$EU^{*}(\delta, n, q) = \mathrm{Max}\ E[u(w(\delta, n, q))]$$

The certainty equivalent of the CEO’s portfolio can be written as

$$CE(\delta, n, q) = [(1-a)\, EU^{*}(\delta, n, q)]^{1/(1-a)}$$

We solve the CEO’s optimization problem numerically (the non-linear payoffs of the options precludes a closed-form solution).

Figure 1 illustrates the effect of decreasing risk on the CEO’s certainty equivalent of his equity portfolio and utility. We plot changes in certainty equivalent of CEO’s portfolio against changes in utility for a 0.5% reduction in firm risk. Consistent with the intuition that CEOs trade off the amount of compensation with the risk of that compensation, Figure 1 shows that changes in the certainty equivalent of compensation are a non-linear function of changes in utility when risk is decreasing. When risk is high, a 0.5% reduction in firm risk results in an increase in the certainty equivalent of compensation and utility. However, when overall firm risk falls to a relatively low level, a further reduction in risk results in a relative reduction in the CEO’s certainty equivalent of compensation and an increase in the CEO’s utility.

6. Sensitivity Analysis

We conduct several supplemental analyses to assess the sensitivity our primary inferences to our maintained identifying assumptions outlined in Section 3.

6.1. Evaluating the parallel trends assumption

Inferences from both difference-in-differences specifications rely on the maintained assumption that, absent the treatment, both treated and control firms would have continued to exhibit similar trends in the outcomes of interest (*Assumption 1*). Our inferences also rely on the assumption that firms did not adjust their pre-treatment outcomes in anticipation of receiving the treatment (*Assumption 3*). To assess the validity of these assumptions, we examine whether firms with relatively high and relatively low exposure to weather exhibit parallel trends before the introduction of weather derivatives. To do so, we estimate a specification that is analogous to Eq. (1), except that we replace the *After* indicator with separate indicators for each of the two years preceding, the year of, and the two years following the introduction of weather derivatives: *After(t=-2), After(t=-1), After(t=0), After(t=1)* and *After(t>=2).*[30]

We present the results of this specification in Table 6. None of the pre-event variables are significant at conventional levels, consistent with the maintained assumption that firms did not change their hedging behavior in anticipation of the introduction of weather derivatives. This also suggests that firms with relatively high and low exposures to the weather had similar—and therefore parallel—trends prior to the introduction of weather derivatives.

6.2. State-level industry deregulation and changing business prospects and policies

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[30] An alternative would be to assess the parallel trends assumption using a modified fuzzy difference-in-differences model. However, such a test would produce larger standard errors and lead to an increased risk of Type I errors. Because this test is an attempt to *falsify* the parallel trends assumption by finding evidence of differential pre-treatment trends, an increased risk of Type I errors would bias this test towards incorrectly failing to reject the parallel trends assumption.

Electricity in the U.S. was traditionally supplied by regional monopolies that owned both the power plants and the transmission lines used to distribute power. Because of the utilities' monopolistic power, states heavily regulated utility companies, setting their rate of return based on their cost of services. Deregulation was triggered by a series of federal actions, which were followed by the passage of state laws ordering the separation of power plants from the distribution facilities. The Energy Policy Act of 1992 was the first act to curb utilities monopolies by expanding the Federal Energy Regulation Commission's (FERC) authority. On April 24, 1996, the FERC issued Order 888, a landmark ruling that required utilities to open their power transmission lines to independent producers. FERC's intent was to introduce competition at the wholesale level and to keep utilities from using their control of the transmission system to limit the entry of lower priced generation.

The primary result of Order 888 was to force many states to deregulate the utility industry, due to concerns that interstate competition would price out regulated monopolies. By 2000, 24 states had passed laws deregulating their utility industries. However, within the next eight years, ten states had repealed or delayed their deregulation laws, mainly as a response to the California Energy Crisis of 2000-01.[31] By 2008, there were only 12 U.S states where utility industries were completely deregulated.

To mitigate concerns that our results might be confounded by the effects of state level industry deregulation, we re-estimate our main tests after including state of location and year joint fixed effects. After including these additional fixed effects, the resulting specification estimates

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[31] The 24 states that deregulated are Arizona, Arkansas, California, Connecticut, Delaware, Illinois, New Jersey, New Hampshire, Maine, Maryland, Massachusetts, Michigan, Montana, Nevada, New Mexico, New York, Oklahoma, Ohio, Oregon, Pennsylvania, Rhodes Island, Texas, Virginia, and West Virginia. By 2008, ten states had repealed or delayed their deregulation laws (Arkansas, Arizona, Illinois, New Mexico, Nevada, Michigan, Oklahoma, Oregon, Virginia and West Virginia).

the difference between firms with different treatment intensities located in the same state at the same point in time, and therefore subject to the same state regulations. These additional fixed effects ensure that any observed treatment effect is due solely to the differences in treatment (i.e., the introduction of weather derivatives) rather than any concurrent regulatory or state economic effects. The results of this analysis, presented in Panel A of Table 7, continue to show a positive relation between controllable risk and executives' incentives and compensation (also note that the unreported first-stage *F*-statistic on the excluded instrument remains well above the recommended value of ten).

A related concern is that changes in state policies or rulings might affect firms based on their state of *incorporation* rather than their state of *location*. To address this related concern, we include state of incorporation and year joint fixed effects. The resulting specification compares firms with different treatment intensities that are incorporated in the same state at the same point in time. We present the results of this analysis in Panel B of Table 7. Again, the coefficient on *Pred Beta-FF* remains largely unchanged. We conclude that our results are not driven by changes in state economics or regulations.

6.3. SFAS133 adoption and shorter event windows

The choice of any particular sample period in a difference-in-differences analysis entails a cost-benefit tradeoff. The benefits of a longer window are twofold. First, expanding the window utilizes more data, which, in turn, produces more powerful statistical tests. Second, a wider window allows more time for both boards' contracting decisions and executives' risk-taking decisions to take effect and manifest in the data. The cost of using a wider window is that it increases the likelihood of capturing differential trends that are unrelated to the event of interest. Therefore, we explore the sensitivity of our inferences to the choice of event window.

Examining a shorter window also allows us to examine the possibility that our results are confounded by the adoption of SFAS 133 (*Accounting for Derivative Instruments and Hedging Activities*). SFAS 133 establishes accounting and reporting standards for derivative instruments and requires an entity to recognize all derivatives as either assets or liabilities on its statement of financial position and to measure derivative instruments at their fair value.[32] The standard became effective for fiscal years beginning after June 15, 2000. Using a three-year event window around 1997 reduces the risk that our results are due to any potential confounding effects from the adoption of SFAS 133.

We tabulate the results of estimating our main tests when using a three-year event window in Table 8. The second-stage results continue to indicate a positive relation between controllable risk and executives' incentives and compensation (note that the unreported first-stage *F*-statistic on the excluded instrument remains well above ten). We conclude that our inferences are robust to the choice of a shorter event window and are unlikely to be an artefact of the adoption of SFAS 133.

6.4. CEO attributes

The introduction of weather derivatives could change the skills that the boards and shareholders of utility firms desire from CEOs, implying that our results could be driven by the turnover and replacement of existing CEOs. We conduct several additional tests to address concerns that our results are attributable to differences in CEO ability and styles driven by turnover. We first exclude 72 firms associated with 90 CEO turnover events that occurred during our sample

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[32] SFAS 133 arguably made the accounting treatment of hedges more complicated, burdensome, and costly to implement. It requires firms to recognize all derivatives as either assets or liabilities on the statement of financial position and measure those instruments at their fair value rather than their notional value. Several studies examine the relevance of SFAS 133 to risk management activities and document mixed evidence. For example, Singh (2004) and Park (2004) find no significant change in earnings volatility following the adoption of SFAS 133, while Zhang (2009) finds that some firms changed their risk management activities following the adoption of SFAS 133.

period. In untabulated results, we find that our inferences are robust to excluding these firms from the sample.

Next, we re-estimate our main specifications after including CEO fixed effects, in addition to firm and year fixed effects. CEO fixed effects absorb time-invariant features of CEO ability and preferences (e.g. risk aversion) and limit our analysis to within-CEO, within-firm variation. Therefore, introducing these fixed effects controls for any changes in the identity of CEOs. We present the results of this analysis in Table 9 and find that our inferences remain unchanged (note that the unreported first-stage *F*-statistic on the excluded instrument remains well above ten). We conclude that our results are not an artefact of CEO turnover or changes in the desired skills of CEOs.

6.5. Alternative measure of weather exposure

Our primary measure of historical weather exposure is based on the sensitivity of firms' revenue to weather. However, it is possible that weather can affect firms' cost structures. For example, extremely cold weather could increase the maintenance and repair costs of gas distribution pipelines. Therefore, we assess the robustness of our results to an alternative measure of firms' exposure to weather risk based on fluctuations in their profitability, which should incorporate both the revenue and cost implications of abnormal weather conditions.[33] To do so, we re-estimate Eq. (3) using quarterly profit as the dependent variable:

$$Profit/Assets_{it} = \beta_{0,i} + \beta_{1,i}\, EDD_{it} + \gamma_i \, ln(Assets_{it}) + \varepsilon_{it} \qquad (5)$$

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[33] We obtain similar results when we define *Treatment* based on cost structure. Specifically, we estimate Eq. (3) using cost as the dependent variable: $Cost/Assets_{it} = \beta_{0,i} + \beta_{1,i}\, EDD_{it} + \gamma_i \, ln(Assets_{it}) + \varepsilon_{it}$, where $Cost/Assets_{it}$ is quarterly COGS scaled by ending total assets. We estimate the equation separately for each firm in our sample using quarterly Compustat data from 1980 to 1997, and we require each firm to have at least 40 quarterly observations. The estimated coefficient β_1 captures the sensitivity of a firm's cost structure to variation in energy demand. Our alternative definition of treatment, *Treatment(Cost)*, is defined as the product of the absolute value of the estimated beta ($|\widehat{\beta_1}|$) and the historical standard deviation of EDD (σ_{EDD}), multiplied by 100. We then re-estimate Eqs. (2a) and (2b) using this alternative definition of firms' exposure to weather risk. Our results are largely unchanged (*t*-statistics on *Pred Beta-FF* range from 1.68 to 3.36).

where *Profit/Assets*$_{it}$ is quarterly income before extraordinary items scaled by ending total assets. We estimate the equation separately for each firm in our sample using quarterly Compustat data from 1980 to 1997, and we require each firm to have at least 40 quarterly observations. The estimated coefficient β_1 captures the sensitivity of a firm's profit to variation in energy demand. Our alternative definition of treatment, *Treatment(Profit)*, is defined as the product of the absolute value of the estimated beta ($|\widehat{\beta_1}|$) and the historical standard deviation of EDD (σ_{EDD}), multiplied by 100. We present the results of re-estimating Eqs. (2a) and (2b) using this alternative definition of weather exposure in Panel A of Table 10. Our results are largely unchanged and the unreported first-stage *F*-statistic on the excluded instrument remains well above ten.

We also examine whether this measure of firms' exposure to weather risk exhibits a violation of the parallel trends assumption. To do so, we replace *After*Treatment* with a series of pre- and post-treatment indicators and report the results of estimating this alternative specification in Panel B of Table 10. None of the pre-treatment indicators is statistically significant at conventional levels, suggesting that the parallel trends assumption is not violated for this alternative measure. Collectively, we conclude that our inferences are robust to using an alternative definition of firms' exposure to weather risk.

7. Conclusion

We examine how the controllability of risk influences the design of executives' incentive-compensation contracts. We find that the CEOs of utility firms with greater historical exposure to weather risk receive relatively less total annual compensation—and that this reduction is attributable to a decline in cash and equity compensation alike—following the introduction of weather derivatives. This finding is consistent with the notion that weather derivatives allow executives to hedge risk that they would otherwise have to bear and, consequently, they receive less of a risk premium in their annual compensation.

We also document a significant decline in these CEOs' equity incentives (i.e., *Portfolio Delta* and *Portfolio Vega*) following the introduction of weather derivatives, which indicates that controllable risk and incentives have a complementary relation. These results provide important empirical evidence about the theoretically ambiguous relation between risk and incentives, and suggests the relation depends on whether risk is controllable (Edman and Gabaix, 2011a; Hemmer, 2006, 2012). Overall, our results show that firms' risk-profiles and hedging opportunities affect the design and structure of CEOs' incentive-compensation contracts.

Finally, it is important to consider how our results might extrapolate beyond our research setting. On one hand, the economic magnitude of the effects that we document might represent a lower bound on the importance of executives' ability to hedge risk because utilities are a relatively stable industry with relatively low inherent volatility. On the other hand, if more risk-averse executives select into the utility industry (e.g., because of its relative stability), then the economic magnitude of the effects that we document might be large relative to the effects that one would expect in other industries. Although it is not obvious how to generalize the economic magnitude of our results beyond our research setting, there is no reason to believe that the *sign* of the relation between risk and incentives that we document is specific to our setting. Instead, the positive relation that we document is consistent with agency-theoretic predictions (e.g., Holmstrom, 1979, 1982; Jenter, 2002; Edman and Gabaix, 2011a; Hemmer, 2006, 2012).

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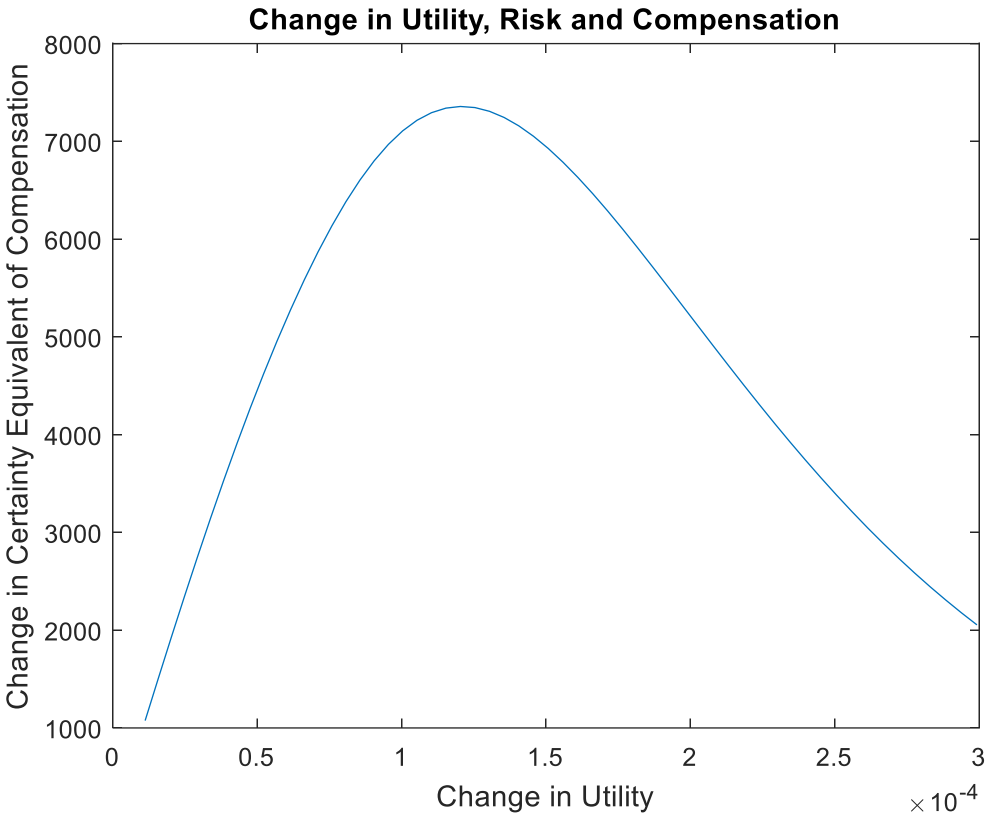
Appendix

Variables Definitions

Variable	**Definition**
WeatherDeriv Use	Dummy equal to one if quarterly and annual reports filed by our sample firms contain any of the following keywords: "Weather Derivative", "Cooling Degree Day", "Heating Degree Day", "CDD" and "HDD." If a firm-year's reports do not contain these hedging keywords, we classify that firm-year as nonuser.
Beta-FF	For each firm-year, we regress daily stock return on Fama-French 3-factor model and daily EDD. EDD is the sum of daily CDD and HDD, which are calculated as Max{0, 65-½*(T_{max}+T_{min})} and Max{0, ½*(T_{max}+T_{min})-65}, respectively. T_{max} and T_{min} are the maximum and minimum daily temperature measured in degrees Fahrenheit, respectively. Beta-FF is the absolute value of the estimated coefficient on EDD.
Risk-FF	Beta-FF multiplied by volatility of EDD.
Beta-FFM	For each year each firm, we regress daily stock return on Carhart 4-factor model and daily EDD. EDD is the sum of daily CDD and HDD, which are calculated as Max{0, 65-½*(T_{max}+T_{min})} and Max{0, ½*(T_{max}+T_{min})-65}, respectively. T_{max} and T_{min} are the maximum and minimum daily temperature measured in degrees Fahrenheit, respectively. Beta-FFM is the absolute value of the estimated coefficient on EDD.
Risk-FFM	Beta-FFM multiplied by volatility of EDD.
Log Ret Vol	Log of stock return volatility.
Log Idio Vol-FFM	Log of idiosyncratic volatility, where idiosyncratic volatility is the volatility of the residuals from a regression of stock return volatility on the Carhart (1997) four factors.
Log Total Comp	Log of total compensation.
Log Cash Comp	Log of salary and bonus.
Log Equity Comp	Log of the value of restricted stock grants plus the value of option grants.
Equity Mix	Value of restricted stock grants plus the value of option grants / total compensation.
Portfolio Vega	Log of the dollar change in wealth associated with a 0.01 change in the standard deviation of the firm's returns. Obtained from Coles et al (2013).
Portfolio Delta	Log of the dollar change in wealth associated with a 1% change in the firm's stock price. Obtained from Coles et at (2013).
Unex/Total	Value of in-the-money unexercised exercisable options divided by the total value of unexercised and exercised options
Log Assets	Log of total assets.
Log Firm Age	Log of firm age, where firm age is the number of years since the firm first appears in CRSP.
Log Stock Return	Log of one plus stock return over the fiscal year.
ROA	Net income plus extraordinary items and discontinued operation scaled by lagged total asset.
Book-to-Market	Book value over market value of equity.
Leverage	Sum of short- and long-term debt minus cash holdings scaled by total assets.
CAPEX	Total capital expenditure scaled by total assets.
After	Dummy equal to one for observations from 1998 onwards and zero otherwise
After(t=-2)	Dummy equal to one if it is two years prior to the introduction of weather derivatives and zero otherwise.
After(t=-1)	Dummy equal to one if it is one year prior to the introduction of weather derivatives and zero otherwise.
After(t=0)	Dummy equal to one if it is the year during which weather derivatives are introduced and zero otherwise.
After(t=1)	Dummy equal to one if it is one year after the introduction of weather derivatives and zero otherwise.
After(t>=2)	Dummy equal to one if it is two or more years after the introduction of weather derivatives and zero otherwise.
Treatment	We use a continuous treatment variable, which is defined as the pre-event sensitivity of stock revenue to weather fluctuations. Following Perez-Gonzalez and Yun (2013), we estimate the following specification: $Rev/Assets_{it} = \beta_{0,i} + \beta_{1,i} EDD_{it} + \gamma_i \ln(Assets_{it}) + \varepsilon_{it},$ where $Rev/Assets_{it}$ is quarterly revenue scaled by total assets. *EDD* is the sum of daily CDD and HDD for each quarter, which are calculated as Max{0, 65-½*(T_{max}+T_{min})} and Max{0, ½*(T_{max}+T_{min})-65}, respectively. $Treatment_{it}$ is

	defined as the product of the absolute value of the estimated beta ($|\widehat{\beta_1}|$) and the historical standard deviation of EDD (σ_{EDD}) during the 1980-1997 estimation period, multiplied by 100.
Treatment(Profit)	We use a continuous treatment variable, which is defined as the pre-event sensitivity of profit to weather fluctuations. We estimate the following specification: $Profit/Assets_{it} = \beta_{0,i} + \beta_{1,i}\, EDD_{it} + \gamma_i\, ln(Assets_{it}) + \varepsilon_{it},$ where $Profit/Assets_{it}$ is quarterly income before extraordinary items scaled by total assets. *EDD* is the sum of daily CDD and HDD for each quarter, which are calculated as $Max\{0, 65-½*(T_{max}+T_{min})\}$ and $Max\{0, ½*(T_{max}+T_{min})-65\}$, respectively. $Treatment_{it}$ is defined as the product of the absolute value of the estimated beta ($|\widehat{\beta_1}|$) and the historical standard deviation of EDD (σ_{EDD}) during the 1980-1997 estimation period, multiplied by 100.

Figure 1



This figure plots the changes in certainty equivalent of compensation against changes in utility given 0.5% decline in return volatility. Our model parameter choice follows Lambert, Larcker and Verrecchia (1991). The CEO is assumed to have power utility with a coefficient of relative risk aversion of two. We assume that the CEO receives 10,000 options with an exercise price equal to the stock price at the time of grant at $50. We assume that the firm's stock price return follows a lognormal process with mean of 10% and we allow return volatility to vary from 30% to 1%. The proportion of CEO's other wealth that is tied to stock price is assumed to be 90%.

Table 1
Descriptive Statistics

The sample period is from 1993 to 2002. All variables are defined in the Appendix.

Panel A: Weather Risk

	N	Mean	Std	Median	25th Pctle	75th Pctle
Beta-FF	899	0.75	0.86	0.49	0.22	0.96
Beta-FFM	899	0.75	0.90	0.48	0.22	0.93
Risk-FF	899	7.37	6.85	5.35	2.57	9.99
Risk-FFM	899	7.34	7.02	5.26	2.39	10.17
Log Ret Vol	899	0.37	0.39	0.24	0.08	0.60
Log Idio Vol-FFM	899	0.27	0.39	0.24	-0.02	0.49
Treatment	899	2.22	2.94	1.01	0.41	2.11

Panel B: CEO Incentive-Compensation Measures

Panel B1: Our Sample

	N	Mean	Std	Median	25th Pctle	75th Pctle
Cash Comp	899	849.28	484.31	738.20	512.21	1033.60
Equity Comp	899	592.60	1187.12	152.21	0.00	604.34
Total Comp	899	1841.82	2318.41	1148.66	712.58	2025.74
Equity Mix	899	0.22	0.22	0.15	0.00	0.37
Portfolio Delta	840	3.44	1.42	3.45	2.46	4.42
Portfolio Vega	868	2.27	1.86	2.61	0.00	3.80

Panel B2: Execucomp Excluding Utilities

	N	Mean	Std	Median	25th Pctle	75th Pctle
Cash Comp	13674	1148.99	1044.63	837.33	514.18	1385.70
Equity Comp	13748	2190.40	4340.58	669.22	72.49	2139.18
Total Comp	13748	4261.02	12162.88	1880.68	946.59	4132.77
Equity Mix	13725	0.39	0.30	0.39	0.08	0.64
Portfolio Delta	12713	5.37	1.56	5.32	4.38	6.33
Portfolio Vega	13396	3.41	1.64	3.56	2.50	4.55

Table 1 Descriptive Statistics, Continued

Panel C: Firm Characteristics

Panel C1: Our Sample

	N	Mean	Std	Median	25th Pctle	75th Pctle
CEO Tenure	899	6.43	3.78	6.00	4.00	8.00
Total Assets	899	7543.32	8859.48	3865.97	1780.81	9688.06
Firm Age	899	48.63	11.50	48.00	44.00	52.00
Stock Return	899	0.10	0.27	0.09	-0.06	0.25
ROA	899	0.03	0.02	0.04	0.03	0.05
Book-to-Market	899	0.67	0.24	0.62	0.53	0.75
Leverage	899	0.37	0.08	0.36	0.32	0.41
CAPEX	899	0.06	0.04	0.05	0.04	0.07

Panel C2: Compustat Excluding Utilities

	N	Mean	Std	Median	25th Pctle	75th Pctle
CEO Tenure	13817	8.32	6.91	7.00	4.00	10.00
Total Assets	98770	1817.53	7249.15	104.33	18.92	548.04
Firm Age	99052	12.85	12.85	8.00	4.00	16.00
Stock Return	68778	0.10	0.67	0.00	-0.30	0.33
ROA	89329	-0.18	0.81	0.01	-0.10	0.07
Book-to-Market	85201	0.55	1.25	0.51	0.24	0.89
Leverage	98254	0.10	0.48	0.09	-0.14	0.33
CAPEX	98770	0.06	0.08	0.03	0.01	0.07

Table 2
Sensitivity of Equity Returns to Weather

This Table presents the results of estimating the regression given by Equation (2a). The sample period is from 1993 to 2002. We use Hornstein and Greene's (2012) method to account for the estimated (rather than observed) dependent variable in column (2) to (5). All variables are defined in the Appendix. Intercepts are included but unreported. *t*-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

		Fama French 3 Factor Model		Carhart 4 Factor Model		Return Volatility	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	WeatherDeriv Use	*Beta-FF*	*Risk-FF*	*Beta-FFM*	*Risk-FFM*	*Log Ret Vol*	*Log Idio Vol-FFM*
After*Treatment	**0.02*****	**-0.04*****	**-0.42*****	**-0.03*****	**-0.41*****	**-0.02*****	**-0.03*****
	(3.06)	**(-5.22)**	**(-5.09)**	**(-5.20)**	**(-5.11)**	**(-4.11)**	**(-4.60)**
Log CEO Tenure	-0.01	-0.03	-0.25	-0.04	-0.31	-0.01	-0.01
	(-0.30)	(-0.80)	(-0.56)	(-0.94)	(-0.71)	(-0.40)	(-0.66)
Log Assets	0.08*	-0.00	0.18	-0.02	-0.08	0.05	0.05
	(1.70)	(-0.05)	(0.24)	(-0.29)	(-0.11)	(1.16)	(0.98)
Firm Age	-0.29**	0.61***	4.81***	0.64***	5.01***	0.13*	0.17*
	(-2.18)	(5.76)	(4.00)	(5.66)	(3.97)	(1.66)	(1.87)
Stock Return	0.03	-0.01	0.14	-0.02	-0.08	-0.19***	-0.21***
	(0.85)	(-0.10)	(0.12)	(-0.18)	(-0.07)	(-4.29)	(-4.29)
ROA	-0.44	-1.48**	-12.70	-1.62**	-14.39*	-1.24***	-1.42***
	(-1.46)	(-2.38)	(-1.61)	(-2.58)	(-1.76)	(-3.34)	(-3.35)
Book-to-Market	0.02	-0.02	-0.33	0.03	0.23	0.23***	0.25***
	(0.45)	(-0.11)	(-0.19)	(0.21)	(0.13)	(2.98)	(2.79)
Leverage	-0.73***	0.07	0.24	0.05	0.20	0.11	0.15
	(-3.12)	(0.21)	(0.06)	(0.15)	(0.05)	(0.55)	(0.72)
CAPEX	0.05	0.60	5.48	0.72	7.09	0.27	0.44
	(0.14)	(1.04)	(0.85)	(1.26)	(1.09)	(0.87)	(1.21)
Observations	899	899	899	899	899	899	899
R-squared	0.49	0.40	0.33	0.40	0.32	0.79	0.75
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3

CEO Compensation

This Table presents the results of estimating the regressions given by Eq. (1). The sample period is from 1993 to 2002. All variables are defined in the Appendix. Intercepts are included but unreported. *t*-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

	(1) *Log Total Comp*	(2) *Log Cash Comp*	(3) *Log Equity Comp*	(4) *Equity Mix*
After*Treatment	**-0.04*****	**-0.02*****	**-0.09****	**-0.01*****
	(-4.13)	**(-3.16)**	**(-2.12)**	**(-4.14)**
Log CEO Tenure	0.05	0.07**	-0.68**	-0.06***
	(0.90)	(2.31)	(-2.37)	(-3.26)
Log Assets	0.08	0.11	-0.31	-0.02
	(0.57)	(1.31)	(-0.49)	(-0.44)
Firm Age	0.33***	0.15**	-0.05	0.04
	(3.81)	(2.44)	(-0.13)	(1.38)
Stock Return	0.28**	0.17**	1.10**	0.03
	(2.13)	(2.56)	(2.27)	(0.80)
ROA	2.11*	1.77**	5.11	0.24
	(1.76)	(2.55)	(1.13)	(0.61)
Book-to-Market	-0.03	-0.20**	0.24	0.01
	(-0.13)	(-2.10)	(0.28)	(0.11)
Leverage	0.11	-0.27	-1.24	0.06
	(0.24)	(-1.13)	(-0.60)	(0.34)
CAPEX	1.15	1.19**	-3.07	-0.15
	(1.39)	(2.36)	(-0.82)	(-0.52)
Observations	899	899	899	899
R-squared	0.77	0.83	0.53	0.49
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes

Table 4
CEO Equity Portfolio Incentives

This Table presents the results of estimating the regressions given by Eq. (1). The sample period is from 1993 to 2002. All variables are defined in the Appendix. Intercepts are included but unreported. *t*-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

	(1) *Portfolio Delta*	(2) *Portfolio Vega*	(3) *Unex/Total*
After*Treatment	**-0.07*****	**-0.07*****	**0.01***
	(-4.30)	**(-3.05)**	**(1.95)**
Log CEO Tenure	0.23**	-0.00	-0.03
	(2.00)	(-0.01)	(-1.23)
Log Assets	0.37**	0.42	-0.03
	(2.20)	(1.12)	(-0.84)
Firm Age	0.97***	0.18	-0.10
	(6.55)	(0.74)	(-1.57)
Stock Return	0.19	-0.22	0.01
	(0.87)	(-1.08)	(0.24)
ROA	-0.60	1.26	-0.18
	(-0.52)	(0.44)	(-0.49)
Book-to-Market	-0.37	-0.67	-0.15**
	(-0.63)	(-1.61)	(-2.03)
Leverage	0.35	-0.30	0.11
	(0.44)	(-0.28)	(0.83)
CAPEX	0.85	1.39	-0.18
	(0.78)	(0.64)	(-0.54)
Observations	840	868	899
R-squared	0.75	0.75	0.83
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Table 5
Fuzzy Difference-in-Differences

This Table presents the second stage results of estimating the fuzzy difference-in-differences regressions given by Eqs. (2a) and (2b).The results of the first stage regressions are reported in Column (1) . *Pred Beta-FF* is the predicted weather beta from the first stage regression. The sample period is from 1993 to 2002. All variables are defined in the Appendix. Intercepts are included but unreported. *t*-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Beta-FF	*Log Total Comp*	*Log Cash Comp*	*Log Equity Comp*	*Equity Mix*	*Portfolio Delta*	*Portfolio Vega*	*Unex/Total*
After*Treatment	**-0.04*****							
	(-5.22)							
Pred Beta-FF		**0.64*****	**0.32*****	**1.47***	**0.21*****	**1.22*****	**1.22****	**-0.13***
		(3.52)	**(2.77)**	**(1.92)**	**(3.48)**	**(3.21)**	**(2.41)**	**(-1.90)**
Log CEO Tenure	-0.03	0.08	0.08**	-0.61*	-0.05**	0.40***	0.03	-0.03
	(-0.80)	(1.20)	(2.39)	(-1.97)	(-2.24)	(3.37)	(0.22)	(-1.33)
Log Assets	-0.00	0.07	0.11	-0.34	-0.02	0.47*	0.33	-0.03
	(-0.05)	(0.36)	(0.98)	(-0.46)	(-0.38)	(1.95)	(0.71)	(-0.76)
Firm Age	0.61***	-0.01	-0.02	-0.82	-0.07	0.44	-0.48	-0.03
	(5.76)	(-0.04)	(-0.15)	(-1.46)	(-1.26)	(1.12)	(-0.92)	(-0.34)
Stock Return	-0.01	0.53***	0.30***	1.68***	0.12**	0.61**	0.15	-0.04
	(-0.10)	(3.02)	(2.87)	(2.95)	(2.05)	(2.18)	(0.51)	(-0.71)
ROA	-1.48**	3.34**	2.39**	7.94	0.64	1.66	4.11	-0.43
	(-2.38)	(1.99)	(2.46)	(1.40)	(1.16)	(0.77)	(1.07)	(-0.92)
Book-to-Market	-0.02	0.05	-0.16	0.42	0.03	-1.22**	-0.51	-0.17*
	(-0.11)	(0.16)	(-1.03)	(0.38)	(0.31)	(-2.21)	(-0.80)	(-1.66)
Leverage	0.07	0.12	-0.26	-1.22	0.06	0.37	-0.13	0.11
	(0.21)	(0.18)	(-0.80)	(-0.48)	(0.24)	(0.29)	(-0.09)	(0.69)
CAPEX	0.60	1.25	1.25*	-2.82	-0.12	1.24	1.62	-0.20
	(1.04)	(1.00)	(1.68)	(-0.62)	(-0.28)	(0.61)	(0.56)	(-0.61)
Observations	899	899	899	899	899	839	868	899
R-squared	0.40	0.49	0.67	0.39	0.03	0.54	0.55	0.80
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 6
Evaluating the Parallel Trends Assumption

This Table presents the results of estimating the difference-in-differences regressions given by the modified Eq. (1) described in Section 6.1. The sample period is from 1993 to 2002. All variables are defined in the Appendix. Intercepts are included but unreported. *t*-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

	(1) *Log Total Comp*	(2) *Log Cash Comp*	(3) *Log Equity Comp*	(4) *Equity Mix*	(5) *Portfolio Delta*	(6) *Portfolio Vega*	(7) *Unex/Total*
*After(t=-2)*Treatment*	-0.00	-0.00	0.06	-0.00	-0.03	-0.01	0.01
	(-0.06)	(-0.57)	(0.71)	(-0.32)	(-1.31)	(-0.55)	(0.68)
After(t=-1) Treatment*	0.00	-0.01	0.02	0.00	-0.03	-0.02	0.01
	(0.21)	(-0.97)	(0.28)	(0.11)	(-1.38)	(-0.77)	(0.85)
After(t=0)*Treatment	**-0.00**	**0.00**	**-0.13**	**-0.01*****	**-0.02**	**-0.06****	**0.01**
	(-0.23)	**(0.31)**	**(-1.43)**	**(-2.81)**	**(-1.25)**	**(-2.09)**	**(1.55)**
After(t=1)*Treatment	**-0.04****	**-0.01**	**-0.06**	**-0.01**	**-0.06*****	**-0.05**	**0.01**
	(-2.03)	**(-1.53)**	**(-0.71)**	**(-1.21)**	**(-2.85)**	**(-1.42)**	**(1.10)**
After(t>=2)*Treatment	**-0.04****	**-0.03****	**-0.12***	**-0.02*****	**-0.10*****	**-0.11*****	**0.01*****
	(-2.46)	**(-2.59)**	**(-1.92)**	**(-4.43)**	**(-4.37)**	**(-3.29)**	**(2.69)**
Log CEO Tenure	0.05	0.07**	-0.69**	-0.07***	0.22*	-0.01	-0.02
	(0.88)	(2.25)	(-2.38)	(-3.37)	(1.94)	(-0.09)	(-1.13)
Log Assets	0.08	0.11	-0.30	-0.02	0.37**	0.43	-0.04
	(0.57)	(1.32)	(-0.47)	(-0.40)	(2.21)	(1.13)	(-0.88)
Firm Age	0.33***	0.15**	-0.01	0.04	0.98***	0.20	-0.11
	(3.81)	(2.46)	(-0.03)	(1.49)	(6.63)	(0.81)	(-1.63)
Stock Return	0.28**	0.18***	1.10**	0.04	0.21	-0.20	0.01
	(2.10)	(2.62)	(2.27)	(0.83)	(0.92)	(-0.96)	(0.20)
ROA	2.12*	1.78**	5.33	0.27	-0.54	1.37	-0.19
	(1.76)	(2.54)	(1.16)	(0.67)	(-0.47)	(0.47)	(-0.49)
Book-to-Market	-0.03	-0.20**	0.22	0.01	-0.37	-0.68	-0.15**
	(-0.14)	(-2.09)	(0.26)	(0.08)	(-0.63)	(-1.62)	(-2.00)
Leverage	0.11	-0.27	-1.22	0.06	0.33	-0.32	0.12
	(0.23)	(-1.13)	(-0.59)	(0.33)	(0.42)	(-0.30)	(0.84)
CAPEX	1.14	1.16**	-3.31	-0.19	0.79	1.24	-0.16
	(1.36)	(2.26)	(-0.89)	(-0.65)	(0.72)	(0.57)	(-0.49)
Observations	899	899	899	899	840	868	899
R-squared	0.77	0.83	0.54	0.49	0.75	0.76	0.83
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 7
Changes in Business Prospects and Policies

This Table presents the second stage results of estimating the fuzzy difference-in-differences regressions given by the modified Eqs. (2a) and (2b) described in Section 6.2. *Pred Beta-FF* is the predicted weather beta from the first stage regression. The sample period is from 1993 to 2002. All variables are defined in the Appendix. Intercepts are included but unreported. t-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

Panel A: Control for Local Business Conditions

	(1) *Log Total Comp*	(2) *Log Cash Comp*	(3) *Log Equity Comp*	(4) *Equity Mix*	(5) *Portfolio Delta*	(6) *Portfolio Vega*
Pred Beta-FF	**0.65*****	**0.28****	**2.06****	**0.21*****	**0.79*****	**0.52**
	(2.83)	**(2.09)**	**(2.23)**	**(2.77)**	**(2.76)**	**(1.02)**
Log CEO Tenure	-0.07	0.03	-1.27***	-0.12***	0.07	-0.32
	(-0.75)	(0.51)	(-2.91)	(-3.55)	(0.59)	(-1.62)
Log Assets	0.10	0.10	-0.02	-0.02	0.57**	0.31
	(0.37)	(0.66)	(-0.03)	(-0.23)	(2.55)	(0.64)
Firm Age	-0.40	-0.14	-3.12**	-0.25**	0.33	-1.22*
	(-1.19)	(-0.70)	(-2.42)	(-2.38)	(1.02)	(-1.78)
Stock Return	0.77***	0.43***	1.87*	0.14	0.36	-0.09
	(2.69)	(2.77)	(1.88)	(1.44)	(1.11)	(-0.21)
ROA	2.82	1.85*	8.04	0.72	0.20	1.68
	(1.43)	(1.79)	(1.08)	(1.08)	(0.09)	(0.42)
Book-to-Market	0.09	-0.04	-0.68	-0.06	-1.73***	-1.00
	(0.16)	(-0.15)	(-0.38)	(-0.37)	(-3.07)	(-1.47)
Leverage	-0.61	-0.41	-5.71	-0.28	-0.10	-1.24
	(-0.65)	(-0.87)	(-1.47)	(-0.80)	(-0.10)	(-1.26)
CAPEX	3.25	2.03	2.02	0.51	2.15	1.47
	(1.58)	(1.61)	(0.28)	(0.79)	(0.85)	(0.37)
Observations	899	899	899	899	840	868
R-squared	0.66	0.80	0.55	0.36	0.84	0.83
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Location-Year Joint FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 7 Cont'd

Panel B: Control for Changes at State of Incorporation

	(1) *Log Total Comp*	(2) *Log Cash Comp*	(3) *Log Equity Comp*	(4) *Equity Mix*	(5) *Portfolio Delta*	(6) *Portfolio Vega*
Pred Beta-FF	**1.02*****	**0.40*****	**2.52***	**0.25****	**0.81****	**1.10***
	(3.57)	**(2.83)**	**(1.91)**	**(2.37)**	**(2.21)**	**(1.75)**
Log CEO Tenure	-0.00	0.04	-1.14***	-0.11***	0.09	-0.20
	(-0.03)	(0.81)	(-2.84)	(-3.56)	(0.87)	(-1.17)
Log Assets	-0.06	-0.00	-0.53	-0.01	0.74***	0.10
	(-0.18)	(-0.02)	(-0.49)	(-0.08)	(4.12)	(0.15)
Firm Age	-0.44	-0.18	-2.70*	-0.19	0.51	-1.02
	(-0.97)	(-0.83)	(-1.81)	(-1.63)	(1.51)	(-1.32)
Stock Return	0.81**	0.47***	1.49	0.08	0.28	-0.03
	(2.53)	(2.83)	(1.42)	(0.88)	(0.91)	(-0.07)
ROA	4.55	2.24	13.37	1.09	0.62	5.82
	(1.49)	(1.48)	(1.39)	(1.29)	(0.27)	(1.07)
Book-to-Market	0.41	0.09	-0.42	-0.01	-1.59***	-0.69
	(0.64)	(0.33)	(-0.22)	(-0.08)	(-2.83)	(-0.77)
Leverage	-0.48	-0.40	-5.25	-0.15	-0.17	-1.58
	(-0.39)	(-0.74)	(-1.28)	(-0.42)	(-0.17)	(-0.99)
CAPEX	2.61	1.82	-0.93	-0.09	0.22	1.00
	(0.90)	(1.29)	(-0.11)	(-0.12)	(0.09)	(0.20)
Observations	899	899	899	899	840	868
R-squared	0.36	0.70	0.45	0.22	0.84	0.74
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Incorporation-Year Joint FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 8
SFAS 133 Adoption and Shorter Event Window

This Table presents the second stage results of estimating the fuzzy difference-in-differences regressions given by the modified Eqs. (2a) and (2b) described in Section 6.3. *Pred Beta-FF* is the predicted weather beta from the first stage regression. The sample period is from 1995 to 2000. All variables are defined in the Appendix. Intercepts are included but unreported. *t*-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

	(1) *Log Total Comp*	(2) *Log Cash Comp*	(3) *Log Equity Comp*	(4) *Equity Mix*	(5) *Portfolio Delta*	(6) *Portfolio Vega*
Pred Beta-FF	**0.68*** **	**0.32** **	**1.53* **	**0.15** **	**0.58** **	**0.67**
	(3.14)	**(2.60)**	**(1.75)**	**(2.40)**	**(1.99)**	**(1.57)**
Log CEO Tenure	-0.07	0.01	-1.10***	-0.09***	0.34***	-0.08
	(-0.75)	(0.14)	(-2.98)	(-3.06)	(2.89)	(-0.41)
Log Assets	-0.11	-0.03	-0.69	-0.03	0.53***	0.12
	(-0.47)	(-0.24)	(-0.86)	(-0.42)	(2.98)	(0.24)
Firm Age	-1.45***	-0.61***	-7.02***	-0.55***	-0.03	-0.68
	(-4.37)	(-3.50)	(-6.78)	(-6.15)	(-0.10)	(-0.91)
Stock Return	0.33*	0.13	0.79	0.04	0.30	-0.17
	(1.73)	(1.15)	(1.41)	(0.83)	(1.35)	(-0.56)
ROA	4.45**	2.50**	10.01	0.71	0.67	4.83
	(2.17)	(2.02)	(1.58)	(1.21)	(0.38)	(1.16)
Book-to-Market	0.20	-0.15	0.59	0.05	-1.11***	-0.13
	(0.70)	(-0.96)	(0.57)	(0.67)	(-3.13)	(-0.29)
Leverage	0.25	-0.27	-3.60	-0.10	-0.97	-2.16
	(0.38)	(-0.65)	(-1.44)	(-0.47)	(-1.05)	(-1.58)
CAPEX	0.31	1.08	-3.66	-0.28	0.59	0.68
	(0.14)	(0.90)	(-0.57)	(-0.48)	(0.29)	(0.19)
Observations	569	569	569	569	531	549
R-squared	0.58	0.74	0.54	0.40	0.82	0.72
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 9
CEO Attributes

This Table presents the second stage results of estimating the fuzzy difference-in-differences regressions given by the modified Eqs. (2a) and (2b) described in Section 6.4. *Pred Beta-FF* is the predicted weather beta from the first stage regression. The sample period is from 1993 to 2002. All variables are defined in the Appendix. Intercepts are included but unreported. *t*-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

	(1) *Log Total Comp*	(2) *Log Cash Comp*	(3) *Log Equity Comp*	(4) *Equity Mix*	(5) *Portfolio Delta*	(6) *Portfolio Vega*
Pred Beta-FF	**0.55*****	**0.27****	**1.81***	**0.15****	**0.64****	**0.85***
	(2.98)	**(2.57)**	**(1.93)**	**(2.16)**	**(2.13)**	**(1.65)**
Log CEO Tenure	-0.03	0.02	-0.58	-0.09**	0.36*	0.04
	(-0.29)	(0.39)	(-1.01)	(-1.99)	(1.83)	(0.15)
Log Assets	0.30	0.26**	0.68	0.06	0.64***	0.76
	(1.61)	(2.51)	(0.88)	(0.98)	(2.97)	(1.64)
Firm Age	0.03	0.15	-1.67	-0.05	0.79***	0.24
	(0.10)	(0.87)	(-0.92)	(-0.43)	(2.66)	(0.80)
Stock Return	0.42**	0.23**	1.67***	0.07	0.35*	-0.08
	(2.45)	(2.32)	(2.74)	(1.31)	(1.68)	(-0.30)
ROA	2.59**	2.12***	4.06	0.26	1.44	1.48
	(2.06)	(3.05)	(0.67)	(0.51)	(1.17)	(0.47)
Book-to-Market	-0.18	-0.21	0.66	0.01	-1.31***	-0.83
	(-0.48)	(-1.22)	(0.46)	(0.06)	(-3.12)	(-1.38)
Leverage	-0.47	-0.60	-3.09	-0.02	0.24	0.38
	(-0.64)	(-1.39)	(-1.00)	(-0.07)	(0.22)	(0.24)
CAPEX	1.56	1.28*	2.12	0.07	1.76	1.31
	(1.32)	(1.82)	(0.43)	(0.17)	(1.23)	(0.56)
Observations	899	899	899	899	840	868
R-squared	0.86	0.89	0.67	0.61	0.83	0.85
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
CEO FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 10
Alternative Measure of Weather Exposure

This Table presents the second stage results of estimating the fuzzy difference-in-differences regressions given by the modified Eqs. (2a) and (2b) described in Section 6.5. *Pred Beta-FF(Profit)* is the predicted weather beta from the first stage regression. The sample period is from 1993 to 2002. All variables are defined in the Appendix. Intercepts are included but unreported. t-statistics are presented below the coefficients in parentheses. ***, **, and * denote statistical significance (two-sided) at the 1%, 5%, and 10% levels, respectively. Standard errors are corrected for heteroscedasticity and are clustered by firm and period (pre-1997/post-1997) level.

Panel A: Difference-in-Differences

	(1) *Log Total Comp*	(2) *Log Cash Comp*	(3) *Log Equity Comp*	(4) *Equity Mix*	(5) *Portfolio Delta*	(6) *Portfolio Vega*
Pred Beta-FF(Profit)	**1.54*****	**0.87*****	**6.51*****	**0.49*****	**2.49*****	**4.54*****
	(3.35)	**(3.39)**	**(3.51)**	**(3.61)**	**(2.72)**	**(5.54)**
Log CEO Tenure	0.13**	0.12***	-0.35	-0.04*	0.37***	0.23**
	(2.40)	(4.12)	(-1.17)	(-1.88)	(3.25)	(2.13)
Log Assets	0.13	0.13	-0.05	-0.01	0.46**	0.52**
	(0.97)	(1.65)	(-0.09)	(-0.15)	(2.41)	(2.20)
Firm Age	-0.60*	-0.38**	-3.93***	-0.27***	-0.53	-2.56***
	(-1.85)	(-2.09)	(-3.11)	(-2.84)	(-0.87)	(-4.29)
Stock Return	0.25*	0.14*	0.88*	0.03	0.10	-0.36
	(1.81)	(1.97)	(1.81)	(0.65)	(0.44)	(-1.53)
ROA	4.23**	2.96***	13.29**	0.87*	3.01	7.88***
	(2.59)	(3.36)	(2.28)	(1.76)	(1.64)	(3.57)
Book-to-Market	-0.09	-0.21**	-0.21	-0.02	-0.44	-0.78**
	(-0.46)	(-2.30)	(-0.24)	(-0.31)	(-0.73)	(-2.42)
Leverage	0.11	-0.25	-1.15	0.07	0.15	-0.41
	(0.23)	(-1.00)	(-0.56)	(0.45)	(0.18)	(-0.53)
CAPEX	0.28	0.76*	-7.32**	-0.43	-0.54	-1.30
	(0.40)	(1.77)	(-2.03)	(-1.54)	(-0.46)	(-0.80)
Observations	899	899	899	899	840	868
R-squared	0.78	0.83	0.54	0.49	0.74	0.76
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 10 (cont'd)
Panel B: Event-Time Difference-in-Differences

	(1) *Log Total Comp*	(2) *Log Cash Comp*	(3) *Log Equity Comp*	(4) *Equity Mix*	(5) *Portfolio Delta*	(6) *Portfolio Vega*
*Afterlaw (t=-2)*Treatment (Profit)*	-0.11	-0.04	-0.19	-0.04	-0.12	-0.05
	(-1.36)	(-0.96)	(-0.46)	(-1.28)	(-1.27)	(-0.44)
*Afterlaw (t=-1)*Treatment (Profit)*	-0.09	-0.06	-0.39	-0.03	-0.35	-0.14
	(-0.95)	(-1.05)	(-0.80)	(-0.70)	(-1.60)	(-1.22)
Afterlaw (t=0)*Treatment (Profit)	**-0.13**	**-0.02**	**-1.45*****	**-0.12*****	**-0.22***	**-0.47****
	(-1.52)	**(-0.28)**	**(-2.78)**	**(-3.47)**	**(-1.77)**	**(-2.57)**
Afterlaw (t=1)*Treatment (Profit)	**-0.36*****	**-0.11****	**-1.11****	**-0.08***	**-0.44*****	**-0.44***
	(-3.42)	**(-2.11)**	**(-2.10)**	**(-1.86)**	**(-3.08)**	**(-1.90)**
Afterlaw (t>=2)*Treatment (Profit)	**-0.34*****	**-0.17*****	**-1.15*****	**-0.14*****	**-0.70*****	**-0.62*****
	(-3.37)	**(-3.48)**	**(-3.06)**	**(-5.02)**	**(-5.09)**	**(-2.71)**
Log CEO Tenure	0.04	0.06**	-0.71**	-0.07***	0.35***	-0.01
	(0.81)	(2.16)	(-2.46)	(-3.51)	(3.86)	(-0.10)
Log Assets	0.07	0.11	-0.33	-0.02	0.48***	0.40
	(0.49)	(1.24)	(-0.52)	(-0.51)	(3.18)	(1.06)
Log Firm Age	0.32***	0.14**	-0.03	0.03	1.05***	0.16
	(3.72)	(2.26)	(-0.09)	(1.29)	(7.80)	(0.65)
Log Stock Return	0.29**	0.18***	1.12**	0.04	0.28**	-0.17
	(2.17)	(2.67)	(2.31)	(0.94)	(1.98)	(-0.85)
ROA	2.11*	1.79**	5.21	0.27	-0.56	1.37
	(1.75)	(2.54)	(1.14)	(0.68)	(-0.53)	(0.47)
Book-to-Market	-0.00	-0.19*	0.29	0.01	-1.30***	-0.62
	(-0.02)	(-1.95)	(0.34)	(0.23)	(-4.79)	(-1.49)
Leverage	0.10	-0.28	-1.26	0.05	0.27	-0.34
	(0.21)	(-1.16)	(-0.60)	(0.30)	(0.44)	(-0.31)
CAPEX	1.15	1.17**	-3.18	-0.19	0.74	1.37
	(1.37)	(2.27)	(-0.86)	(-0.63)	(0.77)	(0.63)
Observations	899	899	899	899	840	868
R-squared	0.77	0.83	0.54	0.49	0.87	0.76
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes