Are the Risk Attitudes of Professional Investors Affected By Personal Catastrophic Experiences?

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Abstract

We adopt a novel empirical approach to show that the risk attitudes of professional investors are affected by their catastrophic experiences – even for catastrophes without any meaningful economic impact on these investors or their portfolio firms. We study the portfolio risk of U.S.-based mutual funds that invest outside the U.S. before and after fund managers personally experience severe natural disasters. Using a differences-in-differences approach, we compare managers in disaster versus non-disaster counties matched on prior disaster probability and fund characteristics. We find that monthly fund return volatility decreases by roughly 60 basis points in year +1 and the effect disappears by year +3. Systematic risk drives the results. Additional analyses do not support wealth effects (using disasters with no property damage) or managerial agency, skill, and catering explanations.

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1. Introduction

Extreme negative experiences can affect investor risk taking for a variety of reasons. Standard utility theory allows such events to affect risk taking due to their effect on unavoidable or background risk (Heaton and Lucas (2000) and Guiso and Paiella (2008)) or their perceived likelihood and salience (Caballero and Krishnamurthy (2009) and Bordalo, Gennaioli, and Shleifer (2013)). Alternatively, models based on habit persistence (Campbell and Cochrane (1999)) or prospect theory (Barberis, Huang, and Santos (2001)) posit that large negative shocks affect risk tolerance. Additionally, the psychology literature suggests that traumatic events can engender more conservative behavior, even for events that have no direct economic consequences (Loewenstein (2000) and Loewenstein, Hsee, Weber, and Welch (2001)).

An increasing number of studies in economics and finance show that experiencing extreme negative events does indeed affect financial decisions and risk taking through changes in preferences or beliefs.[2] There is also a significant body of evidence showing that plausibly non-economic shocks

[2] For example, Guiso, Sapienza, and Zingales (2018) conclude that negative emotions associated with the recent financial crisis reduce financial risk taking by individuals. A developing literature provides causal evidence of consequences that include less financial risk taking among unsophisticated individuals (Gallagher (2014) and Bucciol and Zarri (2015)) and negative psychological outcomes in the general population (Luechinger and Raschky (2009) and Rehdanz, Welsch, Narita, and Okubo (2015)). Other studies find that exposure to extremely negative economic or natural events have long lasting effects on financial risk taking by households and firms (Malmendier and Nagel (2011), Bernile, Bhagwat, and Rau (2017), and Bharath and Cho (2019)).

do affect securities prices.[3] However, it is not obvious that prior results should apply to professionals who work with financial risk for a living.[4] It is this question that we aim to address. The answer matters greatly for the millions of investors who delegate their money management decisions to professionals, since managerial risk attitudes are known to influence investors' choice of managers, explicitly or implicitly, e.g., through style selection.[5] If managers take more or less risk than investors anticipate, especially risk of the systematic variety that investors cannot diversify away, then this mismatch of risk will leave investors worse off.

To answer our research question, we examine whether natural disasters that occur in the United States affect the risk taking of mutual fund managers who are based in the U.S. but invest exclusively outside the U.S. by mandate. Our null hypothesis is that mutual fund managers acting in their investors' interests should not change their portfolio risk after they experience a disaster that does not affect their portfolio firms or their investors. By contrast, if a disaster causes an emotional response by fund managers who personally experience it, then managerial risk taking should change even absent other effects suggested by neoclassical economic theory.

Our experimental design has numerous advantages. First, mutual fund managers are an ideal group of economic agents to study because it is part of their job description to make optimal

[3] For example, see Hirshleifer and Shumway (2003) for the amount of sunshine, Kamstra, Kramer, and Levi (2003) for daylight exposure, Edmans, Garcia, and Norli (2007) for sports games, and Goetzmann, Kim, Kumar, and Wang (2011) and deHaan, Madsen, and Piotroski (2017) for normal fluctuations in the weather.

[4] A few contemporaneous studies use divorce, bereavement, personal wealth, and religious beliefs as shocks to money managers. However, these shocks do have an economic impact on managers and are difficult to interpret as exogenous to managerial behavior, which is not the case in our study given the specific way we use natural disasters. Additionally, the outcome of interest in these studies is generally the returns generated by managers rather than their risk taking.

[5] For instance, see Kumar (2009), Dorn and Huberman (2010), Bailey, Kumar, and Ng (2011), and Barber, Huang, and Odean (2016).

risk-return tradeoffs, including with respect to extreme events. They are professional financial risk takers, unlike individuals in the population at large or even corporate executives, who are not necessarily trained and experienced specifically in financial risk taking. Furthermore, we are able to directly measure risk taking by mutual fund managers (ex post) as the realized volatility of their portfolio returns. This contrasts with other coarser proxies used in the literature, such as the allocation of a household's assets to cash versus debt versus equity, risk attitudes inferred from survey responses, or the volatility of a firm's stock returns, which may be affected by many factors besides individuals' or managers' attitudes toward risk.

Another important advantage of our approach is that we focus on U.S.-based managers of international equity funds (as distinct from global equity funds). Hence, while the managers themselves experience the disasters in our sample, the stocks in a manager's portfolio should not be affected because the portfolio firms are not located in the U.S. (as we carefully verify).[6] Therefore, changes in the volatility of fund portfolios around disaster events must result from active trading by fund managers rather than the performance of their portfolio firms.

Finally, natural disasters provide a unique opportunity to examine the effect of extreme negative exogenous shocks on risk attitudes. While we focus on disasters with the most property damage and/or fatalities, we also show that even the single worst disaster in our sample in terms of damages amounts to a relatively small 0.5% of wealth in the affected county. Furthermore, we show

[6] While unlikely, it is possible that some portfolio firms are affected by disasters if these international firms have above average operations specifically in the county of the fund, or they may be affected if there are above average spillovers from local firms in the county to these international firms. The reason for which these direct or indirect effects would have to be greater than average is that we compare funds in counties affected by disasters to funds in counties that are not – but otherwise these funds are similar. Moreover, we can test for these effects by comparing disasters without any meaningful economic impact to disasters with some economic impact. Our results indicate that these effects are not significant.

that disasters do not affect fund flows during the years thereafter. From the perspective of relatively wealthy mutual fund managers and investors, our sample disasters can therefore be reasonably characterized as non-economic shocks that may have pronounced psychological effects. Moreover, natural disasters are exogenous to risk attitudes.[7] Importantly, since we carefully match our treatment and control funds on the prior probability of a disaster given the fund's location, whether a fund experiences a disaster or not is conditionally random by construction.

In our empirical analysis, we merge a dataset of all natural disasters that occur in the U.S. with a dataset of all international equity mutual funds (excluding index funds) that are based in the U.S. The treatment sample corresponds to mutual funds located in counties (practically, cities) that experience severe natural disasters (top 0.1% of county-months). It comprises roughly 500 fund-years corresponding to about 300 unique funds spanning 33 years. Each "treatment" fund is matched to a "control" fund that does not experience a disaster in the same year.

Using a difference-in-differences setting, we test whether treatment funds change their portfolio risk around disaster events relative to changes in the portfolio risk of control funds. Our approach allows us to hold fixed unobserved heterogeneity in risk taking across funds and time while controlling for differences in time-varying characteristics across treatment funds as well as between treatment funds and control funds. In this setting, changes in risk taking between treatment and control funds around disaster events should be caused by these events.

We find that there is a significant albeit temporary decrease in portfolio volatility after a fund manager experiences a disaster. Compared to the year before the event, on average, the difference in portfolio volatility of the treatment and control funds is roughly 60 basis points lower in

[7] For instance, see the classic study of Wright (1928) as well as Roll (1992) and Hirshleifer and Shumway (2003).

year +1 and 30 bps lower in year +2. These differences-in-differences are economically and statistically significant, corresponding approximately to a 10% and 5% decrease in volatility, respectively. By year +3, the effect is no longer economically or statistically significant.

We also find that the changes in portfolio risk are similar across disasters partitioned by whether they result in high fatalities (and low property damage) or high property damage (and low fatalities). This indicates that the wealth effects of disasters, whether for managers or investors, cannot by themselves explain our baseline results.[8] When we decompose total portfolio risk using the global four-factor model, we find that the decrease in systematic risk accounts for most of the decrease in total risk. In the spirit of Loewenstein (2000), one interpretation of our results is that experiencing a disaster induces an emotional response in fund managers that leads them to make to more conservative investment decisions.

One alternative explanation is that the decrease in risk taking resulting from disasters reflects a decrease in manager-investor agency problems. In particular, since management fees depend on assets under management, managers have an incentive to increase fund size to maximize fee revenue. Moreover, if better fund performance leads to greater fund inflows,[9] managers may take more risk to improve raw performance and thereby attract inflows. In our setting, disasters in the U.S. could lower the relative performance of domestic funds located in the same county as international funds – for example, because the disasters affect the U.S. stocks held by domestic funds. If investors substitute between domestic and international funds located in the same county, managers of international funds that experience a disaster may take less risk because they have less

[8] This evidence also supports our assumption of a similar level of spillovers across domestic and foreign product markets and the portfolio firms of treatment funds compared to control funds.

[9] See Ippolito (1992), Chevalier and Ellison (1997), and Sirri and Tufano (1998), but also see Spiegel and Zhang (2013).

incentive to attract flows with higher performance. The central prediction of this agency explanation is that international funds that experience a disaster should subsequently receive greater inflows. However, comparing treatment and control funds, we find no significant effect of disasters on the difference in fund flows.

A second alternative explanation is that the decrease in risk taking resulting from disasters reflects an increase in managerial skill. In particular, experiencing a disaster may improve managers' evaluation of extreme events (e.g., tail risks) on the universe of stocks in which they invest, even if their portfolio firms are not affected by the disaster. This information advantage may improve managerial stock selection or timing abilities and thereby generate superior risk adjusted performance (Huang, Sialm, and Zhang (2011)). The central prediction of this skill explanation is that funds that experience a disaster should subsequently perform better. While we do find a decrease in portfolio returns after a fund manager experiences a disaster, the results are not statistically significant for returns, whether on a raw or risk adjusted basis.[10] We likewise find no effect on fund survival. While our returns results do not support the skills explanation, the finding of a decrease in raw returns but not risk adjusted returns (alpha) is consistent with a decrease in systematic risk.

A third alternative explanation is that the decrease in risk taking resulting from disasters reflects managerial catering to local investor demand for less risk taking. This catering explanation assumes that fund investors strongly cluster within the county of the fund itself. We examine this possibility empirically using the best proxies in our data for clustering of investors within the fund's county: fund size and fund investor type. We find no difference in risk taking between the smallest

[10] The standard statistical tests that we use have more power to detect changes in the second moment of stock returns than the first (in light of their relatively lower noisiness as a practical matter). This would explain why our volatility and returns results are consistent in economic terms, even though the former is statistically significant while the latter is not.

funds or institutional funds, with investors plausibly concentrated within the county, and the largest funds or retail funds, with their investors spread across the country. The evidence suggests that it is not investors' risk attitudes that are affected by disasters.

Additional evidence is also consistent with a psychological interpretation of our results. Specifically, we find that the effect of disasters on risk taking is greater for managers with few prior disaster experiences. We also find that the effect of disasters is similar in magnitude over time, both over the course of our sample period as well as for funds that are younger versus older. Overall, our main results show that natural disaster experiences lead mutual fund managers to temporarily lower the risk of their portfolios, and further results suggest that managers' behavior may be psychological in nature.[11] These changes in managerial risk taking may lead to a mismatch between the quantity of risk anticipated by fund investors and the quantity delivered by fund managers. To the extent that investors choose funds with specific risk objectives in mind, they will be left worse off because of managers' behavior.

Our study makes several important contributions to the literature. First, prior studies find that the weather affects risk taking by investors (Saunders (1993), Hirshleifer and Shumway (2003), and Bassi, Colacito, and Fulghieri (2013)) and that natural disasters affect the risk taking of households (Gallagher (2014) and Hanaoka, Shigeoka, and Watanabe (2018)) and firms (Dessaint and Matray (2017)). Our study shows that natural phenomena even affect the risk attitude of money

[11] Like most of the literature, we cannot unambiguously separate preferences from beliefs as the mechanism underlying our results. However, two facets of our results suggest preferences as the mechanism rather than beliefs. First, the effect of disasters on risk taking is temporary, and second the effect diminishes with prior experience with disasters. Money managers are well resourced professional risk takers, and there is reasonably good data on natural disasters that is readily available to managers if they wish to incorporate the effects of disasters in their estimates of risk. It would therefore be unusual if the occurrence of a disaster were to surprise managers into temporarily changing their beliefs about disasters. On the other hand, it would be consistent with managers having time-varying preferences.

managers, one group of the most prominent – and presumably rational – professional risk takers in financial markets.

Second, we contribute to the emerging literature on the forces that affect risk taking by mutual fund managers. Job related factors that affect the risk taking of investment professionals include agency problems and managerial skill (Huang, Sialm, and Zhang (2011), employment risk (Kempf, Ruenzi, and Thiele (2009)), and managerial tournaments (Brown, Harlow, and Starks (1996), Busse (2001), and Kempf and Ruenzi (2008)). Moreover, managers' investment decisions are also affected by their personal lives, including by wealth shocks (Chuprinin and Sosyura (2018) and Pool, Stoffman, Yonker, and Zhang (2019)), religious beliefs (Shu, Sulaeman, and Yeung (2012)), and even divorce (Lu, Ray, and Teo (2016)) and bereavement (Liu, Shu, Sulaeman, and Yeung (2019)), not to mention their personal preferences (Bodnaruk and Simonov (2016)). Our study shows that negative personal shocks affect risk taking by professional investors even when they have no direct economic consequences.

Finally, we contribute to the nascent literature on international mutual funds. Local economic shocks can be spread globally via financial markets (Jotikasthira, Lundblad, and Ramadorai (2012) and Ferreira, Massa, and Matos (2018)). Our study shows that even non-economic domestic shocks (from the viewpoint of mutual fund managers, investors, and portfolio firms) can have a spillover effect on risk taking in foreign financial markets. This is important because U.S.-based international equity funds account for approximately $2 trillion of assets as of 2015, or roughly a third of the assets of domestic equity funds (see the Investment Company Institute's Fact Books).

However, they receive little scholarly attention compared with U.S. equity mutual funds in spite of their growing importance in investors' portfolios.[12]

The rest of this paper is organized as follows. Section 2 presents the sample, data, and methodology. Section 3 presents the main results, and Section 4 presents alternative explanations. Section 5 presents cross-sectional contrasts and robustness tests. Section 6 concludes.

2. Sample, Data, and Methodology

2.1. Selection of Disasters

We obtain data on natural disasters from the Spatial Hazard Events and Losses Database for the United States (SHELDUS). SHELDUS contains data for 18 different types of events: avalanche, coastal, drought, earthquake, flooding, fog, hail, heat, hurricane/tropical storm, landslide, lightning, severe storm/thunderstorm, tornado, tsunami/seiche, volcano, wildfire, wind, and winter weather. Our data include the number of fatalities and the amount of property damage for each county-month with an event. This is important because it allows us to distinguish between natural disasters involving fatalities alone (and hence with no effect on local wealth) and those involving property damage alone (and thus with some effect on local wealth). (To illustrate fatalities without damages, heat waves can cause fatal strokes, snowstorms can result in freezing to death, and flooding can lead to drowning in the course of motor vehicle accidents or recreational activities – without necessarily doing a meaningful amount of property damage in the county as a whole.) While our data on natural disasters are at the county level, mutual fund managers are located in medium or large size cities,

[12] Notable exceptions include Cumby and Glen (1990), Didier, Rigobon, and Schmukler (2011), Busse, Goyal, and Wahal (2014), and Cremers, Ferreira, Matos, and Starks (2016).

and these cities typically coincide with one county (which is sometimes larger than the city, e.g., Los Angeles County, and sometimes smaller, e.g., the five borough-counties of New York City).

We focus on the most severe events in SHELDUS so that they can plausibly affect local risk taking. For this reason, we set thresholds for what constitutes a natural disaster in our setting based on fatalities and/or damages. Since there is variation in population and income across counties and over time, we scale fatalities and damages by population and income.[13] We obtain county-year level data for this purpose from the Bureau of Economic Analysis.

A disaster in our sample must have a minimum of 8 fatalities per million people and/or a minimum of $2,000 of property damage per million dollars of income. These thresholds are chosen to correspond to roughly the top 0.1% of their respective distribution of events at the county-month level. It turns out that there is little overlap between disasters driven by fatalities as opposed to damages. The main types of disasters in our sample are as follows (using the labels in SHELDUS): flooding (20%), wind (19%), severe storm/thunderstorm (15%), winter weather (12%), tornado (10%), hail (7%), lightning (5%), and hurricane/tropical storm (4%). The remaining disaster types amount to only 8% of our sample, with no type accounting for more than 3% of the total. By definition, our disasters are the most severe events in SHELDUS.

We verify that the thresholds we use to define natural disasters produce the same rate of disasters across counties irrespective of their population or income. For comparison purposes, we create four groups of counties: counties in which a mutual fund is located in any year (less than 100 counties); counties in which a mutual fund is located in a particular year; counties with both population and income per capita of at least the 1st percentile of counties in which a mutual fund is

[13] We use income rather than wealth throughout the paper because data on wealth are not available at the county-year level.

located in any year (about 1,300 counties); and all counties (roughly 3,100 counties). For all four groups of counties, our thresholds for fatalities and damages each produce a similar and roughly 0.1% rate of disasters at the county-month level. Consequently, our definition of disasters is not specific to the sample of counties with mutual funds that we study further below.

[Insert Figure 1 about here]

In Figure 1, we graph descriptive statistics for natural disasters. We focus on counties in which mutual funds are located. While our analysis is at the county-month level, we graph statistics at the year level for ease of interpretation. At this stage, we separate disasters driven by fatalities as opposed to damages.

Panel A shows that the rate of disasters as a group does not clearly trend over time. Roughly 0.1% of county-months, on average, have a disaster driven by fatalities, which is the same proportion as county-months with damages driven disasters. Panel B similarly shows that the losses caused by disasters as a group do not clearly trend over time. Given the heterogeneity of both population and income across counties and time, we scale fatalities in a county-month by the population of the county, and similarly we scale damages by the income of the county. During our sample period, the average county-month has experienced roughly 11 fatalities per million people and $12 of damages per $1,000 of income.

It is worth digging deeper into the wealth effects of our disasters. We focus here on disasters driven by damages rather than fatalities. While fatalities obviously have negative wealth effects on the people involved and their households, they have no wealth effects on the population as a whole or an arbitrary member thereof. Furthermore, Figure 1 Panel B shows that the average conditional loss due to damages is a mere 1.2% of income. Looking more closely at the distribution of conditional losses (not tabulated), the 90th and 95th percentiles are 0.30% and 0.57%, respectively.

The 99th percentile of the distribution is 1.92%, and the maximum is still only 2.07%. Assuming a ratio of wealth to income of four times (the ratio of measurable U.S. wealth to U.S. GDP), even the maximum conditional loss is a relatively small 0.5% of wealth in the county.

Overall, even for most extreme disasters driven by damages, wealth effects are likely to be relatively small. This is especially the case for mutual fund managers and investors, groups of people that are closer to the top of the wealth distribution than the middle let alone the bottom, even after a disaster, particularly once insurance is taken into account. Additionally, whatever the implications of the foregoing analysis may be for fund investors, we are able to specifically examine the effect of disasters on fund flows. To preview our results (Figure 3 and Table 6), we do not find that disasters affect fund flows during the years thereafter. The results of these analyses as a whole indicate that, as far as the direct economic effects of disasters on managers and investors are concerned, even disasters driven by damages can be reasonably characterized as non-economic shocks.

2.2. Selection of Mutual Funds

We merge our dataset of natural disasters in the U.S. with a dataset of all international equity mutual funds based in the U.S. Since we conduct our empirical analysis at the fund-year level, we collapse our disasters to the county-year level. Counties with a disaster in one or more months in a given year are deemed to have a disaster that year, whereas counties without a disaster in any month are deemed to have no disaster that year. In the vast majority of county-years with a disaster, there is exactly one month with a disaster. If a county-year has more than one month with a disaster, we use the first month with a disaster.

We obtain most of our data on mutual funds from the CRSP Mutual Fund database. We select international equity funds (which invest only in non-U.S. stocks) and not global equity funds (which invest in both U.S. and non-U.S. stocks). This ensures that our sample portfolio managers are based in the U.S., but since they invest outside the U.S., the stocks in their portfolio cannot be affected by disasters experienced by the managers themselves. To ensure that managers have the ability to change the risk taking of their funds, we only include active funds in our sample and exclude passive funds, namely, those that are indicated by CRSP to be index funds or exchange traded funds. (Other than making small changes to their tracking error, funds that track an index make it impossible for managers to actively trade. By extension, managers of such funds cannot choose the risk level of the fund.) Similarly, we only include in our sample diversified funds, and we exclude funds that focus on specific industries rather than the stock market as a whole. (Funds specific to an industry are a relatively small proportion of the total, but they are difficult to combine with diversified funds because the usual international factor returns do not necessarily apply to such funds.)

After applying these basic filters, there are almost 11,800 fund-years comprising nearly 1,500 unique funds that can possibly experience a natural disaster the following year. Our data comprehensively cover the universe of international equity funds based in the U.S. In 2013, for example, they cover three-quarters of the universe by assets and three-fifths of the universe by number of funds.[14] We obtain data on the location of mutual fund managers from Morningstar. These data identify the location of the portfolio manager of each fund. For the sake of simplicity, we continue to refer to this as the location of the fund (rather than the location of the portfolio manager more precisely). Additionally, we do not distinguish between sole managed and team

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[14] For the initial sample of funds, from which our final sample of treatment and control funds are taken, total assets equal $1.5 trillion in 2013, the last year in our sample, and the number of funds is 824. By comparison, according to the Investment Company Institute's Fact Books, there was $2.0 trillion invested in global equity mutual funds in the same year (which includes both international only funds and domestic plus international funds) spread across 1,345 funds. Of all equity fund assets, 26% was invested in global funds and the rest in domestic funds.

managed funds because unless managers recognize the link between the disaster they experience and their risk taking reaction, we do not have any prediction about whether they fail to do so differently as individuals versus as part of a group.

A natural disaster the following year is experienced by approximately 5% of fund-years between 1981 and 2013. Focusing on international equity mutual funds that experience a disaster, our sample of "treatment" funds comprises 483 fund-years. This corresponds to 330 unique funds and spans the years 1981-2013. At the fund-year level, roughly 66% of disasters are driven in part by fatalities, and 36% are driven in part by damages, with less than 2% of disasters being driven by both fatalities and damages. Our treatment funds are located in 82 unique county-years and 39 unique counties. Our sample disasters are not clustered in certain counties as 41% of our counties experience exactly one disaster, 33% experience two disasters, and 95% experience four or fewer disasters.

To verify that our funds are indeed international funds and do not invest in the U.S., we obtain data on their holdings from the Thomson Mutual Funds database. We find that 80.4% of our treatment funds have domestic assets worth less than 1% of total (domestic plus foreign) assets. Only 14.0% of our treatment funds have domestic assets worth more than 5% of total assets. On average, our funds have only 5.5% of their assets invested in U.S. stocks.[15]

Although our sample disasters are spread out across many county-years, we nevertheless take a difference-in-differences approach and compare otherwise similar funds that experience a disaster to funds that do not, both before and after the disaster. Specifically, we use propensity

[15] In our robustness tests, we find similar results for funds that have more than a trivial proportion of their assets invested in domestic stocks.

score matching to match each of our treatment fund-years to an equal number of "control" fund-years.

To estimate propensity scores, we use the fund's total net assets, turnover ratio, raw returns, flows, and volatility. We also include as a covariate the prior probability of a disaster in the county, measured from 1970 (the beginning of our disasters data) until the current year (e.g., over 25 years for a fund in 1995). We include this covariate to account for any differences between treatment and control funds based on the local incidence of disasters. Additionally, we match treatments and controls based on the target region of the world in which they invest. We do so to account for any differences in risk exposure around the world. Since some target regions are very thick with funds while others are very thin, we organize target regions into three groups: "general", "developed markets", and "emerging markets".[16] All covariates are measured during the year before the disaster. We match treatment funds to control funds first on year and target region and then on the closest propensity score.

[Insert Table 1 about here]

Table 1 shows that our treatment and control funds are well matched. The individual covariates are generally well balanced for our treatment and control funds, whether we examine the mean or the median of the two distributions. The one exception is size, with assets being greater for treatment funds in both the mean (at the 10% level) and the median (at the 5% level). Additionally, flows are also lower for treatment funds in the mean (at the 5% level), though they are not significantly different in the median. The propensity scores – the ultimate arbiters of covariate

[16] The "general" group (73% of our treatment fund-years) comprises funds that invest in stocks around the world without a focus on either developed or emerging markets. The "developed markets" group (6% of fund-years) comprises "Canada", " "European", and "Japan" funds. The "emerging markets" group (22% of fund-years) comprises "Emerging markets", "China", "India", "Latin America", "Pacific", and "Pacific ex Japan" funds.

balance – are not significantly different at conventional levels.[17] On the whole, the results indicate good matching between treatment and control funds, but to be conservative, we include our matching covariates as control variables in our regressions.

It is a feature of our sample that we have very few repeated observations on the same fund. In particular, of our 330 unique treatment funds, 72% appear only once, and 81% of our 291 control funds appear only once. Even pooling both groups together and thus allowing switching between the treatment and control groups, we only have a single observation for 64% of our funds. Of our treatment and control funds, 95% and 100%, respectively, appear three times or less. This has a number of implications. First, our results are not driven by many observations on just a few funds. For this reason, we do not include fund fixed effects because unobserved fund heterogeneity will be removed by the after versus before difference in our difference-in-differences setting. Additionally, less than 8% of treatment funds become control funds within three years of a disaster, and the converse happens less than 11% of the time. Therefore, our inferences are not influenced by overlapping treatment and control observations within the same event window.

2.3. Methodology

Our difference-in-differences setting is ideally suited for examining and testing the effect of natural disasters on risk taking by mutual fund managers. At this point, our sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. We begin our main empirical analysis with graphical evidence of the evolution of risk taking around disasters. We consider the three years before a disaster (denoted years -3 through -1) and

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[17] We also examine the proportion of assets invested in domestic stocks for our treatment and control funds, and we find that they are similar.

the three years after a disaster (denoted years +1 through +3). Each treatment and control fund can appear for up to three years before and after the disaster, subject to data availability.

To test the change in risk taking, we then take a regression approach. Our regression specifications have several common features. In our main specifications, the unit of observation is the fund-event year, as before. We pool all the event year observations together for treatment and control funds, and we run regressions that compare, in terms of risk taking, the treatment funds during one of the years after the disaster (e.g., year +1) to themselves during the year before the disaster (e.g., year -1) as well as the same event years for the corresponding control funds. To this end, we include three dummy variables: one for the treatment funds, another for the post-disaster period, and a third for the interaction of the first two. The coefficient on this interaction dummy variable captures our outcome of interest: the change in risk taking for treatment funds after they experience a disaster.

While our propensity score matching ensures that, on average, our treatment funds are well matched to our control funds, we still want to ensure that we account for any differences between individual pairs of treatment and control funds. To this end, we include as control variables in our regressions the covariates from our matching. These covariates are the fund's total net assets, turnover ratio, raw returns, flows, and volatility. The first three of these variables are measured in natural logarithms. We include year fixed effects to remove unobserved heterogeneity across time that is common to both treatment and control funds. We cluster standard errors by county-year. Finally, we winsorize variables whenever appropriate at the 1st and 99th percentiles.

2.4. Descriptive Statistics

[Insert Table 2 about here]

In Table 2, we present descriptive statistics for our sample of fund-years. Variables are defined in Appendix Table 1. We start with the independent variables. Our sample funds are relatively large, with assets of over $700 million, on average (median assets of around $125 million). They turn over roughly three-quarters of their assets (median of one half).

Moving on to the dependent variables, we start with risk taking, measured as the volatility of monthly raw returns during each event year. The volatility that we can estimate for the sample as a whole is about 5.0% per month on average (median of 4.7%). This is comparable to the volatility of the stock market as a whole. For the restricted sample upon which we impose restrictions needed to estimate the global four-factor model, volatility is a bit higher, at 5.2% per month on average (median of 4.8%), but it is still comparable. For future reference, it is worth noting that the total volatility of our sample funds is mostly systematic rather than idiosyncratic. This is in line with our expectations for mutual funds, which are well diversified portfolios of stocks that are mainly exposed to various sources of systematic risk. Overall, ours is a representative sample of international equity mutual funds.

Finally, the growth rate of total net assets is approximately 2.0% per month higher, on average, than the raw returns of our funds (median of 0.3%). Mean raw returns are roughly 0.4% per month (median of 0.6%), in line with the performance of international equities. Our sample funds tend to underperform relative to the global four-factor model, with mean (median) alphas of -0.37%

(-0.31%) per month based on the global four-factor model. The mean (median) Sharpe ratio based on monthly returns is about 0.13 (0.14).

3. Main Results

3.1. Risk Taking

We now examine whether risk taking changes as a result of natural disasters experienced by mutual fund managers. We begin by graphing the volatility of monthly raw returns starting three years before a disaster and ending three year thereafter. For each event year, we graph the difference in volatility between treatment and control funds.

[Insert Figure 2 about here]

Figure 2 shows the results. During the three years before managers experience a disaster, volatility trends roughly in parallel for treatment and control funds, and the difference between the two groups is not statistically significant. This validates our difference-in-differences approach. In year +1, volatility falls by an economically significant 60 basis points or so for treatment funds compared to control funds. Thereafter, volatility rises each year, until it returns to its pre-disaster level in year +3.[18] In summary, the disaster experiences of fund managers result in a significant but temporary decrease in risk taking.

Next, we formally test whether risk taking decreases as a result of a disaster. To this end, we run regressions of the volatility of monthly raw returns as described earlier. We compare years +1 through +3 in succession to year -1, for both treatment and control funds.

[18] If we extend the event window back to year -5 and forward to year +5, we find that treatment and control funds trend in parallel long before and long after a disaster.

[Insert Table 3 about here]

The results in Panel A of Table 3 confirm the results of Figure 2. There is a statistically significant decrease in volatility of approximately 60 basis points in year +1 compared to year -1. In year +2, volatility is still lower than in year -1 by a statistically significant 30 bps or so. By year +3, the difference-in-differences in volatility is no longer statistically significantly different from zero. The results of this formal test confirm our earlier inference that disasters lead to fund managers decreasing their risk taking significantly but temporarily.

Although we demonstrate that even disasters driven by damages have only a trivial effect on the wealth of mutual fund managers and investors, we directly examine whether the decrease in risk taking can be partly explained by wealth effects. To this end, we test whether disasters driven by damages (with possible wealth effects) have a different effect from disasters driven by fatalities (without possible wealth effects). Specifically, we create a dummy variable for disasters driven by damages but not driven by fatalities, and we run the same regressions as in Panel A but adding our new dummy variable as well as its interaction with the three previous dummy variables (Treatment, After, and Treatment × After).

The results in Panel B of Table 3 are similar to those for our baseline results in Panel A. However, the incremental effect of our new triple interaction of interest (Treatment × After × Damages) is not statistically significant in any of the years after the disaster. This suggests that our results cannot be explained by the wealth effects of our disasters.

3.2. Risk Decomposition

Next, we examine whether total risk decreases because of systematic risk or idiosyncratic risk or both. There is no broad consensus in the literature about the standard asset pricing model for international stocks. However, the literature does indicate that it would be appropriate to use the global four-factor model to explain the returns on global stock portfolios such as mutual funds (Fama and French (2012)). Our purpose is simply to approximate the contributions of systematic and idiosyncratic risk to total risk, so a sensible factor model satisfies our needs.

We estimate the systematic and idiosyncratic components of total volatility from the global four-factor model using monthly returns. We obtain data from Ken French's website on global market factor returns. The four factors capture the return on the market portfolio as well as the returns to size, book-to-market, and momentum portfolios (Griffin (2002)). We require a minimum of 24 months and a maximum of 36 months of returns for our estimates before a disaster, and we impose the same restrictions thereafter. As a result, we can only compare a single period before and after as opposed to several individual years before and after.

Finally, we run the same regressions as in Table 3 but with some limitations. We can only test a single difference-in-differences because there is only one non-overlapping three year period before a disaster, and the same is true thereafter. Moreover, our sample size decreases (by more than 10%) because we require at least two years of returns both before and after a disaster. Furthermore, as Figure 2 shows, volatility is relatively flat during the three years before a disaster but rises steeply during the three years thereafter. Comparing three years before and three years after will therefore understate the magnitude of the effect of a disaster by about 50% (by averaging the full effect in year +1 with no effect in year +3).

[Insert Table 4 about here]

Table 4 presents the results. Consistent with our baseline results, total volatility decreases, but by only roughly half of the magnitude in Table 3, due to the limitations already mentioned. Additionally, systematic volatility decreases, by about the same magnitude as the decrease in total volatility. By contrast, the change in idiosyncratic volatility is neither economically nor statistically significant.

To decompose the change in total risk into its systematic and idiosyncratic components, we perform a basic variance decomposition (results not tabulated). Rather than using standard deviation to measure risk, we use variance. As a basis of comparison, before a disaster, roughly 79% of total risk is systematic, on average, and about 21% is idiosyncratic, which is in line with what we would expect for mutual funds as well as diversified portfolios of stocks. After a disaster, the decrease in total risk relative to its mean is 9.9%, and it is larger for systematic risk (11.1%) than idiosyncratic risk (3.0%). Given the initial dominance of systematic risk over idiosyncratic risk, most of the change in total risk is explained by the change in systematic risk rather than idiosyncratic risk: roughly 93.4% versus 6.6%. In summary, the evidence suggests that disasters lead managers to decrease risk taking, principally by taking less systematic risk.

3.3. Cash versus Equity[19]

To further improve our understanding of the mechanism through which fund managers decrease portfolio risk, we examine the cash holdings of funds as well as their equity volatility derived from adjusting asset volatility by cash holdings. Managers can take less risk by increasing their cash holdings, which are risk free, and they can also take less risk by holding less risky stocks within the equity portion of their portfolio, a portion that is inherently more risky than the cash portion. We perform two tests in this regard. Both tests require data on cash holdings the availability of which means that our sample size decreases by about two-thirds. Using the reduced sample for

[19] We are grateful to an anonymous referee for suggesting this analysis.

which cash holdings data are available, we verify that our results are similar to those in our baseline regressions in Panel A of Table 3 (results not tabulated).

[Insert Table 5 about here]

First, we examine the cash-equity mix of funds using the ratio of cash to assets as the dependent variable and otherwise running regressions similar to those in Table 3. The results in Panel A of Table 5 show that the cash-to-assets ratio increases by roughly 2 percentage points in year +1, statistically significantly, and it is not statistically significant thereafter. For comparison, the cash holdings of our sample funds in year -1 are typically modest, with a mean (median) cash-to-assets ratio of 5% (3%). Now, the mandate of equity fund managers to remain fully invested limits their ability to make large adjustments to their cash holdings from the perspective of their assets as a whole. Nevertheless, 2 p.p. is a relatively large increase in the cash-to-assets ratio relative to its typical value. This suggests that increasing cash holdings is one way in which managers take less risk, although with a modest impact on their entire portfolio.

Second, and following from our first test, we examine equity volatility by adjusting asset volatility for cash holdings. Specifically, since cash is risk free and uncorrelated with equity and our sample funds are equity funds, we assume that cash plus equity equals total fund assets. Consequently, the volatility of equity can be calculated as the volatility of assets multiplied by the inverse of the weight of equity (which itself equals the ratio of assets to equity). Having adjusted volatility in this manner, we run the same regressions as in Table 3 but with adjusted volatility (i.e., equity volatility) as the dependent variable.

Panel B of Table 5 (volatility of equity) shows similar results to those in Panel A of Table 3 (volatility of assets). While the coefficient estimates are no longer statistically significant in year +3, they are only marginally insignificant in year +2 and still outrightly significant in year +1. Given that cash holdings are typically well under 10% of assets even after the effect of a disaster, we would expect that equity volatility would decrease by a similar magnitude to asset volatility. This is indeed what we find.

Finally, we consider the possibility that fund managers are distracted by disasters, and as a result they manage their portfolio less actively. The increase in cash holdings that we find is consistent with the managerial distraction explanation, but the decrease in equity volatility that we also find suggests that managerial distraction is not a complete explanation of our results. Furthermore, if managers are distracted, we would expect them to trade less frequently, which we can test by examining portfolio turnover. This is what we do, running regressions similar to those in Table 3. Our untabulated results indicate no statistically significant effect of disasters on portfolio turnover. This absence of evidence on turnover, together with the significant decrease in equity volatility, is inconsistent with the managerial distraction explanation.

4. Alternative Explanations

4.1. The Agency Explanation

One possible explanation for the decrease in risk taking resulting from natural disasters is that it reflects a decrease in manager-investor agency problems. To summarize, fund managers may be able to attract flows, and thereby increase their fees, by taking more risk. Natural disasters in the U.S. may hurt the performance of domestic funds and thus help the relative performance of international funds (e.g., the stocks held by the former, but not the latter, are affected by disasters). If investors move their money from domestic funds to international funds, then managers of the latter have less incentive to attract flows by taking more risk. This agency explanation predicts that there should be greater inflows for our sample of international funds following a natural disaster.

[Insert Figure 3 about here]

We examine this prediction by graphing monthly flows during the 37 months centered on the month of a disaster (three years). Every event month, we graph the cumulative difference in mean flows between treatment and control funds. The results are shown in Figure 3. It is worth noting that cumulative monthly flows are increasingly negative during the six months from months -18 to -13 (i.e., before the disaster). However, the roughly 6 percentage point difference between treatment and control funds during this period, or about 1% per month, on average, is small compared to the standard deviation of monthly flows of more than 8% (see Table 2).

Figure 3 shows that after a disaster, there is essentially no change in flows for at least 18 months thereafter. This result has a number of important implications. First, it does not support the agency explanation, which predicts inflows after the disaster. Second, it provides additional evidence that the decrease in risk taking cannot be explained by the wealth effects of our disasters, which predict greater outflows from investment funds including international equity mutual funds, as investors use their savings to replace their income. Finally, whatever the effect of disasters on local investors, the fund's investors as a group do not appear to respond to the performance implications, if any, of the decrease in risk taking.

To formally test whether disaster experiences result in a change in flows, we run the same regressions as in Table 3 but with some modifications. The dependent variable is now mean monthly flows rather than the volatility of monthly raw returns, and the independent variables now exclude annualized flows.

[Insert Table 6 about here]

The results of Table 6 confirm the results of Figure 3. The change in flows is neither statistically nor economically significant in any of the years after a disaster. The results indicate that disasters do not affect flows during the years thereafter. On the whole, the evidence does not support the agency explanation for the decrease in risk taking resulting from disasters.

4.2. The Managerial Skill Explanation

Another possible explanation is that the decrease in risk taking resulting from natural disasters reflects an increase in managerial skill. By way of summary, disasters may endow managers that experience them with an information advantage. Combined with stock selection or timing abilities, such an advantage may allow managers to outperform on a risk adjusted basis (Huang, Sialm, and Zhang (2011)). This skill explanation predicts that there should be an increase in risk adjusted returns as a result of natural disasters.

[Insert Figure 4 about here]

To examine this prediction, we first graph monthly returns during the 37 months centered on the month of a disaster (three years), just like in Figure 3, but for returns rather than flows. As Figure 4 shows, there is essentially no change in returns during the 18 months before a disaster, and there is no immediate change during the two months thereafter. This provides further evidence that treatment funds do not hold international stocks with greater local exposure than control funds. If they did, the value of these stocks should have decreased as a result of the disaster (and immediately thereafter), and consequently the returns of these funds would also have decreased, which is not what we observe in the data.

During the year or so starting three months after the disaster, there is indeed a decrease in returns, of about 2.5 percentage points cumulatively, or roughly 20 bps per month, on average. By way of comparison, the standard deviation of monthly raw returns is 1.9% (see Table 2). However, the decrease in the returns of funds that experience a disaster would appear to be consistent with the decrease in their risk. At the same time, it may still be consistent with the skill explanation, which predicts an increase in risk adjusted returns but not necessarily raw returns.

[Insert Figure 5 about here]

In a related but more general analysis, we also graph the death rate of our treatment and control funds during the years after a disaster. Figure 5 shows that the proportion of funds that die after a disaster is similar for treatment and control funds. In other words, disasters do not appear to affect fund survival. Once again, this does not provide definitive evidence for or against the skill explanation according to which it is risk adjusted returns that should increase.

[Insert Table 7 about here]

We formally test whether disaster experiences affect returns using the regressions in Table 3 with a few modifications. The dependent variable is now mean monthly raw returns rather than the volatility of monthly raw returns, and the independent variables now exclude annualized raw returns. Table 7 Panel A presents the results. Monthly raw returns in year +1 are 14 basis points lower, or about 1.7 percentage points lower on an annualized basis, but the results are not statistically significant.[20] Consistent with the pattern for risk taking, the coefficient estimates for

[20] We also examine the statistical significance of mean monthly returns over horizons of less than one year but still during the year after a disaster. Our estimates are in line with the pattern of returns depicted in Figure 4. The results are generally statistically significant during the months in the first half of the year but not thereafter.

returns decrease in magnitude over time. In summary, the results suggest that disasters do not affect raw returns during the years thereafter.

However, it is risk adjusted returns rather than raw returns that are predicted to increase according to the skill explanation. We now turn to testing this prediction first using alphas. With this measure, we need to assume an equilibrium asset pricing model. We estimate alphas from the global four-factor model, as in Table 4. We run a modified version of the regressions in Table 4, with the dependent variable now being alpha and the independent variables now excluding annualized raw returns. The limitations are the same as for Table 4: we only have one period before and one period after the disaster; the sample size decreases slightly; and averaging over the years after the disaster will understate its effect. The results in Table 7 Panel B indicate that the change in alpha is neither statistically nor economically significant.

We also test the prediction of the skill explanation using Sharpe ratios. This risk-return measure is well suited to well diversified portfolios of stocks, and it does not require us to assume a particular equilibrium asset pricing model. We run modified versions of the regressions in Table 3, with the dependent variable now being the monthly Sharpe ratio and the independent variables now excluding annualized volatility. Table 7 Panel C presents the results, none of which are statistically significant in any year. The results in Panel C, like the results in the previous two panels, do not support the skill explanation.

Taken as a whole, the results indicate that the disaster experiences of mutual fund managers do not appear to affect the returns to their investors. This is the case on both a raw and risk adjusted basis. More precisely, the latter evidence does not support the skill explanation for the decrease in risk taking caused by disasters. At the same time, we acknowledge that our standard statistical tests have less power to detect changes in the first moment of stock returns than the second.

4.3. The Catering Explanation

A final possible explanation is that the decrease in risk taking resulting from natural disasters reflects managerial catering to local investor demand for less risk taking. Disasters may not affect managers, but they might affect local investors and their risk attitudes. This explanation predicts that the change in risk taking should be positively related to the clustering of investors within the county of the fund. Note that we are interested in the clustering of investors in a fund around the fund itself, not the clustering of these investors around the stocks held by a fund.

We test this catering explanation using two proxies for the aforementioned clustering: fund size (small versus large) and fund investor type (institutional versus retail). While investors in the smallest funds may be clustered locally, this is highly improbable for the largest funds because their geographic coverage would have to be substantial in order to cover many investors and thus amass a large pool of assets under management. Similarly, institutional funds may be dominated by locally clustered institutional investors, but this is unlikely for retail funds. This is because institutional investors generally have regular and personal contact with the managers of the funds in which they invest. To minimize the requisite communication costs, institutions tend to be located close to their funds (e.g., Hochberg and Rauh (2013) and Sialm, Sun, and Zheng (2019)). By contrast, retail investors generally have no access to the managers of the funds in which they invest, so physical proximity is of little importance in their case. In consequence, funds seek out retail investors from around the country, not just close to the fund. Turning to the predictions of the catering explanation, there should be a greater decrease in risk taking for the smallest funds and for institutional funds.

We run the same regressions as in Table 3 but with a modification. Table 3 includes three dummy independent variables (for treatment funds, for the post-disaster period, and for their interaction). As an addition, we interact these dummy variables with a pair of dummy variables either for the smallest and largest funds or for institutional only and retail only funds. We also include the pair of dummy variables itself as independent variables. We sort funds based on total net assets and consider those in the bottom and top deciles as the smallest and largest, respectively.

[Insert Table 8 about here]

The results are presented in Table 8. In both Panels A and B, the main difference-in-differences (treatments versus controls, after versus before) has roughly the same economic and statistical significance as in our baseline results (Table 3). However, the effects of small funds versus large funds, as compared to funds in the middle, are not significant (Panel A). Similarly, the effects of institutional only versus retail only funds, compared to funds that are mixed, are also insignificant (Panel B). In summary, the results provide no evidence of a difference in risk taking between small and institutional funds with investors that are plausibly concentrated locally and large and retail funds with investors dispersed around the country. This evidence does not support the catering explanation for the increase in risk taking generated by disasters.[21]

5. Cross-Sectional Contrasts and Robustness Tests

In this section, we first examine several cross-sectional contrasts that support the psychological interpretation of our results. We then perform a number of robustness tests. For our tests, we use modified versions of the regressions in Table 3.

First, we examine whether managers react more strongly to disasters when they have fewer recent local disaster experiences. Managers experiencing a disaster for the first time should have a

[21] Since the agency explanation also requires that investors cluster locally, the foregoing evidence similarly does not support that explanation.

stronger psychological reaction to the disaster than managers that have already experienced disasters many times. To test this prediction, in addition to the three dummy independent variables in Table 3 (Treatment, After, and Treatment × After), we add an interaction with another independent dummy variable. This fourth dummy independent variable captures fund-years in the bottom half of the rate of disasters in the county during the previous 10 years.

[Insert Table 9 about here]

Table 9 Panel A presents the results. As expected, the impact of a disaster is greater if it follows a period of few disasters than if it follows a period of many disasters. When there have been few disasters in the recent past, risk taking falls by 89 basis points in year +1 and is still down by 59 bps in year +2. While the incremental effect of a low rate of prior disasters is economically significant for two years after a disaster, we are somewhat cautious about the interpretation of these results because they are only marginally statistically significant. However, the evidence does broadly suggest that managers react more strongly to their first experience with disasters compared to their subsequent disaster experiences. This is consistent with a psychological reaction to disasters, but it is also consistent with rational learning about disasters in the sense that the occurrence of a disaster is informative to managers about subsequent occurrences thereof. We further explore learning in our next two tests.

In our second cross-sectional contrast, we examine whether the managerial reaction to disasters is stronger in the first half of our sample period than in the second. We have already seen in Figure 1 that neither the rate of disasters nor the losses caused by disasters trends over time. However, it is possible that there is still a learning effect about the rate of disasters or the loss from disasters. (For example, given disaster preparedness efforts, actual disasters may have turned out to be less deadly and/or damaging than people had expected.) If there is learning, then the effect of disasters on risk taking should weaken over time. We test this prediction as in Panel A of Table 9, but this time the fourth dummy independent variable captures the first half of the sample period.

The results are presented in Panel B of Table 9. For all of years +1 through +3, the results for the entire sample period are similar, in both economic and statistical terms, to the results in Table 3. By contrast, the results for the first half of our sample period are not significantly different. In summary, we do not find evidence of a learning effect, or, more generally, a change over time in the effect of disasters on risk taking.

Our third cross-sectional contrast is closely related to our second. We examine whether the managerial reaction to disasters is stronger for younger funds than older ones. Even if there is no learning over time across all funds, there could still be learning within individual funds. Our test of this prediction is the same as in Panel B of Table 9, but the fourth dummy independent variable now captures the bottom half of the sample by fund age. The results (not tabulated) are not significantly different for younger funds compared to older funds. Once again, we do not find evidence of a learning effect.

We now turn to our robustness tests,[22] the results of which are tabulated in the Internet Appendix. We begin by examining whether our results are robust to using an alternative matching methodology to account for the investment strategy of funds. Rather than matching based on fund characteristics, we match based on the correlation of fund returns. We continue to match treatment and control funds based on the target region of the world in which they invest. However, rather than proceeding with propensity score matching, we instead select the control fund that has the highest correlation of returns with the treatment fund to which we are matching. To estimate correlations,

[22] We thank an anonymous referee for suggesting several of these.

we require a minimum of 12 months and a maximum of 36 months of returns for each fund before a disaster. Otherwise, we run the same regressions as for our baseline results in Table 3. Our inferences from the results here are similar to those from our baseline results.

In a related robustness test, we examine whether our results are significantly different based on the target region of the fund. While a mere 6% of our funds focus only on developed markets, a more sizable 22% focus on emerging markets (Section 2.2), which should allow us to draw meaningful inferences about differences across target regions. We again run the same baseline regressions, but we include two dummy variables, one for emerging markets and one for developed markets, and we also include the requisite interactions. The results do not indicate any significant incremental effect for either emerging markets or developed markets. This suggests that fund managers do not behave differently based on the target region of their fund, but it is also consistent with our control funds providing reasonable benchmarks for our treatment funds.

Next, we examine whether our results are robust to using alternative asset pricing models. Instead of estimating the systematic and idiosyncratic components of total volatility from the global four-factor model, we variously use the global market model and the global five-factor model. We find that the results are similar for these alternative asset pricing models.

It is also possible that our funds have significant local exposure because they invest in foreign firms that operate in the county of the fund. Note that this would require an extreme local bias on the part of fund managers and similarly extreme local operational focus on the part of their portfolio firms. (By far the most plausible example in our sample would be a fund manager in Chicago (Cook County, IL) investing much of his portfolio in foreign firms that also had much of their operations in Chicago.) To test this prediction, we sort our sample based on whether a fund has domestic exposure. The "no domestic exposure" group comprises the roughly 80% of our funds that have no more than 1% of their assets invested in domestic stocks, while the "some domestic exposure" group comprises the remaining funds. We run the same baseline regressions, but we include a dummy variable to capture funds with some domestic exposure, and we also include the requisite interactions. We find that the results for funds with some domestic exposure are not significantly different.

Additionally, it is possible that funds located in major financial centers are driving the results. For example, financial centers may have a higher concentration of funds with both managers and investors that are more locally biased than elsewhere. Alternatively, more intense competition between fund managers in financial centers may greatly magnify an otherwise small decrease in risk taking. We test this prediction by running the same baseline regressions, but we sort our sample based on whether a fund is located in a financial center, which comprises New York, Philadelphia, Chicago, Boston, Los Angeles, and San Francisco. We include in our regressions a dummy variable to capture funds located in a financial center, and we also include the requisite interactions. We find that the results are not significantly different for funds based on whether they are located in financial centers.

Finally, we examine whether our results are robust to using different thresholds to define severe disasters. For our baseline results, we use roughly the top 0.1% of fatalities and damages at the county-month level, which strikes a good balance between capturing the most severe disasters and generating a large enough sample of fund-years. Our baseline sample of approximately 500 fund-years with disasters comprises about 5% of all international equity mutual funds. We now lower the thresholds for fatalities and damages so that we capture an additional 50% and 100% fund-years (i.e., approximately 750 and 1,000 fund-years, respectively, corresponding to up to 7.5% and 10% of the universe of relevant funds). We rerun our baseline regressions with these larger samples. The results remain economically and statistically significant in year +1, and they decrease in magnitude as we include more funds that are less affected by disasters, consistent with our expectations. Additionally, we also raise the thresholds for fatalities and damages so that we capture 50% fewer fund-years. Our overall inferences remain unchanged (results not tabulated).[23] Taken as a whole, the evidence suggests that our results do not depend critically on the definition of severe disasters.

6. Conclusion

We study the effect of personal catastrophic experiences, in the form of natural disasters in the U.S., on the risk attitudes of professional investors, as embodied by U.S.-based managers of mutual funds that invest in non-U.S. stocks. Our approach has numerous advantages, including: focusing on professional risk takers; precisely measuring risk taking as portfolio volatility; and using shocks that can affect risk attitudes through psychology but cannot rationally affect portfolio choice. In our empirical analysis, we take a difference-in-differences approach. We compare managers in counties that experience a disaster to managers in counties without a disaster while controlling for fund and county characteristics.

We find that risk taking decreases significantly but temporarily after a disaster. Since the results are similar for disasters with only fatalities and only property damage, our findings do not support wealth effects. Decomposition of risk indicates that the decrease is primarily attributable to systematic rather than idiosyncratic risk. Given the advantages of our approach, the natural interpretation of our findings is a psychological one. Alternative explanations include managerial

[23] Note that we are drawing smaller samples from an absolutely very small sample of very rare events. Statistically, for a higher threshold, even if the response of subjects to bigger shocks is stronger in the population itself, this does not guarantee that in any particular draw the response will in fact be stronger.

agency, skill, and catering, but additional results do not support these explanations. Since mutual fund investors choose funds that take risks in line with investors' own risk preferences, our results suggests that managers' personal catastrophic experiences may result in a temporary misalignment of their risk preferences and those of their investors.

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Table 1

Quality of the Matching of Treatment and Control Funds

This table shows the quality of the matching of treatment and control funds. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S. The variables shown are matching covariates and propensity scores. Variables are defined in Appendix Table 1.

	Mean		Median		p-value of test of equality of means	p-value of test of equality of medians
	Treat-ment firms	Control firms	Treat-ment firms	Control firms		
ln(Total net assets) ($ millions)	4.93	4.73	4.92	4.50	0.086	0.015
ln(Annual turnover ratio) (%)	-62%	-66%	-60%	-58%	0.439	0.832
Annualized raw returns (%)	5.5%	4.7%	10.1%	9.1%	0.488	0.352
Annualized flows (%)	23%	34%	3%	7%	0.045	0.266
Annualized volatility (%)	19.2%	19.5%	18.5%	19.4%	0.517	0.269
Rate of prior disasters (%)	6.8%	6.4%	3.8%	4.0%	0.316	0.531
Propensity score	0.072	0.070	0.051	0.051	0.523	0.792

Table 2

Descriptive Statistics

This table presents descriptive statistics for the main variables used in this paper. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S. Each fund can appear for up to three years before and after the disaster. Variables are defined in Appendix Table 1.

	Mean	Standard deviation	25th percentile	Median	75th percentile
Independent variables					
- Total net assets ($ millions)	712	1,946	37	124	443
- Annual turnover ratio (%)	72.5	61.6	30.0	53.0	96.0
- Annualized raw returns (%)	5.1	19.5	-8.4	9.6	17.4
- Annualized flows (%)	26.4	85.4	-11.9	4.9	30.6
- Annualized volatility (%)	19.4	7.8	14.0	19.0	22.2
Dependent variables					
- Volatility of monthly raw returns (%)	4.99	2.04	3.49	4.68	6.12
- Total volatility of monthly returns (%)	5.24	1.82	3.94	4.79	6.41
- Systematic volatility of monthly returns (%)	4.71	1.63	3.54	4.34	5.84
- Idiosyncratic volatility of monthly returns (%)	2.07	1.26	1.20	1.67	2.46
- Mean monthly flows (%)	2.04	8.18	-1.15	0.29	2.41
- Mean monthly raw returns (%)	0.35	1.90	-0.99	0.61	1.56
- Monthly alpha (%)	-0.37	0.64	-0.62	-0.31	-0.02
- Monthly Sharpe ratio	0.124	0.392	-0.185	0.137	0.405

Table 3

The Effect of Natural Disasters on Mutual Fund Risk Taking: Total Risk

This table shows the effect on risk taking of U.S. natural disasters experienced by U.S. managers of international equity mutual funds. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S. Each fund can appear for up to three years before and after the disaster. The dependent variable is the volatility of monthly raw returns. In Panel A, the dependent variable is regressed on three dummy variables: one for the treatment funds, another for the post-disaster period, and a third for the interaction of the first two. The other independent variables are the fund's total net assets, annual turnover ratio, annualized raw returns, and annualized flows. The first two of these variables are measured in natural logarithms. In Panel B, a dummy variable is added for disasters driven by damages but not by fatalities. This dummy variable is included by itself as well as interacted with the three dummy variables in Panel A. Variables are defined in Appendix Table 1. The dependent variables are multiplied by 100. Standard errors are clustered by county-year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Only selected results are tabulated.

Panel A: Pooling Together Disasters Driven by Fatalities and Damages

	Dependent variable is volatility of monthly raw returns		
	Year +1	Year +2	Year +3
Treatment dummy variable	-0.62***	-0.32**	-0.10
× After dummy variable	(-3.43)	(-2.03)	(-0.67)
Treatment dummy variable	-0.09	-0.08	-0.09
	(-0.78)	(-0.67)	(-0.76)
After dummy variable	0.23	0.21	0.01
	(1.38)	(1.17)	(0.07)
Observations	1,919	1,863	1,806
Adjusted R^2	0.483	0.525	0.547

Panel B: Separating Disasters Driven by Damages from Disasters Driven by Fatalities

	Dependent variable is volatility of monthly raw returns		
	Year +1	Year +2	Year +3
Treatment dum. × After dum.	0.13	0.28	0.01
× Damages dum.	(0.43)	(0.94)	(0.04)
Treatment dummy variable	-0.66***	-0.41**	-0.10
× After dummy variable	(-3.11)	(-2.09)	(-0.56)
Treatment dummy variable	-0.11	-0.10	-0.11
	(-0.81)	(-0.68)	(-0.75)
After dummy variable	0.53**	0.36	0.14
	(2.12)	(1.51)	(0.79)
Observations	1,919	1,863	1,806
Adjusted R^2	0.485	0.525	0.547

Table 4

The Effect of Natural Disasters on Mutual Fund Risk Taking: Systematic versus Idiosyncratic Risk

This table shows the effect on risk taking of U.S. natural disasters experienced by U.S. managers of international equity mutual funds. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S. Each fund can appear for up to three years before and after the disaster. The dependent variable is the total, systematic, and idiosyncratic volatility of monthly returns. Volatility is estimated from the global four-factor model using monthly returns over three years before the disaster and three years after the disaster. The dependent variable is regressed on three dummy variables: one for the treatment funds, another for the post-disaster period, and a third for the interaction of the first two. The other independent variables are the fund's total net assets, annual turnover ratio, annualized raw returns, and annualized flows. The first two of these variables are measured in natural logarithms. Variables are defined in Appendix Table 1. The dependent variables are multiplied by 100. Standard errors are clustered by county-year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Only selected results are tabulated.

	Dependent variable is volatility of monthly returns		
	Total	Systematic	Idiosyncratic
Treatment dummy variable	-0.30**	-0.32**	-0.01
× After dummy variable	(-2.30)	(-2.69)	(-0.33)
Treatment dummy variable	-0.18*	-0.15	-0.10
	(-1.93)	(-1.71)	(-1.64)
After dummy variable	-0.07	0.07	-0.29
	(-0.17)	(0.18)	(-1.68)
Observations	1,657	1,657	1,657
Adjusted R^2	0.421	0.513	0.216

Table 5

The Effect of Natural Disasters on Mutual Fund Risk Taking: The Role of Cash Holdings

This table shows the effect on risk taking of U.S. natural disasters experienced by U.S. managers of international equity mutual funds. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S. Each fund can appear for up to three years before and after the disaster. The dependent variable is the cash-to-assets ratio in Panel A and the volatility of monthly raw returns adjusted for cash holdings in Panel B. The dependent variable is regressed on three dummy variables: one for the treatment funds, another for the post-disaster period, and a third for the interaction of the first two. The other independent variables are the fund's total net assets, annual turnover ratio, annualized raw returns, and annualized flows in both panels plus annualized volatility in Panel A. The first two of these variables are measured in natural logarithms. Variables are defined in Appendix Table 1. The dependent variables are multiplied by 100. Standard errors are clustered by county-year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Only selected results are tabulated.

Panel A: Cash Holdings

	Dependent variable is cash-to-assets ratio		
	Year +1	Year +2	Year +3
Treatment dummy variable	2.07*	0.87	1.77
× After dummy variable	(1.83)	(0.66)	(1.41)
Treatment dummy variable	-1.58*	-1.32	-1.54*
	(-1.72)	(-1.43)	(-1.74)
After dummy variable	-1.79*	-1.23	-2.73***
	(-1.81)	(-0.81)	(-2.78)
Observations	660	635	593
Adjusted R^2	0.149	0.138	0.119

Panel B: Volatility Adjusted for Cash Holdings

	Dependent variable is volatility of monthly raw returns		
	Year +1	Year +2	Year +3
Treatment dummy variable	-0.51**	-0.37	-0.24
× After dummy variable	(-2.01)	(-1.59)	(-1.06)
Treatment dummy variable	-0.23	-0.18	-0.25
	(-1.37)	(-1.11)	(-1.50)
After dummy variable	-0.17	0.29	-0.35
	(-0.60)	(1.11)	(-1.41)
Observations	660	635	593
Adjusted R^2	0.659	0.640	0.650

Table 6

The Effect of Natural Disasters on Mutual Fund Flows

This table shows the effect on flows of U.S. natural disasters experienced by U.S. managers of international equity mutual funds. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S. Each fund can appear for up to three years before and after the disaster. The dependent variable is mean monthly flows. The dependent variable is regressed on three dummy variables: one for the treatment funds, another for the post-disaster period, and a third for the interaction of the first two. The other independent variables are the fund's total net assets, annual turnover ratio, and annualized volatility. The first two of these variables are measured in natural logarithms. Variables are defined in Appendix Table 1. The dependent variables are multiplied by 100. Standard errors are clustered by county-year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Only selected results are tabulated.

	Dependent variable is mean monthly flows		
	Year +1	Year +2	Year +3
Treatment dummy variable	0.70	0.70	0.60
× After dummy variable	(1.34)	(1.37)	(1.16)
Treatment dummy variable	-0.51	-0.51	-0.52
	(-1.09)	(-1.08)	(-1.09)
After dummy variable	-1.75***	-1.66***	-2.15***
	(-3.91)	(-3.35)	(-4.57)
Observations	1,918	1,861	1,804
Adjusted R^2	0.066	0.106	0.104

Table 7

The Effect of Natural Disasters on Mutual Fund Performance

This table shows the effect on performance of U.S. natural disasters experienced by U.S. managers of international equity mutual funds. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S. Each fund can appear for up to three years before and after the disaster. The dependent variable is the mean monthly raw return in Panel A, the monthly alpha in Panel B, and the monthly Sharpe ratio in Panel C. Alpha is estimated from the global four-factor model using monthly returns over three years before the disaster and three years after the disaster. The dependent variable is regressed on three dummy variables: one for the treatment funds, another for the post-disaster period, and a third for the interaction of the first two. The other independent variables are the fund's total net assets, annual turnover ratio, and annualized flows in all panels plus annualized volatility in all panels except in Panel C. The first two of these variables are measured in natural logarithms. Variables are defined in Appendix Table 1. The dependent variables are multiplied by 100. Standard errors are clustered by county-year. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Only selected results are tabulated.

Panel A: Mean Monthly Raw Returns

	Dependent variable is mean monthly raw return		
	Year +1	Year +2	Year +3
Treatment dummy variable	-0.14	-0.06	0.08
× After dummy variable	(-1.29)	(-0.59)	(0.84)
Treatment dummy variable	0.07	0.08	0.07
	(0.96)	(1.12)	(1.06)
After dummy variable	-0.03	-0.15	-0.13
	(-0.35)	(-1.56)	(-1.30)
Observations	1,919	1,863	1,806
Adjusted R^2	0.683	0.671	0.674

Panel B: Monthly Alphas

	Dependent variable is monthly alpha
	Year +1 to Year +3
Treatment dummy variable × After dummy variable	-0.06
	(-0.90)
Treatment dummy variable	0.03
	(0.68)
After dummy variable	0.03
	(0.32)
Observations	1,657
Adjusted R^2	0.152

Panel C: Monthly Sharpe Ratios

	Dependent variable is monthly Sharpe ratio		
	Year +1	Year +2	Year +3
Treatment dummy variable	-2.98	-3.01	-0.83
× After dummy variable	(-1.53)	(-1.48)	(-0.44)
Treatment dummy variable	3.65***	3.64***	3.65***
	(2.76)	(2.67)	(2.69)
After dummy variable	0.09	-1.23	0.42
	(0.06)	(-0.75)	(0.23)

Observations	1,919	1,863	1,806
Adjusted R^2	0.706	0.730	0.733

Table 8

The Effect of Natural Disasters on Mutual Fund Risk Taking: The Role of Local Investor Clusters

This table shows the moderating role of local investor clusters on the effect of natural disasters on mutual fund risk taking. The same regressions are run as in Table 3 but with minor modifications. The three dummy independent variables (treatment funds, post-disaster period, and their interaction) are interacted with a pair of dummy independent variables either for the smallest and largest funds or for institutional only and retail only funds. The pair of dummy variables itself is also included as independent variables. The smallest and largest funds are in the bottom and top deciles, respectively, of total net assets. The dependent variables are multiplied by 100. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Only selected results are tabulated.

Panel A: Difference-in-Differences for the Smallest Funds versus the Largest Funds

	Dependent variable is volatility of monthly raw returns		
	Year +1	Year +2	Year +3
Treatment dummy var. × After dummy var.	0.54	0.11	0.30
× Smallest funds dummy variable	(1.02)	(0.24)	(0.70)
Treatment dummy var. × After dummy var.	0.13	-0.14	0.05
× Largest funds dummy variable	(0.29)	(-0.32)	(0.12)
Treatment dummy variable	-0.68***	-0.32*	-0.14
× After dummy variable	(-3.49)	(-1.85)	(-0.81)
Other dummy variables?	Yes	Yes	Yes
Observations	1,919	1,863	1,806
Adjusted R^2	0.484	0.525	0.547

Panel B: Difference-in-Differences for Institutional Only Funds versus Retail Only Funds

	Dependent variable is volatility of monthly raw returns		
	Year +1	Year +2	Year +3
Treatment dummy var. × After dummy var.	0.24	0.20	0.24
× Retail only funds dummy variable	(0.77)	(0.62)	(0.80)
Treatment dummy var. × After dummy var.	0.05	0.19	0.29
× Institutional only funds dummy variable	(0.14)	(0.58)	(0.95)
Treatment dummy variable	-0.70**	-0.47*	-0.30
× After dummy variable	(-2.38)	(-1.89)	(-1.23)
Other dummy variables?	Yes	Yes	Yes
Observations	1,919	1,863	1,806
Adjusted R^2	0.485	0.525	0.548

Table 9

Cross-Sectional Contrasts

This table shows cross-sectional contrasts for the effect of natural disasters on mutual fund risk taking. The same regressions are run as in Table 3 but with minor modifications. The three dummy independent variables (treatment funds, post-disaster period, and their interaction) are interacted with another dummy independent variable. This fourth dummy independent variable captures fund-years in either the bottom half of the rate of disasters in the county during the previous 10 years, or in the first half of the sample period. The dependent variables are multiplied by 100. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% levels, respectively. Only selected results are tabulated.

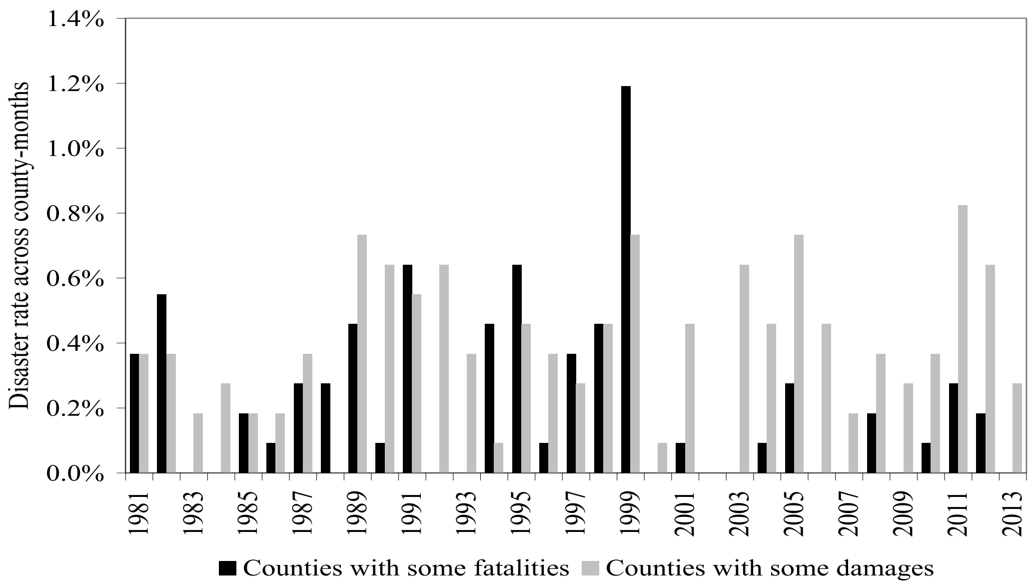
Panel A: Difference-in-Differences for Low versus High Rate of Prior Disasters

	Dependent variable is volatility of monthly raw returns		
	Year +1	Year +2	Year +3
Treatment dummy var. × After dummy var.	-0.56*	-0.54*	-0.17
× Few recent local disasters dummy variable	(-1.71)	(-1.76)	(-0.63)
Treatment dummy variable	-0.33	-0.05	-0.01
× After dummy variable	(-1.16)	(-0.22)	(-0.05)
Other dummy variables?	Yes	Yes	Yes
Observations	1,919	1,863	1,806
Adjusted R^2	0.484	0.527	0.548

Panel B: Difference-in-Differences for First versus Second Half of Sample Period

	Dependent variable is volatility of monthly raw returns		
	Year +1	Year +2	Year +3
Treatment dummy var. × After dummy var.	0.27	0.35	0.00
× First half dummy variable	(0.56)	(0.84)	(0.00)
Treatment dummy variable	-0.65***	-0.37**	-0.10
× After dummy variable	(-3.25)	(-2.27)	(-0.59)
Other dummy variables?	Yes	Yes	Yes
Observations	1,919	1,863	1,806
Adjusted R^2	0.483	0.529	0.546

Panel A: Disaster rate



Panel B: Loss given disaster

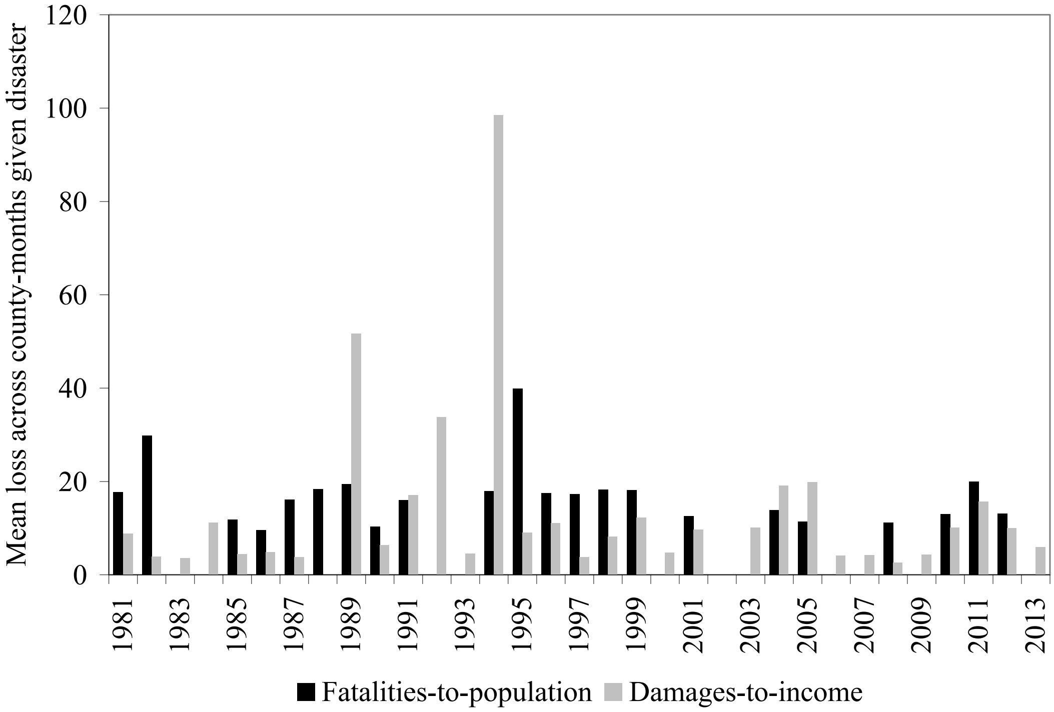


Figure 1. The disaster rate and the loss given disaster over time at the county level. This figure presents the disaster rate and the loss given disaster across county-months. The sample comprises all counties in which a mutual fund is located in any year between 1981 and 2013. Disasters driven by fatalities are presented separately from disasters driven by damages. Fatalities are reported per million people and damages are reported in dollars per $1,000 of income. Results are presented at the year level rather than the month level for ease of interpretation.

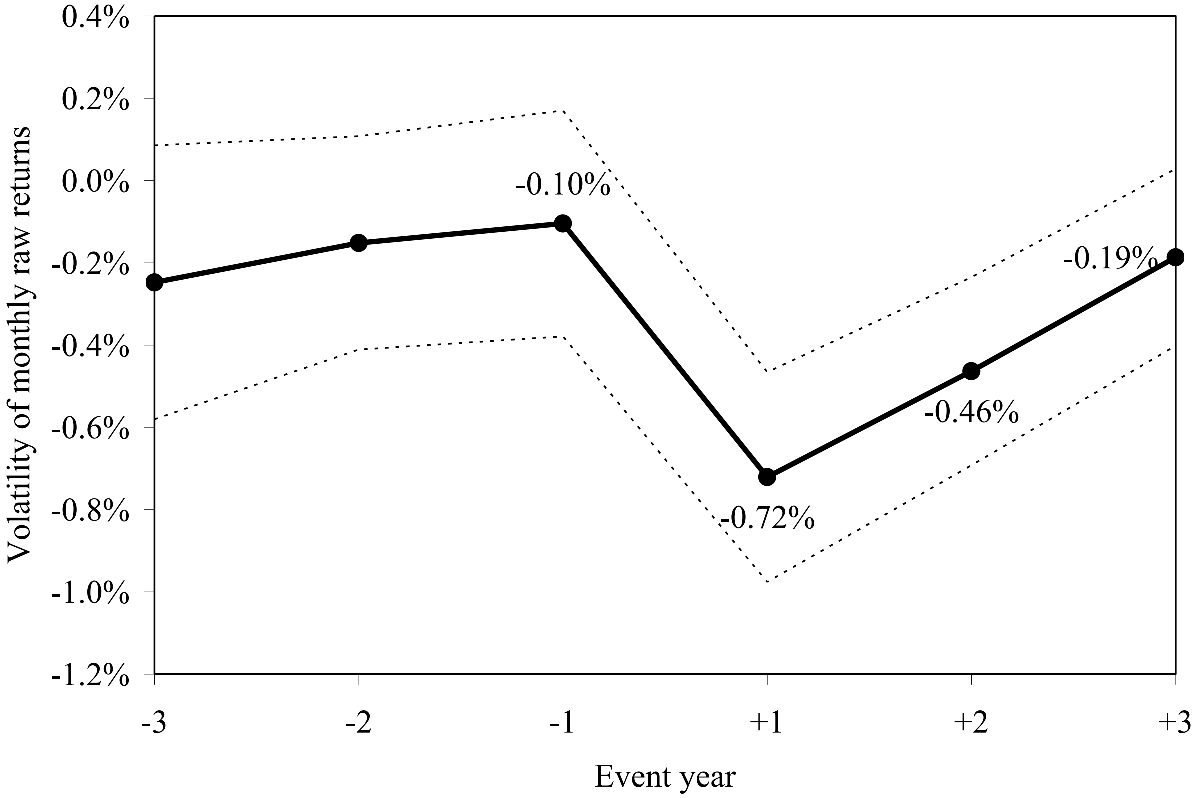


Figure 2. Mutual fund risk taking around disaster experiences. This figure presents the difference in mean risk taking between treatment and control funds before and after disasters experienced by mutual fund managers. The dashed lines represent a 95% confidence interval. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S. Risk taking is measured as the volatility of monthly raw returns.

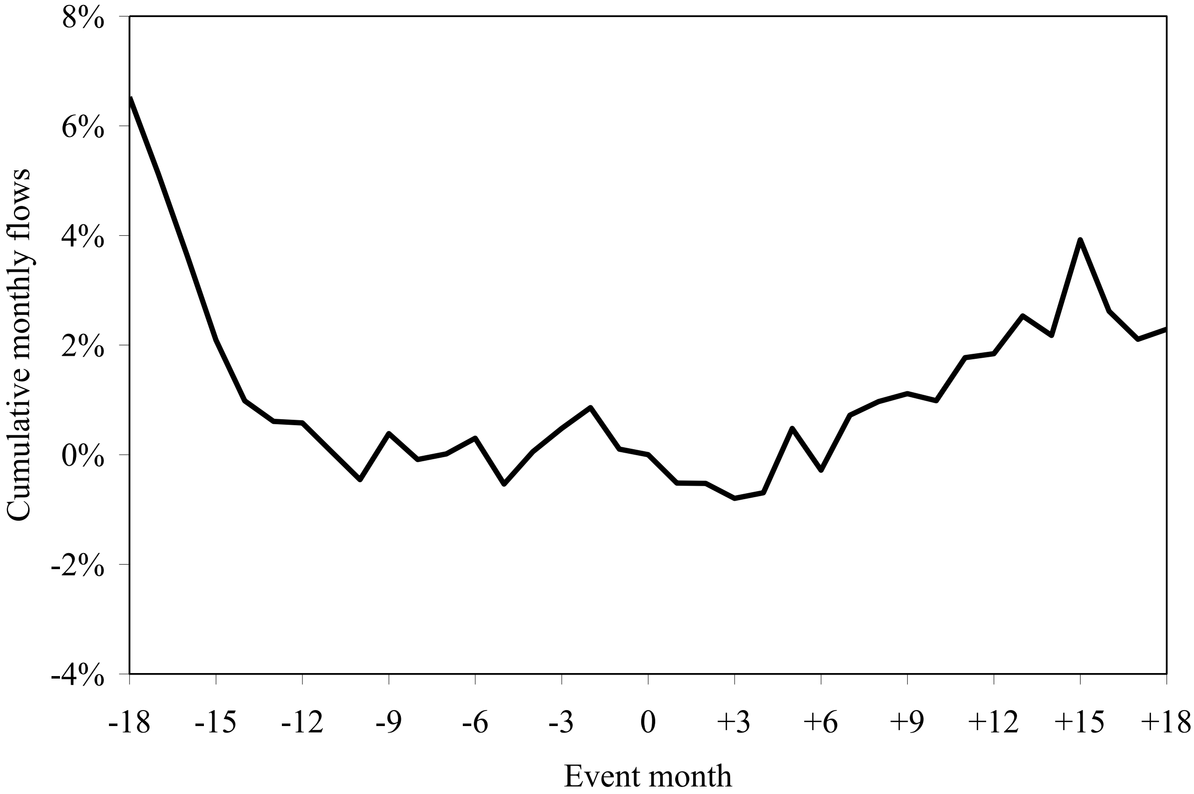


Figure 3. Mutual fund flows around disaster experiences. This figure presents the cumulative difference in mean monthly flows between treatment and control funds before and after disasters experienced by mutual fund managers. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S.

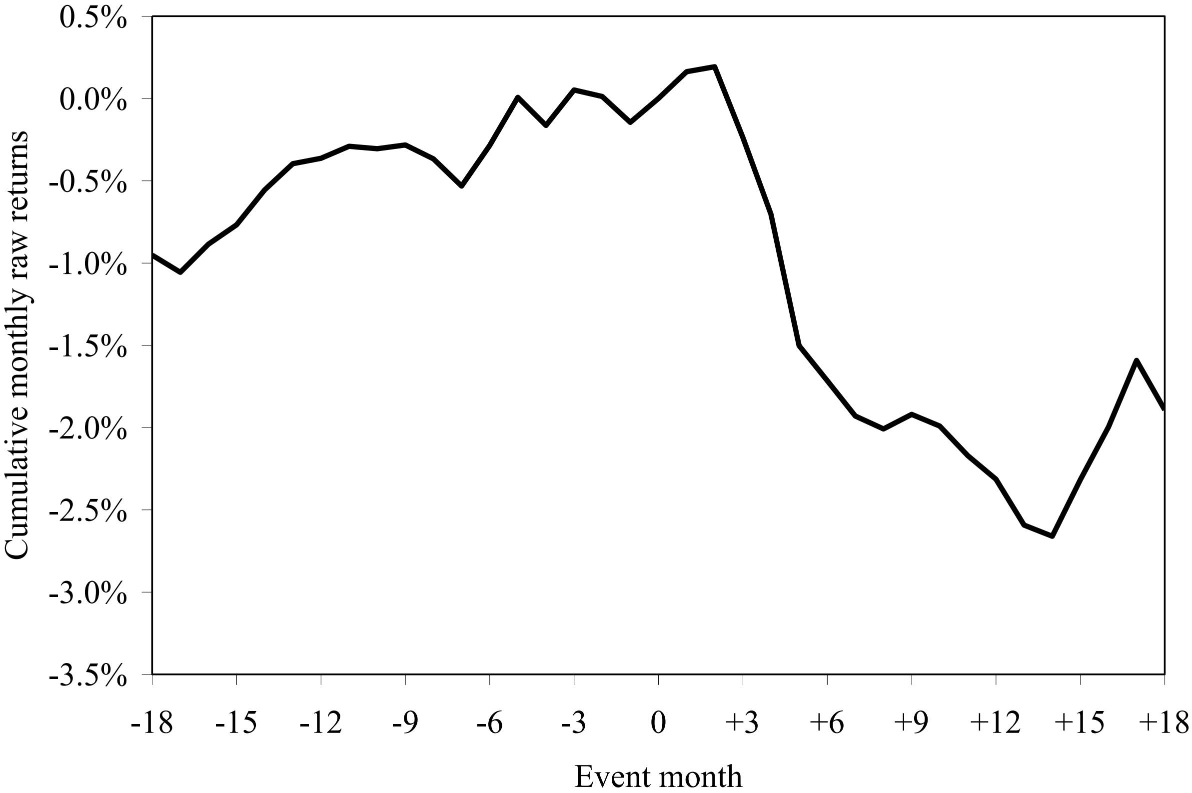


Figure 4. Mutual fund returns around disaster experiences. This figure presents the cumulative difference in mean monthly raw returns between treatment and control funds before and after disasters experienced by mutual fund managers. The sample comprises 483 treatment fund-years between 1981 and 2013, and the same number of matched control fund-years. Treatment fund-years experience a natural disaster in the U.S., whereas control fund-years do not. All funds are international equity mutual funds based in the U.S.

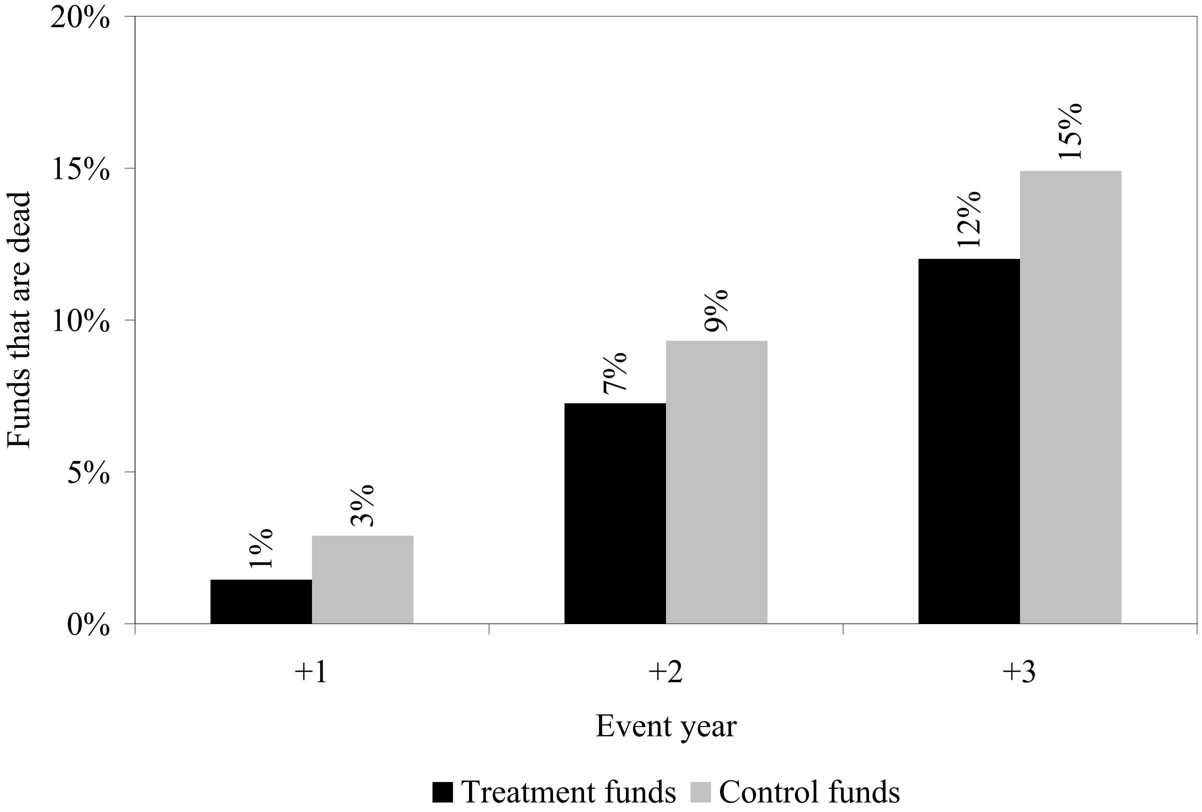


Figure 5. Mutual fund deaths after disaster experiences. This figure presents the proportion of funds that die after disasters experienced by mutual fund managers. The sample comprises 483 fund-years for treatment funds and the same number of fund-years for matched control funds. The sample spans the years 1981-2013. All funds are international equity mutual funds based in the U.S.

Appendix Table 1

Variable Definitions

Name	Definition
Disaster variables	
- Fatalities driven disaster	A minimum of 8 fatalities per million people in the county-month (approximately the top 0.1% of fatalities-to-population)
- Damages driven disaster	A minimum of $2,000 of property damage per million dollars of income in the county-month (approximately the top 0.1% of damages-to-income)
- Disaster	A fatalities driven disaster and/or a damages driven disaster
Independent variables	
- Total net assets	The total net assets of the fund at the end of the year
- Annual turnover ratio	The annual turnover ratio of the fund
- Annualized raw returns	The raw returns of the fund during the year annualized from monthly returns
- Annualized flows	The flows of the fund during the year annualized from monthly flows. Monthly flows are calculated as the growth rate of the total net assets of the fund minus the returns of the fund.
- Annualized volatility	The annualized standard deviation of the monthly raw returns of the fund during the year
- Prior probability of disaster	The probability of a disaster in the county measured from 1970 until the current year
Dependent variables	
- Volatility of monthly raw	The standard deviation of monthly raw returns of the fund during the year

returns	
- Systematic, idiosyncratic, and total volatility of monthly returns and monthly alpha	These variables are estimated from the global four-factor model using monthly returns. Estimates before and after the disaster both require a minimum of 24 months and a maximum of 36 months of returns.
- Mean monthly flows	The mean of the monthly flows of the fund during the year. Monthly flows are calculated as the growth rate of the total net assets of the fund minus the returns of the fund.
- Mean monthly raw returns	The mean of the monthly raw returns of the fund during the year
- Monthly Sharpe ratio	The ratio of the mean monthly raw returns of the fund to the volatility of monthly raw returns