Fear and Risk:
Do Visceral Factors Affect Risk Taking?

Vicki L. Bogan
Cornell University
vlb23@cornell.edu.edu

Scott E. Yonker
Cornell University
sey8@cornell.edu

Current Version: July 2021

Abstract
We provide empirical evidence that visceral factors affect financial risk taking by showing that exposure to mass shootings alters mutual fund managers' risk taking decisions. Funds that are exposed to mass shootings subsequently decrease risk relative to their peers. The effect that we document is temporary, lasting approximately one quarter before reverting to normal levels and is strongest among managers with demographics shown to express greater fear from mass shootings. Together with laboratory studies that show that market downturns induce fear, our findings suggest that fear could exacerbate variation in risk taking, generating the highly volatile countercyclical risk premiums shown to exist in markets.

Key words: risk taking, fear, trauma, psychological shocks, mass shootings, mutual fund managers

JEL classification: G11; G40; G41; D90; D91

Bogan: SC Johnson College of Business, 201K Warren Hall, Cornell University, Ithaca, NY 14853. E-mail: vlb23@cornell.edu.
Yonker: SC Johnson College of Business, 201J Warren Hall, Cornell University, Ithaca, NY 14853. E-mail: sey8@cornell.edu.
All errors are our own. We thank Jawad Addoum, Bob Connolly, Will Cong, Byoung Hwang, Andrew Karolyi, Justin Murfin,Ville Rantala, Noah Stoffman and seminar participants at Cornell University and the University of Miami for useful comments. We thank Max Gardner for excellent research assistance.

"Be fearful when others are greedy, and greedy when others are fearful."

- Warren Buffett - business man and investor

1. Introduction

Rational agent models struggle to explain a number of empirical regularities found in asset markets. High volatility in asset prices (Shiller, 1981; Grossman and Shiller, 1981), the large equity premium (Mehra and Prescott, 1985), and the countercyclical nature of expected risk premiums (Fama and French, 1989) cannot be adequately explained using standard asset pricing models. Theory indicates that systematic fluctuations in investors' willingness to take risk could be the key to understanding these patterns. Asset pricing models that predict these regularities have relied on time varying risk aversion that is inversely related with consumption (Campbell and Cochrane, 1999) or recent market returns (Barberis et al., 2001) or have employed ambiguity aversion that peaks during recessions due to increased pessimism (Ju and Miao, 2012). As a result, empiricists have become interested in testing whether investors' risk taking behavior varies with the business cycle and identifying the channels through which such variation is generated.

Recent empirical work documents evidence consistent with countercyclical financial risk taking, finding that investors became more conservative following the 2007-08 financial crisis (Guiso et al., 2018; Dohmen et al., 2016; Gerrans et al., 2015; Necker and Ziegelmeyer, 2016). In this setting, however, it is difficult to identify the channel causing investors to reduce risk. Risk aversion may increase after financial crises from decreased investor wealth, but investors also may learn about the distribution of returns from crises, leading them to pessimistically update their expectations of future returns and risk. Moreover, market downturns could evoke a visceral response, leading to temporary changes in investors' risk aversion, in their beliefs about the distribution of expected returns, or in their understanding of the distribution. While the evidence suggests that decreases in wealth can partially explain why investors reduced risk following the financial crisis, negative emotions evoked by the crisis itself also seemed to play an important role (Guiso et al., 2018; Necker and Ziegelmeyer, 2016).[1]

Utility can be modeled as being state dependent on negative emotions or "visceral factors" (Loewenstein, 2000). Visceral factors include a wide range of negative emotions, such as fear, anger, and hunger and are moderated by cognitive deliberations. Decisions under risk and uncertainty are one domain under which visceral factors are believed to be important (Loewenstein, 2000) and they have been shown to be salient in lab experiments (Kuhnen and Knutson, 2005, 2011). Due to the fleeting nature of negative emotions, they are a potential source of variability in risk taking behavior that could ultimately lead to volatility in asset prices.

To estimate the importance of visceral responses to market movements on risk taking, Cohn et al. (2015) conduct experiments that isolate these emotional effects. Subjects in real stakes games who were primed with market bust scenarios subsequently made less risky allocations than those who were primed with market booms. Furthermore, those primed with busts reported higher levels of the emotion *fear*, suggesting that fear could be the cause of the change in risk taking. Other laboratory experiments that generate fear through horror films (Guiso et al., 2018) and electric shocks (Cohn et al., 2015) provide direct support for the idea that fear, a visceral factor, reduces risk taking.

Yet, a lacuna in the literature exists as to whether visceral factors, like fear, have a statistically and economically significant effect on the financial risk taking of *actual* investors operating in *actual* financial markets. Wang and Young (2020) take an important first step, showing that terrorist attacks are correlated with aggregate mutual fund flows. Therefore, we seek to estimate the causal effects of visceral factors on financial risk taking in a real world setting. We exploit randomly occurring, traumatic events to explore whether financial risk taking decisions are affected by investors' negative emotions. Specifically, we analyze the risk taking decisions of managers of actively managed U.S. domestic equity mutual funds exposed to mass shootings, utilizing their proximity to these events as treatments of fear.

[1] Guiso et al. (2018) report that even bank customers who experienced no decline in wealth due to the financial crisis reported large increases in risk aversion. Necker and Ziegelmeyer (2016) come to this conclusion because the change in wealth is deemed too small to generate the sizable change in risk aversion.

We focus on mass shootings as a proxy for fear for five reasons. First, mass shootings have been shown to induce fear in individuals in nearby communities.[2] We discuss this literature in detail in Section 4. Second, mass shootings are frequent. Mass shootings have unfortunately become an epidemic in the United States. From January of 1999 through June of 2016, the period we study, there were over 250 mass shootings. Third, they are random. By definition, the mass shootings that we study are unrelated to gangs, drugs, or organized crime and often occur in areas with low crime rates and no prior history of violence (Lowe and Galea, 2017). Fourth, they are unconstrained by geography. There is at least one event in 39 of the lower 48 states during our sample period. This feature mitigates concerns that the treatment is related to risk preferences that cause investors to select into areas that are more prone to traumatic events. Finally, mass shootings and their effects are uncorrelated with both macroeconomic and local economic conditions. In other words, the response elicited by mass shootings should be purely emotional and unrelated to managers' wealth, property values, or future labor income.

We choose mutual funds managers as our subjects for this natural experiment because the context in which they operate is ideally suited for our analysis. First, the financial risk taking decisions of professional mutual fund managers are observable and measurable over long periods of time. Both holdings and return information is available through regulatory filings and standard databases. Second, heterogeneity in backgrounds, financial literacy, and skill sets is likely much smaller among these professional investors than in the general population, producing a more homogeneous sample of subjects. Third, mutual funds have clearly stated investment objectives and styles, which is advantageous since this allows us to compare the risk taking of managers to others who are trying to achieve similar goals and have similar constraints. Finally, behavioral biases of mutual fund managers tend to be much smaller than those of individual investors, so finding positive evidence of the effect of "fear" on risk taking among these managers will serve as a conservative estimate of the effect of fear in the general investing population.[3]

[2]While mass shootings have been shown to evoke several other negative emotional responses, like anxiety and depression, for simplicity in exposition we use the term "fear" to indicate the negative visceral response elicited by mass shootings, recognizing that it is impossible to isolate a particular emotion outside of a laboratory setting.

[3]More experienced investors make fewer behavioral mistakes (List, 2003, 2004). In the context of mutual funds, the local bias in stock holdings exhibited by mutual managers documented in Coval and Moskowitz (1999) is much smaller than that exhibited by individual investors as documented in Ivković and Weisbenner (2005). Contrary to these

A potential challenge to our identification strategy is that delegated portfolio managers are charged with acting in the best interest of their investors. While this fiduciary duty along with governance mechanisms should prevent managers from imprinting their own preferences on their delegated portfolios, there is ample evidence that managers often do so despite these safeguards.[4]

We conduct our tests using a difference in difference testing framework, pooling over 200 mass shooting events from 1999 to 2016. Exposure to fear is based on the proximity of mutual fund advisers to mass shooting locations. While some events affect all managers, we assume that those who live closer to a mass shooting location will experience stronger negative emotions. Our results are consistent with this notion.

We document robust evidence that is consistent with "fear" inducing temporary reductions in professional investors' financial risk taking. Managers reduce their risk for approximately 60 trading days (about one fiscal quarter) following exposure to high fatality mass shooting events, after which their risk levels revert to normal. The magnitude of this effect is stronger for funds located closer to shooting locations. Consistent with the emotion of fear driving these findings, funds run by managers who are more susceptible to fear of mass shootings respond more dramatically (Vuori et al., 2013; Lowe and Galea, 2017). The reductions in risk are driven by reduced systematic risk. Exposed mutual fund managers decrease their systematic risk by moving into stocks with lower exposures to the market risk factor.

We provide the first direct empirical evidence that visceral factors affect financial risk taking in actual markets. The ephemeral nature of the effect is important, since it suggests that visceral factors are a source variation in investor risk taking, and potentially in asset prices. In conjunction with the literature showing that mass shootings induce fear (Addington, 2003; Hawdon et al., 2014; Kaminski et al., 2010; Shultz et al., 2014), these findings add to the detailed work of Guiso et al. (2018) and Cohn et al. (2015) who identified fear as a candidate to generate countercyclical dynamics in risk aversion. When combining the temporary effect that we document with the

studies, Haigh and List (2005) document greater behavioral biases in professional CBOE traders than college students in a lab setting. We note, however, that CBOE traders and mutual managers have very different incentives and training.

[4]We discuss this literature in detail below. See for example, Chevalier and Ellison (1997, 1999).

finding that market downturns evoke fear (Cohn et al., 2015), systematic changes in investors' emotional states could exacerbate countercyclical changes in risk taking, adding volatility to expected market risk premia. This is consistent with known empirical regularities in asset prices (Mehra and Prescott, 1985; Fama and French, 1989; Shiller, 1981; Grossman and Shiller, 1981) and also with the assumption of countercyclical risk taking of several prominent asset pricing models (Campbell and Cochrane, 1999; Barberis et al., 2001; Ju and Miao, 2012). Showing that visceral factors can feasibly cause these dynamics in actual markets is a main contribution of the paper.

2. Related literature

2.1. Behavioral economics and risk taking

Our findings also connect to the behavioral economics literature on emotions and nonstandard decision-making (DellaVigna, 2009). A number of studies indirectly link investors' emotional states with their risk taking decisions by documenting that investors' moods and emotions are related to stock market fluctuations. The implicit assumption within these studies is that negative emotions reduce aggregate risk taking, which drives down stock prices. Stock indexes have lower returns on cloudy days (Saunders, 1993; Hirshleifer and Shumway, 2003), when humans systematically get less sleep (Kamstra et al., 2000) and during times of reduced daylight (Kamstra et al., 2003). Further, Edmans et al. (2007) find that returns on country-level stock indexes are lower the day after a country's national soccer team is eliminated from the World Cup. We complement these studies by providing the first *direct* empirical evidence that negative emotional states affect financial risk taking decisions, validating the mechanism proposed in this literature.

In the domain of nonstandard risk taking decisions, scholars document life-cycle effects, showing that individuals become less willing to take risks as they grow older (Paulsen et al., 2011; Levin et al., 2007). Emerging literature has documented that individuals' past experiences have long-lasting effects on risk attitudes. Individuals who experience lower stock market returns during their lifetimes take less financial risk (Malmendier and Nagel, 2011) and firms run by CEOs who grew up during the Great Depression have more conservative financial policies (Malmendier et al., 2011), while those run by CEOs who experience natural disasters early in life have riskier policies (Bernile et al., 2017). Personal traumatic experiences also can have long-lasting effects on risk taking. Bogan et al. (2013) find that combat experience in veterans decreases their probability of investing in risky assets. Additionally, inflation experiences have been shown to matter for financial decisions. Malmendier and Nagel (2016) demonstrate that past experience with inflation influences inflation expectations and ultimately mortgage decisions. Their findings are consistent with the assertion that past experiences have long-lasting effects on the formation of nonstandard beliefs and expectations. Complementary to this work, we present findings of current, non-financial, traumatic experiences having *temporary* effects on professional financial risk taking.[5]

More generally, our findings also add to the large literature in economics that challenges the Stigler and Becker (1977) notion of individual preferences being stable through time (Schildberg-Hörisch, 2018). Evidence of risk preference instability has key implications for a number of areas within economics, as individual risk taking behavior can influence health outcomes (Anderson and Mellor, 2008), labor market choices (Bonin et al., 2007; Hsieh et al., 2017), and financial decisions (Epstein and Zin, 1990), for example. Understanding factors that evoke changes in individual risk preferences over time is vital for better understanding of both individual decision-making and market behavior.

Within this vast literature our study compares most directly with recent survey-based studies that use exogenous shocks to understand how and for what duration these events affect agents' risk attitudes. This literature has used shocks based on natural disasters (Cameron and Shah, 2015), violence (Voors et al., 2012; Moya, 2018; Brown et al., 2019), and war (Callen et al., 2014). The majority of these studies show that exposure to traumatic shocks leads to reduced risk tolerance (Callen et al., 2014; Cameron and Shah, 2015; Moya, 2018; Brown et al., 2019), with Voors et al. (2012) the exception. The aforementioned studies, that rely on surveys, have the benefit of precisely identifying the mechanism through which risk taking changes (i.e. preferences, beliefs,

[5] Bernile et al. (2018) also study mutual fund managers, estimating their risk taking responses to natural disasters. They find results consistent with ours; exposed managers reduce risk through reductions in systematic risk, but the effect that they document is much more persistent, which is consistent with life experiences altering beliefs about future returns and risk (Malmendier and Nagel, 2011; Bernile et al., 2017) and is inconsistent with fleeting nature of visceral factors.

ambiguity), but are limited by the potential relevance of the subjects that are recruited. Critics of these survey-based studies, argue that using lottery type games or questions to measure risk aversion in developing countries may not generate findings that have external validity in a real world investing context (Chuang and Schechter, 2015; Vieider, 2018).

While our study is unable to pin down the precise mechanism that drives changes in risk taking, we focus on highly relevant market participants to address any external validity critique and exploit a natural experiment to present empirical evidence of risk taking behavior that is consistent with the nonstandard preferences (risk preferences conditional on emotional states à la Loewenstein (2000) and Loewenstein et al. (2001)) documented in this literature.

2.2. Mutual fund management

Finally, our study connects to the literature that illustrates how the personal incentives, characteristics, and backgrounds of mutual fund managers affect their professional decisions. Career concerns (Chevalier and Ellison, 1997, 1999), changes in personal wealth (Pool et al., 2019), family deaths (Shu et al., 2016), political ideology (Hong and Kostovetsky, 2012), familiarity (Pool et al., 2012), neighboring managers (Hong et al., 2005; Pool et al., 2015), and exposure to catastrophic weather events (Alok et al., 2020; Bernile et al., 2018) have been shown to influence mutual fund managers' portfolio and risk taking decisions. Finding that personal exposure to mass shootings affects fund manager risk taking contributes to this literature by providing strong causal evidence that is supportive of earlier findings.

3. Data and sample construction

3.1. Data sources

To explore the effect of fear on mutual fund manager risk taking, we construct a data set that merges data from a number of different sources. Below we describe the sources of these data.

3.1.1. *Mutual fund data*

We use two primary sources of data to construct the sample of active U.S. domestic equity mutual funds: the Center for Research on Security Prices (CRSP) Mutual Fund Database and Morningstar Direct. Share class-level information on daily returns, total net assets (TNA), annual expense ratios, turnover, and Lipper fund styles come from the CRSP database. Share class groupings and fund holdings data come from Morningstar Direct. We utilize data from the first quarter of 1999 through the fourth quarter of 2016.

3.1.2. *Mass shooting data*

Data on mass shootings are from the Stanford Mass Shootings in America database (SMSA, hereafter).[6] Using online media resources, the database was constructed by researchers at the Stanford Geospatial Center at Stanford University in the aftermath of the Sandy Hook Elementary School shooting. Mass shootings are defined as those having at least three shooting victims (not necessarily fatalities) that are unrelated to gangs, drugs, or organized crime.[7] The database includes 336 mass shooting events beginning with the University of Texas - Austin shooting in 1966 with 48 victims and 16 casualties and ending in June of 2016 with the Pulse Nightclub shooting in Orlando that had over 100 victims with 50 dead.

The database is extensive, including information on event dates, severity of the incidents (numbers of victims and fatalities), locations, location types (i.e. primary school, entertainment venue), shooter demographics, and types fire arms used. Most important to our study are the dates, locations, and number of victims and fatalities. One known drawback of the SMSA database is that location coordinates can be imprecise. Rather, many locations are coordinates for the cities of the shootings. We verify the general accuracy of the coordinates by comparing the cities and states in the database with those returned through a reverse geocode lookup function that uses the Google Geocoding API.

[6]The data are publicly available at `https://github.com/StanfordGeospatialCenter/MSA`.

[7]More information on the data is found `https://library.stanford.edu/projects/mass-shootings-america`.

We also cross check locations with another mass shooting database constructed and maintained by the online journalism site *Mother Jones*. The *Mother Jones* database covers fewer events, covering only events with four or more fatalities from 1982 through 2012 and events with three or more fatalities thereafter. We are able to match all but one of the 55 events covered by the *Mother Jones* database during our sample period and confirm the accuracy of both databases. Location implied zip codes match exactly in 50% of the cases. For the remaining cases, the median distance between event zip codes associated with the two databases is 3.6 miles, with a maximum distance of 13.6 miles.[8] In addition to using the *Mother Jones* database to validate the accuracy of the SMSA database, we also use it to confirm the robustness of the results.

3.1.3. Other data sources

Daily factor returns are from Ken French's website.[9] Beta Suite by WRDS is used to calculate factor loadings for individual stock holdings. Adviser location information is from each mutual fund's most recent semi-annual Form N-SAR filing. The NBER ZIP code distance database is used to calculate distances between mass shooting event locations and mutual fund adviser ZIP codes.[10] ZIP code level demographic data are from the 2000 U.S. Census. We infer gender of mutual fund managers using the R package "gender," which uses data from the Social Security Administration's birth files to determine gender based on first name. When middle names are available, those are also used to help determine gender. The algorithm has been shown to be 97% accurate, but often cannot determine the gender of foreign names. The algorithm is able to infer gender for about 97% of the managers in the sample. Details are described in Blevins and Mullen (2015).

3.2. Sample construction

The sample construction follows four basic steps. First, we identify the mass shooting events to be included in the analysis. Second, we identify the broad quarterly candidate sample of

[8]Information on this database is found at `https://www.motherjones.com/politics/2012/12/mass-shootings-mother-jones-full-data/`

[9]The data are found at `https://mba.tuck.dartmouth.edu/pages/faculty/ken.french/data_library.html`.

[10]These data are found `https://data.nber.org/data/zip-code-distance-database.html`.

mutual funds from which to populate the analysis of each event. Third, we select mutual funds from the candidate sample during each event quarter and identify the treatment and control groups. Fourth, we pool all event-fund observations to create the final sample used in the analysis. We discuss each of these steps in detail below.

3.2.1. Mass shooting events

Data on daily mutual fund returns and quarterly TNA, which are necessary for the construction of our risk measures and controls, is available beginning in September 1998. The source of the shooting data, the SMSA database, was permanently suspended in June of 2016. Therefore the sample includes events beginning in the first quarter of 1999 running through the second quarter of 2016. While other databases that track mass shootings still exist, we focus on the SMSA database to maintain consistency across all events in the data definitions and collection methodology. There are a total of 261 mass shootings in the database during this time period, however, we identify 7 events that are listed more than once. After dropping duplicates there are 254 mass shooting events during the 17.5 year period from January 1999 through June of 2016.

We next identify which of these events are helpful in identifying the effect of fear on mutual fund manager behavior. Our measure of fear is based on the proximity of mutual fund advisers to mass shooting events. Therefore, we input the latitude and longitude coordinates provided by the SMSA database into reverse geocoding functions to obtain the ZIP codes where the mass shootings took place. Using the NBER ZIP code distance database, we then calculate distances between event ZIP codes and the ZIP codes of mutual fund adviser locations for each active U.S. domestic equity fund in existence during event quarters. We keep events that have at least one fund adviser located within 100 miles of the shooting. There is at least one such fund for 210 of the 254 possible events (83%).

Figure 1 plots the geographic distribution of the 210 mass shooting events. The size of the circles indicates the severity of the shooting by the number of fatalities involved. As we show later, mutual managers only respond to more severe events, therefore our main analysis focuses on events with above the median number of fatalities (more than 3). There are 84 such events.

These events are indicated in black, while those with three or fewer are indicated in gray. The map shows that mass shootings in the U.S. are widespread. There is at least one shooting in 39 of the lower 48 states. Twenty-nine states have had at least one high fatality shooting (those with four or more fatalities). California has had the most shootings with 20, followed by Florida (18), and Texas (16).

Figure 2 shows the number of mass shootings by year included in the sample, split by median fatality. Events are much more frequent during the final four and a half years of the sample (1Q2012–2Q2016). Of the 210 events, 165 (79%) occur during this period. The disparity from the earlier part of the sample is not as great for high fatality events, but is still substantial. Fifty-four of the 84 (64%) high fatality mass shootings occur during this period.

Figure 3 shows the distribution of high fatality mass shootings by location type. Most mass shootings occur in a residential home or neighborhood (42%), followed by retail, wholesale, or services facilities (11.9%), the shooter's place of work (9.5%), and government or military facilities (7.1%). It is notable that if we were to combine the educational facility categories (Primary/secondary school, College/university), they would tie with work as the third most common location type of high fatality mass shootings.

3.2.2. Quarterly candidate mutual fund sample

For each event we draw our sample of funds from actively managed U.S. domestics equity funds in existence during the quarter of each event. We therefore begin by constructing a quarterly panel of these "candidate funds" from which to draw our sample. To do so, we follow a procedure similar to Pástor et al. (2015) whereby we merge all active and inactive share classes of funds in Morningstar Direct with those in the CRSP Mutual Fund database.

We start with all equity mutual fund share classes included in the CRSP quarterly summary files beginning in the first quarter of 1999 until the fourth quarter of 2016. We exclude ETFs and variables annuities. We then match these share classes by ticker and CUSIP to active and inactive share classes in Morninstar Direct. We are able to match about 89% of the quarterly equity fund share class observations listed in CRSP. Next, share classes of index funds are removed by dropping those of funds that are categorized as an index fund by either CRSP or Morningstar Direct. We also drop share classes of funds with the word "index" in the fund name. Allocation funds and international equity funds are then dropped. We limit funds to those in twelve highly populated Lipper U.S. domestic style classes. These styles are the cross product of the size styles; Large-Cap, Mid-Cap, Small-Cap, Multi-Cap, and the value/growth styles of; Value, Growth, and Core. We do this to insure adequate observations within each event-style cluster because our analysis is conducted within event-style. We drop funds for which ZIP code information is missing. Then we remove funds that are missing lagged control variables and extremely small funds, those with lagged TNA below the fifth percentile of the candidate sample during the quarter.

The sample of candidate funds that is generated from this process averages about 1,575 active domestic equity funds per quarter during the sample period. Table A.1 in the Appendix details the construction of the candidate sample and Figure A.1 shows the number of candidate funds per quarter.

3.2.3. Pooled event sample

For each event, we identify treated and control funds and pool the event-fund observations to create our pooled event sample. The initial sample of funds for an event is the sample of candidate mutual funds during the quarter of the event. We initially categorize funds located within 100 miles of the event as "treated" and all other funds are initially categorized as "control" funds. Table 1 shows the number of funds located within 100 and 50 miles from the ten most deadly mass shootings during the sample period.

Since the analysis is conducted within event-style, we remove all control funds in styles for which there exists no treated funds. Panel A of Table 2 shows that nine of the possible 12 styles are included for the median event. Following this pooled event construction yields an initial sample with 221,704 fund-event observations. To avoid cross contamination of events, we next remove from the control group any fund that is in the treatment group of another event during the same quarter.[11] About 67% of the quarters contain multiple events. Therefore this filter is substantial.

[11]This filter is applied using all events regardless of the number of fatalities.

It reduces the sample by 30% to 156,363 fund-event observations. Finally, to keep the sample consistent across risk measures we keep observations with non-missing changes in our four main measures of risk taking. This leaves us with 146,778 fund-event observations. Our final pooled event sample includes 210 events, 1,707 event styles, and averages about 700 funds per event or 85 funds per event style. The sample of high fatality events includes 84 events, 729 event styles, and averages about 890 funds per event or just over 100 funds per event style.

3.3. Variable construction

3.3.1. Treatment variables

Mutual funds are treated based on their proximity to mass shooting events. We calculate distances between funds and event locations using ZIP codes of funds' advisers and event locations. We calculate these distances only for ZIP codes that are within 100 miles of each other.

Table 3 displays summary statistics for the high fatality sample, which is the focus of the analysis. It indicates there are 3,690 funds that are within 100 miles of a mass shooting event or about 5% of the sample. About 2.5% of the sample is located within 50 miles of a mass shooting event. Our main specifications use indicator variables based on proximity to the mass shooting as treatment variables. We use several different cutoffs. In some specifications, we include only one treatment variable, I(MS dist. ≤ 100) or I(MS dist. ≤ 50), that indicates that the fund is located within 100 or 50 miles of the mass shooting. In other specifications, we use several indicator variables or other measures to allow for heterogeneous treatment effects based on proximity to the event.

3.3.2. Dependent variables

The dependent variables of interest are changes in various measures of fund risk including: total volatility, systematic risk, idiosyncratic risk, and tracking error. We estimate the change in these variables from the pre-event period to the post-event period using daily mutual fund return data. Mutual fund returns are reported at the share class-level, so for funds with multiple share classes, we share class-weight the returns each day based on the prior month-end TNA to create fund-level returns.

In our primary analysis, we estimate the levels of risk in the pre-event and post event period using 30 trading days of data. Specifically, pre-event risk is estimated over trading day -30 to -1 and post-event risk is estimated from trading day 1 to 30, where time 0 is the event date. Total volatility is measured as the standard deviation of the fund's excess daily returns. Systematic risk measures are the estimated factor loadings from regressions of the excess daily fund returns on daily factor returns. Idiosyncratic risk is the standard deviation of the residuals from the factor regressions. Tracking error is the standard deviation of the mutual fund returns in excess of the value-weighted market portfolio.

Volatility measures tend to be highly skewed and so, for these measures, we focus on the natural logarithm of these values. Thus, our dependent variables of interest are the changes in the natural logarithm of volatility measures from the pre-event to post-event periods. Systematic risk measures are linear and can take negative values (especially when including zero-cost factors), and so, for these measures, we simply use the change in factor loadings. In both cases, we trim the changes in risk at the 1st and 99th percentiles to mitigate the effect of outliers on our estimates. Table 3 shows that the mean and median market beta are around one, but the market beta exhibits substantial variation with a standard deviation of 0.19. On average, volatility falls by about 2% over our event periods. Any time trend will be differenced out by our specification.

In the spirit of Huang et al. (2011), we also construct a series of holding-based measures of risk. Specifically, we estimate factor loadings for individual stocks using 36 months of monthly excess return data up until the last month end that is prior to the fund's last quarterly holding report before the event. We then use these estimated factor loadings to calculate portfolio betas based on both the pre-event holdings and post-event holdings. In other words, factor loadings are not allowed to change based on changing conditions over the event period. They are estimated only using data prior to the event. Formally we estimate,

$$\Delta\hat{\beta}^h_{i,t} = \sum_{j=1}^{N} w_{i,j,t}\hat{\beta}_{j,t-1} - \sum_{k=1}^{M} w_{i,k,t-1}\hat{\beta}_{k,t-1} \,, \tag{1}$$

where $w_{i,j,t}$ is the weight of stock j in fund i in the fund's first holding report following the event, $w_{i,k,t-1}$ is the weight of stock k in fund i's last quarterly report prior to the event, $\hat{\beta}_{j,t-1}$ and $\hat{\beta}_{k,t-1}$ are the estimates of stock j and k's factor loading using 36 monthly excess returns up until the month end prior to fund i's last quarterly filing prior to the event. Fund i holds N stocks in period t and M in period $t-1$. Note that for funds that hold the same subset of stocks in $t-1$ and t, this equation simplifies to sum of the change in weights times the factor loadings estimated at time $t-1$. In other words, it captures how funds actively change their risk exposures.[12]

Table 3 shows that the mean and median holdings-based market beta is higher than the estimated market beta. This is because weights in the calculation of holdings-based betas are a percentage of the total equity holding portfolio. Hence holdings such as cash, which bring the total market exposure down, are not reflected in this measure. It is also noteworthy that changes in holdings-based systematic risk measures have much lower variation. The standard deviation of the change in the holdings-based measure of systematic risk is about 40% of that of the change in systematic risk estimated from the time series.

We also investigate dynamics of fund flows around events. Data on TNA are available monthly. Therefore monthly fund flows from $t-1$ to t for fund i with J share classes is calculated as:

$$F_{i,t} = \frac{\sum_{j=1}^{J}\{TNA_{i,j,t} - TNA_{i,j,t-1}(1+r_{i,j,t})\}}{\sum_{j=1}^{J} TNA_{i,j,t-1}} \times 100. \tag{2}$$

$TNA_{i,j,t}$ is the total net assets of share class j in fund i at the end of month t and $r_{i,j,t}$ is the return of share class j of fund i from $t-1$ to t. The numerator is the aggregated dollar dominated flows to the fund, while the denominator is the total beginning net assets of the fund. Therefore, flows are in percent.

[12]This measure is not the same measure used in Huang et al. (2011). They use weights based on holdings during times t and $t-1$ to calculate portfolio return series over the common pre-event period. They then estimate risk measures from these portfolio-based return series and take differences in the estimates. Our methodology is only applicable for linear factors.

3.3.3. Control variables

Fund styles are defined by the Lipper class listed in CRSP. As discussed earlier, these are limited to twelve highly populated style categories. Some models also control for the lagged natural logarithm of TNA (lag ln(TNA)), where the lagged value is the quarter end value prior to the event. For funds with multiple share classes, fund TNA is the sum of the TNA share class values reported in CRSP aggregated across funds with common "FundId" in the Morningstar Direct database. Fund age is determined by the Fund's oldest share class, which is identified as the minimum of the inception dates reported in CRSP and Morningstar. For funds with multiple share classes, expense ratios and turnover ratios are share class-weighted averages of these variables reported in CRSP. Table 3 indicates that the average fund in the sample has TNA of about $1.5 billion and an expense ratio of about 1.2% per year, while the median fund is much smaller at about $250 million in TNA.

4. Empirical methodology

In order to analyze the casual effect of fear-inducing events on mutual fund managers, we use a difference in difference testing framework to estimate the average treatment effect of fear on changes in risk taking. Our proxy for fear is exposure to mass shootings. Similar to Edmans et al. (2007), we identify three specific characteristics that are important to satisfy in order for a fear proxy to be used to study this link with risk taking. First, the proxy must induce fear in exposed populations. Second, to reinforce generalizability, evidence that the proxy causes fear in multiple different populations and situations is preferable. Finally, the amount of fear must be substantial enough to cause a reaction from investors.

There is a substantial literature in psychology, criminology, and psychiatry that supports the first two of these characteristics, and thus serves to justify our choice of proxy. Mass shootings have been shown to induce fear in individuals in nearby communities. For instance, hospital workers reported increased fear following the 2011 mass shooting in Tuscon, Arizona that infamously victimized Congresswomen Gabby Giffords (Shultz et al., 2014). Additionally, Kaminski et al. (2010) show that mass school shootings increase various measures of fear in college students. Addington (2003) documents that exposure to mass shootings increases fear in adolescents. Hawdon et al. (2014) present evidence that fear from mass shootings leads individuals (age 18 to 74) to withdraw from community life. The literature documents that fear evoked by mass shootings tends to be strongest among women and in younger individuals (Vuori et al., 2013; Lowe and Galea, 2017). Scholars also find that fear from exposure to mass shootings increases with the degree of physical exposure and social proximity to the incident (Shultz et al., 2014).[13] The third criteria is an empirical issue, validated by our subsequent analysis. However, since our identification strategy exploits heterogeneity at the mutual fund level, unlike Edmans et al. (2007), it is unnecessary for our events to evoke an emotional response by the majority of market participants, just a sufficient number of market participants.

We measure exposure to fear based on the proximity of mutual fund advisers to mass shooting locations. While some events may induce negative emotions among all managers, we assume that those who live closer to a mass shooting location will experience greater levels of fear. We construct risk taking measures from both daily return data and holding data. Our main tests focus on realized total daily volatility as measured by the standard deviation of daily excess returns. For every fund in each event, we construct this measure using 30 trading days prior to the event and 30 days after the event. We then test, whether proximity to mass shootings is related to changes in risk taking over the event period.

For our main specification, we estimate the following regression equation:

$$\Delta \ln(\sigma_{i,s,k}) = \beta Exposure_{i,k} + \gamma^{\intercal}\mathbf{x}_i + \delta_{s,k} + \epsilon_{i,k}. \tag{3}$$

In Equation 3, $\Delta \ln(\sigma_{i,s,k})$ is the change in risk-taking of fund i in style category s, over the event period for event k. $Exposure_{i,k}$ is the treatment variable that is either an indicator or a continuous measure of the exposure of fund i's managers to event k. Therefore, β measures the

[13] Of course, fear is not the only emotional response elicited by mass shootings. Shooting victims are often afflicted with post traumatic stress disorder (PTSD), which can cause fear, anxiety, and depression (Lowe and Galea, 2017). See Lowe and Galea (2017) for a review of mental health effects of mass shootings and Muschert (2007) for the effects of school shootings.

average treatment effect of fear on fund risk-taking. Importantly, the regression includes style by event fixed effects, $\delta_{s,k}$, which control for heterogeneous changes in risk taking by fund style over each of the event periods. Therefore, the treatment effect is estimated relative to funds in the same style category over the same period of time. Regressions also include a vector of lagged fund-level control variables ($\mathbf{x}_i$), but, as we show later, they are not important for our estimates of β due to the random nature of the treatment.

Our identifying assumption is that exposures to mass shootings is randomly assigned to funds within styles through time. In other words, exposure to mass shootings is uncorrelated with other determinants of changes in fund risk taking within funds styles for any given event. Later we provide evidence of this, at least for observable factors.

Abadie et al. (2017) argue that clustering standard errors should be done to address sampling and experimental design issues. We cluster standard errors by event to address any sampling design issues. We also cluster by adviser ZIP codes to address an experimental design issue, which is that the treatment is correlated for advisers located in the same ZIP code since funds' proximities to mass shooting events determine the treatment.

One key feature of the difference in difference design is that it underestimates the treatment effect for events that are nationally traumatic in nature. Thus, any results could be considered a lower bound of the effect of mass shootings on risk taking.

When estimating the dynamics of the effect of fear on risk taking we use log levels of risk as the dependent variable and focus on the interaction terms of our fear proxy and elapsed time since the event. Formally, we estimate:

$$\ln(\sigma_{i,s,k,t}) = \sum_{j=1}^{T} \beta_j \{I(t=j) \times Exposure_{i,k}\} + \gamma^{\intercal}\mathbf{x}_i + \delta_{s,k,t} + \psi_{i,k} + \epsilon_{i,k,t}. \tag{4}$$

Equation 4 is a dynamic version of Equation 3. The t subscript indicate the time period since the pre-event period ($t = 0$). When $T = 1$, this regression model is the level form of Equation 3 and will yield $\hat{\beta}_1$ equal to $\hat{\beta}$ from Equation 3 when no lagged control variables ($\mathbf{x}_i$) are included.[14]

[14]Due to the trimming of the dependent variable, our estimate of β in Equation 3 provides a more conservative estimate of the treatment effect than the estimate of β_1 from Equation 3 when $T = 1$.

This is because the model includes fund by event fixed effects, $\psi_{i,k}$, and also style by event by time fixed effects ($\delta_{s,k,t}$). In the case when $T > 1$, estimates of the β_j's capture the change in risk taking since the event period. The risk-taking measures are calculated over 30 trading day intervals, so for $t = 0$ the risk taking measures are estimated using daily returns from -30 to -1 trading days prior to the event, for $t = 1$ trading days 1 to 30 after the event are used, and for $t = 2$ trading days 31 to 60 are used, etc. Therefore, risk is measured in approximately half quarter intervals (the average quarter has 62 trading days).

5. Results

5.1. Results by event severity

We hypothesize that the treatment of fear will be greater when managers are closer to events and when events are more severe. We therefore begin our analysis conducting our tests along the dimensions of severity and proximity. However, we have very little guidance on how severe a shooting must be to evoke fear in surrounding communities and just how close a person must be to an event in order for it to generate a differential effect on risk taking.

We initially estimate Equation 3 using all sample events regardless of their severity using two different treatment variables based mutual fund proximity to mass shootings; 100 and 50 miles from the shooting.[15] We then split the sample between low and high fatality events, where high fatality events are those with greater than the median number of fatalities. The change in the natural logarithm of total realized fund risk is the dependent variable.

Table 4 displays the regression results. For the full sample of events, the coefficient estimates on the treatment variables are negative, but neither are statistically different from zero. When splitting the sample between low and high fatality events, we see that exposure to low fatality events has no differential effect on risk taking among mutual fund managers, but exposure to high fatality events does. The coefficient estimates on mass shooting exposure, displayed in columns 5 and 6, indicate that greater exposure to high fatality mass shooting events leads to reduced risk

[15] Coval and Moskowitz (1999) uses 100 kilometers (62.1 miles) to define local equities relative to mutual fund managers.

taking. The average treatment effect estimated in column 6 indicates that funds that are within 50 miles of a mass shooting experience changes in risk taking that are 0.6% lower than peers within the same style category.

Using these findings to guide the rest of the analysis, we focus on the 84 high fatality events and define the baseline treatment variable as being located within 50 miles of the event. In section 5.3, we also show how treatment effects vary with distance to the event.

5.2. Types of risk

We next test what type of risk is affected. We decompose total risk into two components; systematic risk and idiosyncratic risk. As discussed before, we measure systematic risk by estimating market betas from regressing daily excess fund returns on daily excess market returns. Idiosyncratic risk is the standard deviation of the residual terms from those regressions. We also test whether fear pushes managers to herd toward the market. For this purpose, we test whether exposed funds reduce tracking error.

The results in Table 5 indicate that fund mangers react to fear by reducing systematic risk. The market betas of funds within 50 miles of a shooting event fall by 0.006 compared to similar funds that are not exposed. This indicates a 0.6% decrease for the median fund, which has a market beta of 1.01. This magnitude is in line with the decrease in total risk and is about 5% of a standard deviation of changes in market beta. Neither the idiosyncratic risk nor tracking error of mutual funds are affected by fear. We therefore focus on total risk and systematic risk in the subsequent analysis.

5.3. Effect of distance

As discussed previously, we expect that closer proximity to mass shooting events evokes greater fear and potentially a greater effect on risk taking. We test this by estimating Equation 3 with two different treatment specifications that allow for heterogeneous treatment effects. The first includes multiple treatment indicator variables. We split the mass of the 100 mile indicator evenly among quartiles based on distance, so that about 1.25% of the sample (5.0%/4) falls into each indicator. We end up with cutoff points of 11 miles (25th percentile), 43 miles (median), and 80 miles (75th percentile). We then define indicators based on these quartiles. For example, funds within 11 miles of the event are in quartile 1, while those between 80 and 100 miles from the event are in quartile 4. In this specification, the effects should be strongest for quartile 1 and the weakest for quartile 4.

In the second specification, we include both the 100 mile indicator variable and the interaction of this variable with the natural logarithm of one plus the distance from the fund to the mass shooting. Because the continuous distance variable is only constructed for funds within 100 miles of the mass shooting, the level of this variable on its own is superfluous. The indicator variable itself captures the effect for funds located in the same ZIP code as the mass shooting. Therefore, it should be negative. If proximity to the shooting location matters for the treatment effect, then the coefficient estimate on the interaction term should be positive.

The regression results displayed in Table 6 are consistent with our hypothesis that closer proximity to mass shootings generates greater visceral responses. Using the indicator variable specification, the coefficient estimates on the quartile 1 distance indicators are the largest in magnitude, indicating that those funds within 11 miles of the shooting event location subsequently decrease their risk by about 1% relative to their peers. This is true for both total and systematic risk. In the continuous interaction specification of the treatment, we estimate that funds located in the same ZIP code as the shooting event reduce risk by about 1.4%, relative to their peers and that this effect decreases with increased distance. The coefficient on the interaction term is significantly positive at 0.003. The estimated magnitudes of both specifications are highly consistent with one another. For example, the estimates from column 2 indicate that the average treatment effect for a fund located 4.8 miles (the average distance from event locations in quartile 1) from the mass shooting site would be -0.009 ($=-0.014 + 0.003 \times \ln(5.8)$) and at 90 miles it would be 0.00 ($=-0.014 + 0.003 \times \ln(91)$). These are extremely close to our coefficient estimates on the indicators for quartiles 1 and 4, -0.01 and 0.00, respectively.

The regression results in column 4 indicate that funds located in the same ZIP code as a mass shooting reduce their systematic risk by about 1.3% relative to their peers, which is about 10%

of the standard deviation of changes in systematic risk. To get a sense of this magnitude, we compare it to the baseline estimate of Pool et al. (2019) shown in column 3 of Table 3 of that article. They find that a one standard deviation decrease in personal housing wealth around the financial crisis led to about a 1.8% decrease in mutual fund risk taking, which was about 8.5% of a standard deviation of the change in mutual fund risk taking around the housing crisis.

5.4. Mechanism of risk reduction

Our measures to this point are based on realized risk. We show that funds exposed to fear realize lower risk than their peers through reductions in systematic risk. We now use holdings-based measures to identify *how* funds reduce systematic risk. There are two basic ways that equity funds can reduce systematic risk. They can move into holdings with lower systematic risk or they can reduce their portfolio weight in equities, moving more to cash or fixed income. Equity funds usually have constraints on using this second mechanism, since they are charged with investing in equity securities, but some variation does exist among funds' equity weights as shown in Table 3.

Again, we estimate Equation 3, this time using two alternative dependent variables: the change in the holdings-based market beta, calculated using Equation 1, and the change in the portfolio weight allocated to equity securities. The holdings come from funds' most recent quarterly holdings reports. To be included in the pre-period, the report date must be between -60 and -1 trading days relative to the event and to be included in the post period, the report date must be between 1 and 60 trading days relative to the event. Due to this, the horizon over which changes in risk taking are measured varies. Consequently, this introduces noise into these estimates such that they are not directly comparable to our earlier estimates.

The results displayed in Table 7 show that funds reduce risk by moving into less systematically risky stocks and not by reducing their equity weights. Funds that are more exposed to fear reduce their equity portfolio betas by about -0.003 relative to their peer funds. At about 6% of a standard deviation, the estimated magnitude as a percentage of the standard deviation of the change in the dependent variable is comparable to our earlier estimates using realized risk.

5.5. Dynamics of risk reduction

To this point, we have shown that fund managers exposed to fear reduce risk relative to their peers over a horizon of 30 trading days. We now test the duration of this effect. The potential implications for risk premia are different if fear has an ephemeral versus permanent effect. If the effect of fear on risk taking is temporary then this will induce greater volatility in risk taking and consequently risk premia, than would a permanent effect. This type of dynamic has the potential to provide an additional source of variation in risk taking and potentially in asset prices.

Figure 4 displays the estimates of the β_j's and their 90 percent confidence intervals from estimating Equation 4 with $T = 4$. The risk measures for the pre-event period are estimated 30 trading days prior to event and for the post-event period they are estimated over consecutive 30 trading day windows. Thus, each period is estimated over about a half of a fiscal quarter. Using either total risk or systematic risk, we find that our fear proxy has a temporary effect on risk taking that lasts for about one quarter.

5.6. Manager characteristics

We next test whether mass shootings affect fund managers differently based on observable characteristics. The literature on mass shootings finds that the effects are stronger on women and on younger individuals (Vuori et al., 2013; Lowe and Galea, 2017). Behavioral biases of mutual fund managers also have been shown to be stronger among less experienced managers (See for example Pool et al. (2012)), so it is possible that experience would lessen the effect of exposure to mass shootings on risk taking.

We test these predictions by interacting the treatment variable with each of these characteristics. Table 8 shows the results. In columns 1 and 4, we interact the treatment variable with the proportion of the management team that is female. The coefficient estimate on the interaction term in column 1 indicates that the response of female management teams to exposure to mass shootings is much greater than that of male management teams. All female funds reduce risk about five times more than all male funds. The results reported in column 4 using systematic risk are comparable in terms of magnitude, but the interaction term is not quite statistically significant.

The results also show that older and more experienced management teams respond less to exposure to our fear proxy. In this specification, age and experience are measured as the natural logarithm of one plus the average age or experience of managers of funds. Again, we estimate similar magnitudes using either total or systematic risk, but the significance of the results is weaker using systematic risk.

Finding heterogeneous treatment effects consistent with heterogeneity documented in the psychology and psychiatry literatures on mass shootings increases our confidence the changes in risk taking that we document are indeed effects from exposure to mass shootings and related to the visceral factor, fear.

5.7. Robustness

In this section, we conduct a number of robustness checks of the results. We test the validity of our mass shooting randomness assumption as well as the sensitivity of our results to our choice of control group, event horizon, risk measure, and data set. Furthermore, we perform placebo tests and tests of alternative mechanisms for risk reduction.

5.7.1. Balance tests

We assume that our treatment is randomly assigned to funds within styles for each event. In Figure 1, we showed that mass shootings are not constrained by geography. Now, we provide additional evidence supporting our randomness assumption. If the treatment is randomly assigned, then treated and control funds should not differ along observable dimensions. We test whether treatment and control funds differ by regressing fund characteristics on the treatment variable including style by event fixed effects. The results are displayed in Panel A of Table 9, which shows the treated and control funds do not differ significantly along any observable fund characteristics. The results in Panel B show that treated and control funds do not differ based upon adviser ZIP code level demographic characteristics. This also can be tested in a multiple regression framework by regressing the treatment variable on each of these characteristics simultaneously. Unreported results show that these regressions have almost no explanatory power and yield insignificant coefficients on all of the explanatory variables.

5.7.2. Alternative controls

We show above that the treatment variable is uncorrelated with observable fund characteristics. We now test whether the inclusion of these controls significantly alters the average treatment effect. If the treatment is randomly assigned, then inclusion of controls should not alter our estimates. Table A.2 shows the regression results using five different sets of controls with changes in total risk and systematic risk as dependent variables. No matter the permutation of control variables, the average treatment effect remains steady at -0.006. This is consistent with a randomly administered treatment.

5.7.3. Alternative control groups

We remove from the control group any fund that is treated during the same quarter by another event. We extend this constraint and remove funds that are treated in the previous quarter or the subsequent quarter. Table A.3 shows the regression result when we remove funds from the control group that are exposed by another event in the previous quarter and when we remove from the control group funds that are exposed to another event in the previous or subsequent quarter. These changes make no difference for our estimate of the average treatment effect.

5.7.4. Alternative event horizons

Admittedly, the event window of 30 trading days was chosen mostly for statistical reasons. We show that the results are robust to alternative event windows, with the caveat that we already have shown the effects to be temporary, so lengthening the windows will likely reduce the treatment effects that we estimate. The regression results using risk measures estimated over 45 and 60 trading days, instead of 30 days are shown in Table A.4. The results are consistent with our previous findings. Extending the trading periods reduces the significance and magnitudes of the estimates, but in all specifications we estimate a significantly negative effect of fear on risk taking.

5.7.5. Alternative risk measures

We estimate both systematic and idiosyncratic risk measures using a four factor model based on Fama and French (1993) augmented with the momentum factor of Carhart (1997). We then run our tests using each of these measures of risk. Panel A of Table A.5 shows the results. Consistent with earlier findings, funds exposed to fear reduce market risk, but the statistical significance is lower. Panel B of Table A.5 displays results of tests using the holdings-based measures of risk from the four factor model. Consistent with earlier findings, exposure to mass shootings leads funds to move into stocks with lower market betas.

5.7.6. Placebo tests

We conduct bootstrap simulations in which we randomize the assignment of the treatment within event-style clusters. This helps to ensure that our estimates of the average treatment effect (ATE) are not driven by omitted variables that are correlated with the treatment. We run two simulation models. In the first, within each event-style cluster, we randomly assign the treatment to the same number of funds that are actually treated within the cluster in our data. We then estimate Equation 3 with the change in the natural logarithm of total volatility as the dependent variable. We run 1,000 simulations. In the second, model, within each event-style cluster, we randomly assign the treatment to the same number of ZIP codes that are treated within that cluster in the actual data. Each fund within a treated ZIP code is considered treated. This accounts for the geographic clustering in the assignment of the treatment. Again, we run 1,000 simulations. We match the frequency of the treatment based upon the 50 mi. exposure level. Therefore, in the actual data, our estimate of the ATE is 0.0064, as displayed in column 6 of Table 4.

The distributions from these two simulations are displayed in Figure A.2. In both cases, our actual estimate of the ATE is larger in magnitude than all coefficient estimate generated from bootstrap samples. For both simulation methods, the median boostrap estimates are extremely close to zero. Moreover our estimate of the ATE is over three standard deviations from the mean estimate in both distributions.

5.7.7. Fund styles

We test whether a particular style or size category drives the results by interacting the treatment variable with indicators for these fund-mandated categories. The results shown in Table A.6, shows that no particular style category seems to drive our results. Size categories also do not seem to matter with regard to the effect size, but there is evidence that small-cap funds have the lowest treatment effects among size categories.

5.7.8. Alternative mechanisms

If expected fund flows are related to mass shooting exposure, then it is possible that fund managers reduce risk in response to expected investor flows instead of fear. Expected flows could be correlated with mass shootings, if for example, fund clienteles are local. This would cause investors and managers to be simultaneously exposed to mass shootings. We test this alternative mechanism by estimating the relationship between *actual* fund flows and mass shooting exposure.

To do so, we estimate a regression model that is analogous to Equation 4, with flows instead of risk as the dependent variable. Time units in the analysis are months since data on TNA are available monthly. Flows at $t = 0$ are those during the month of the event.

The estimates of β_j's from Equation 4 with flows as the dependent variables are displayed in Figure A.3. The fund flows of funds exposed to mass shootings do not change significantly following mass shooting events. This finding casts doubt on the alternative explanation that managers reduce risk in response to an expected decrease in fund flows.

Because our analysis relies on actual and not expected fund flows, we note that we cannot completely rule out that managers are just extremely effective at catering to investors. However, we find this "catering" alternative unlikely for a number reasons. First, the catering mechanism relies on mutual fund investors being local. While there is evidence that investors invest in nearby companies (Huberman, 2001; Ivković and Weisbenner, 2005), similar evidence does not exist for mutual fund investors. Second, if fund managers were to anticipate higher outflows, then they would increase cash to satisfy those potential flows. Yet, that is not what we find. Earlier we showed that mutual fund managers do not reduce their equity exposure (i.e. raise cash), but instead reduce risk by moving into equities with lower systematic risk.

A relative decline in the *realized* risk of exposed funds following a mass shooting could occur simply because the riskiness of the equities that they hold in their portfolios experience a relative decline. In this case, the decreased risk is not because of decisions made by managers. However, this mechanism is unlikely for several reasons. Mutual fund holdings tend to be biased toward local companies (Coval and Moskowitz, 1999) and firms exposed to terror events have been shown to be potentially more risky (Karolyi and Martell, 2010; Karolyi, 2008). Therefore, if systematic changes in the underlying risk profiles of fund holdings drive differential changes in fund risk, then we would expect to find that realized risk of exposed funds increases relative to their unexposed peers. Furthermore, our findings using holdings-based measures use risk profiles of firms prior to the event, ensuring that changes in portfolio holding risk do not drive our main findings.

5.7.9. Terrorism

Our analysis focuses on mass shooting events. Wang and Young (2020) show that the number of monthly international terrorist attacks are correlated with aggregate mutual fund flows. A natural question is whether mass shootings are a subset of *domestic* terrorist acts. We therefore analyze the overlap in events between those included in the SMSA database and terrorist attacks included in the union of the Global Terrorism Database (GTD)[16] and the International Terrorism: Attributes of Terrorist Events (ITERATE) database during our sample period.

The terrorism databases list 422 events over the same period. There are 28 events (4.3%) in the interesection between these terrorist events and the 254 events included in the SMSA database. Of the 422 terrorist acts only 22 (5.2%) include more than three deaths. Of these "high fatality" terrorist events, 14 (16%) overlap with the 84 high fatality events used in our main analysis. Figure

[16] The database is maintained by the National Consortium for the Study of Terrorism and Responses to Terrorism (START) at the University of Maryland. The data can be accessed at `https://gtd.terrorismdata.com`.

A.4 displays the overlap of these events by year. It shows that there is not substantial overlap between terrorist and mass shooting events. In unreported results, removing these events from the sample does not alter the results.

5.7.10. Alternative data source

We also confirm our results using a completely separate data source on mass shootings, the *Mother Jones* mass shooting database. As previously mentioned, the coverage of events relative to the SMSA database is reduced, focusing on more severe events. We also documented that locations of shootings do not always match with the SMSA database. We therefore run our tests using this alternative source. Table A.7 shows the results of regressions using the *Mother Jones* events for each of the measures of risk that we investigate. We use two different measures of exposure, one based on the locations from the SMSA database and one based on locations from the *Mother Jones* database. The findings are consistent with our previous results. Funds exposed to mass shootings reduce their risk relative to peers and this reduction is driven by systematic risk. The treatment effects that we document using these data are slightly larger than our earlier estimates. This may not be surprising, since the *Mother Jones* database covers only high fatality events.

6. Conclusion

We document a causal effect of fear on risk taking among actively managed U.S. mutual fund managers. Consistent with the idea that utility can be represented as state dependent on visceral factors (Loewenstein, 2000), we find that this effect is temporary. Since theory indicates that temporary changes in risk attitudes will lead to greater volatility in assets prices, these findings have the potential to help to explain why asset prices are much more volatile than rational models predict (Shiller, 1981; Grossman and Shiller, 1981). When combined with evidence that market downturns induce fear (Cohn et al., 2015), our findings also suggest that fear contributes to generating countercyclical variation in risk taking, which ultimately could help to explain countercyclical expected risk premia (Fama and French, 1989) and why equity premiums are so large (Mehra and Prescott, 1985). Our findings also validate the assumptions of prominent asset pricing models (Campbell and Cochrane, 1999; Barberis et al., 2001; Ju and Miao, 2012) that can explain these empirical regularities in asset prices.

In addition to supporting the laboratory findings of Cohn et al. (2015) and Guiso et al. (2018) in a real market setting, our study highlights a number of important open questions for future research. While the direction and the ephemeral nature of our estimates are informative, the magnitude of effect that we document is modest. Understanding how big a role fear plays in the volatility of asset prices would require a study that compares estimates of the amount of fear induced by exposure to mass shootings with that of other events that systematically induce fear (i.e. market busts).

Fig. 1: Geographic distribution of mass shootings 1999-2016

The figure plots the geographic distribution of mass shootings occurring from 1999 to 2016 in which at least one least one mutual fund adviser of an actively managed U.S. domestic equity fund is located within 100 miles of the event. The number of fatalities resulting from the event is displayed by the size of the circle. Events with above the median number of fatalities (3), are displayed in black and those with three or fewer fatalities are displayed in gray.

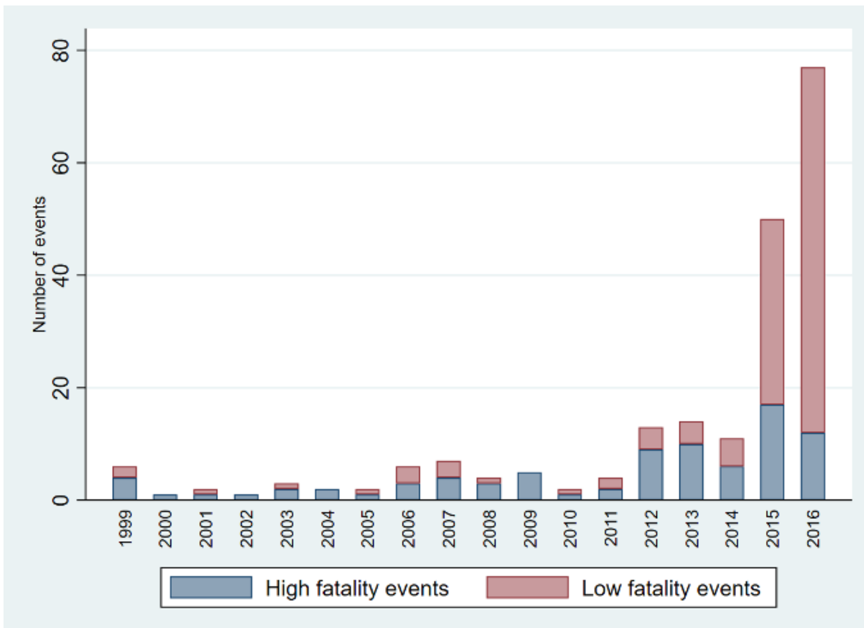


Fig. 2: Mass shootings by year and fatalities

The figure shows the number of mass shootings by year and severity. High fatality events are those with above the median number of fatalities (3). Low fatality events are those with three or fewer fatalities. The sample includes all mass shootings with at least one mutual fund adviser of an actively managed U.S. domestic equity fund located within 100 miles of the event. The sample includes 210 mass shootings spanning the years 1999 through 2016.

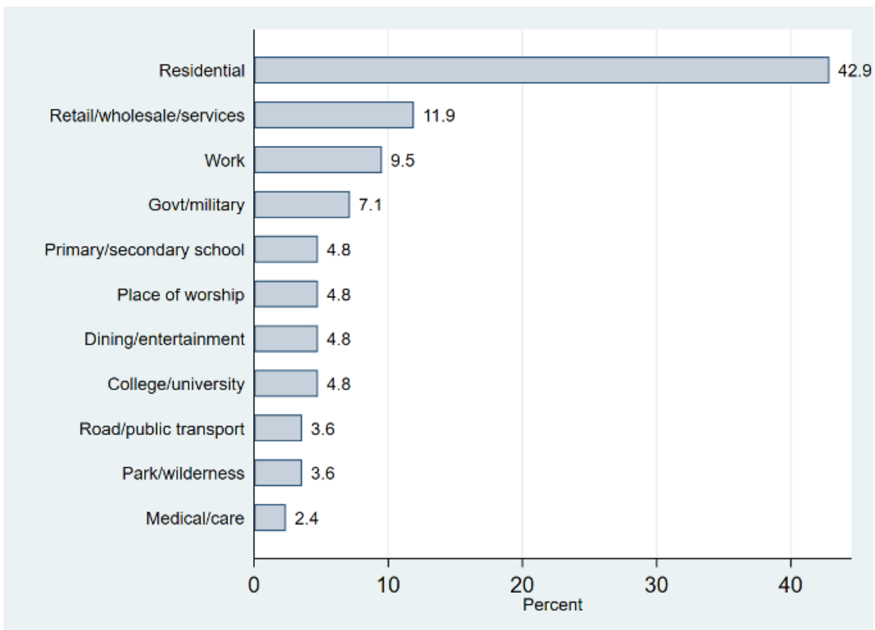


Fig. 3: Frequency of mass shootings by location type

The figure graphs the distribution of mass shootings by location. The sample includes all mass shootings with over three fatalities for which at least one mutual fund adviser of an actively managed U.S. domestic equity fund is located within 100 miles of the event. The sample includes 84 mass shootings spanning the years 1999 through 2016.

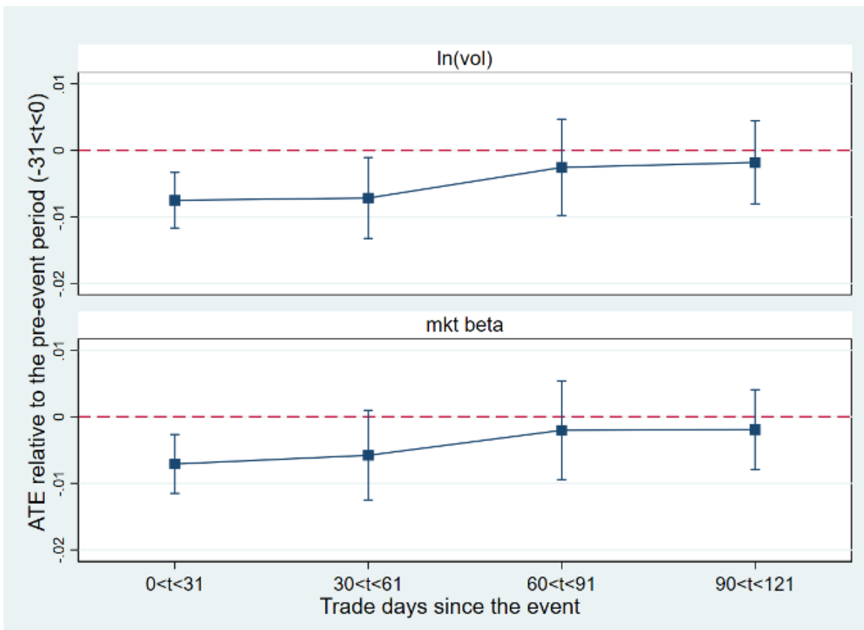


Fig. 4: Dynamics of the effect of fear on fund risk

The figure displays the estimates of the β_j's and their 90 percent confidence intervals from estimating Equation 4 with $T = 4$ using the pooled event sample of 84 high fatality mass shootings from the 4th quarter of 1999 through the 2nd quarter of 2016. Observations are at the mutual fund-event-time level. The fear treatment variable, I(MS dist. ≤ 50), is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location. The risk measures for the pre-event period are estimated 30 trading days prior to event and for the post-event period they are estimated over consecutive 30 trading day windows.

Table 1: 10 most fatal mass shootings 1999–2016

The table lists information on the 10 deadliest mass shootings in the U.S. from 1999 through 2016. Included are the dates, locations, number of fatalities and victims, and the number of U.S. domestic equity funds in the sample that are within 100 miles and 50 miles.

					Funds within	
Date	Event	Location	Fatalities	Victims	100 mi.	50 mi.
06/12/16	Orlando Nightclub Massacre	Orlando, FL	50	102	26	8
04/16/07	Virginia Tech Campus	Blacksburg, VA	33	49	0	0
12/14/12	Sandy Hook Elementary School	Newtown, CT	28	29	628	104
12/02/15	San Bernardino, California	San Bernardino, CA	16	35	112	42
04/20/99	Columbine High School	Littleton, CO	15	37	48	48
04/03/09	Immigration Services Center	Binghamton,NY	14	17	4	0
11/05/09	Fort Hood Army Base	Fort Hood, TX	13	45	22	0
09/16/13	Washington Navy Yard	Washington D.C.	13	15	168	134
07/20/12	Movie Theater in Aurora	Denver, CO	12	70	52	52
03/10/09	Geneva County, Alabama	Geneva, AL	11	16	0	0

Table 2: Mass shooting event summary statistics

The table summarizes data on the sample of mass shooting events from the SMSA database from the first quarter of 1999 through the second quarter of 2016 for which there was at least one mutual fund located within 100 miles of the shooting. There are a total of 210 such events. Summary statistics of these events are displayed in Panel A. Panel B displays statistics for the sample of high fatality events, which are defined as those with above median fatalities.

Panel A: Event characteristics: all events

	Mean	Median	Std	5th	95th	N
victims	7.371	5.000	9.561	3.000	17.000	210
fatalities	3.557	3.000	4.766	0.000	9.000	210
# of styles treated	8.129	9.000	3.593	2.000	12.000	210

Panel B: Event characteristics: events above median fatalities

	Mean	Median	Std	5th	95th	N
victims	10.940	7.000	14.156	4.000	35.000	84
fatalities	7.036	5.500	5.910	4.000	14.000	84
# of styles treated	8.679	10.000	3.492	2.000	12.000	84

Table 3: Summary statistics for the staggered event panel

The table provides summary statistics for the pooled event sample of 84 high fatality mass shootings from the first quarter of 1999 through the second quarter of 2016. Observations are at the mutual fund-event level. The sample construction is detailed in Section 3.2.3.

	Mean	Median	Std	5th	95th	N
MS dist	44.758	43.320	32.076	2.054	91.506	3,690
ln(1+ MS dist)	3.390	3.791	1.115	1.116	4.527	3,690
I(MS dist. ≤ 100)	0.049	0.000	0.217	0.000	0.000	74,689
I(MS dist. ≤ 50)	0.026	0.000	0.159	0.000	0.000	74,689
total volatility (vol)	1.087	0.937	0.481	0.575	2.098	74,689
market beta (mkt beta)	1.019	1.007	0.188	0.726	1.349	74,689
idiosyncratic volatility (idio vol)	0.340	0.297	0.193	0.120	0.726	74,689
tracking error (track error)	0.378	0.318	0.233	0.127	0.864	74,689
market beta holding-based (mkt beta hold)	1.065	1.044	0.178	0.807	1.384	55,851
equity weight	0.955	0.970	0.055	0.862	0.999	55,833
Δ ln(vol)	-0.021	-0.022	0.307	-0.557	0.511	74,689
Δ mkt beta	-0.008	-0.005	0.124	-0.224	0.195	74,689
Δ ln(idio vol)	-0.046	-0.051	0.255	-0.463	0.383	74,689
Δ ln(track err)	-0.045	-0.051	0.257	-0.461	0.394	74,689
Δ mkt beta hold	-0.004	-0.003	0.049	-0.086	0.077	55,851
Δ equity weight	0.000	0.000	0.024	-0.040	0.041	55,833
lag TNA	1,539.635	251.300	5,821.432	10.900	6,052.900	74,689
lag ln(age)	2.455	2.558	0.824	0.920	3.802	74,689
lag ln(TNA)	5.543	5.527	1.935	2.389	8.708	74,689
lag exp ratio	0.012	0.011	0.004	0.006	0.019	74,689
lag turn ratio	0.743	0.570	0.750	0.110	1.890	74,689
Prop. female mgrs	0.093	0.000	0.213	0.000	0.500	73,264
Avg. mgr exp	10.324	9.667	5.589	2.458	20.500	73,264
ln(1+ mgr exp)	2.291	2.367	0.556	1.241	3.068	73,264
Avg. mgr age	48.088	47.000	8.689	36.000	64.000	59,851
ln(mgr age)	3.857	3.850	0.177	3.584	4.159	59,851
rural%	1.244	0.000	5.798	0.000	7.200	62,163
ln(pop density)	8.396	8.712	1.883	5.875	10.813	63,423
female%	47.889	49.460	6.632	37.270	54.730	62,163

The table is continued from the previous page.

	Mean	Median	Std	5th	95th	N
white%	74.206	77.590	20.648	20.390	93.820	62,163
married%	41.152	39.200	13.483	23.320	63.640	61,544
college%	52.319	57.290	20.739	14.460	76.730	61,399
ln(med income)	10.855	11.007	0.566	9.738	11.519	61,399

Table 4: The effect of fear on risk taking: by event severity

The table reports regression results from fixed effects regressions of changes in risk taking on exposure to fear (Equation 3), using the pooled event sample of 210 mass shootings from the first quarter of 1999 through the second quarter of 2016. Regressions are estimated using the full sample of events (columns 1 and 2) and also for low (columns 3 and 4) and high (columns 5 and 6) fatality events. High fatality events are those with more than 3 (the median) deaths. Observations are at the mutual fund-event level. The dependent variable is the change in the natural logarithm of total fund risk, which is estimated using daily data from -30 to -1 trading days prior to the event to 1 to 30 trading days after the event. Indicator variables based on proximity to mass shooting events are used as measures of exposure to fear. I(MS dist. ≤ 100) (I(MS dist. ≤ 50)) is an indicator variable indicating if a fund's adviser is within 100 (50) miles of the shooting location. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	All events		Low fatality		High fatality	
	(1)	(2)	(3)	(4)	(5)	(6)
I(MS dist. ≤ 100)	-0.003		-0.001		-0.004**	
	(-1.55)		(-0.46)		(-2.13)	
I(MS dist. ≤ 50)		-0.002		0.001		-0.006**
		(-1.08)		(0.36)		(-2.63)
Style-event FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.93	0.93	0.93	0.93	0.93	0.93
N	146,778	146,778	72,108	72,108	74,670	74,670
Num. events	210	210	126	126	84	84

Table 5: The effect of fear on risk taking: types of risk

The table reports regression results from fixed effects regressions of changes in various measures of risk taking on exposure to fear (Equation 3), using the pooled event sample of 84 high fatality mass shootings from the first quarter of 1999 through the second quarter of 2016. Observations are at the mutual fund-event level. The dependent variables are: the change in market beta in column 1, the change in the natural logarithm of idiosyncratic risk in column 2, and the change in the natural logarithm of tracking error in column 3. All of these changes are estimated using daily data from -30 to -1 trading days prior to the event to 1 to 30 trading days after the event. The fear treatment variable, I(MS dist. ≤ 50), is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ mkt beta	Δ ln(idio vol)	Δ ln(track err)
	(1)	(2)	(3)
I(MS dist. ≤ 50)	-0.006**	0.003	0.002
	(-2.42)	(0.57)	(0.32)
Style-event FE	Yes	Yes	Yes
Adj-R-squared	0.50	0.40	0.42
N	74,670	74,670	74,670
Num. events	84	84	84

Table 6: The effect of fear on risk taking: the effect of distance

The table reports regression results from fixed effects regressions of changes in risk taking on exposure to fear (Equation 3), using the pooled event sample of 84 high fatality mass shootings from the first quarter of 1999 through the second quarter of 2016. Observations are at the mutual fund-event level. The dependent variables are: the change in the natural logarithm of total fund risk in columns 1 and 2 and the change in market beta in columns 3 and 4. All of these changes are estimated using daily data from -30 to -1 trading days prior to the event to 1 to 30 trading days after the event. The fear treatment variables in columns 1 and 3 are indicator variable indicating if the distance of a fund is in the 1st, 2nd, 3rd, or 4th quartile of this distance of funds that are within 100 miles of the mass shooting event. In columns 2 and 4, the fear treatment variables include an indicator variable, I(MS dist. ≤ 100), indicating if a fund's adviser is within 100 miles of the shooting location and the interaction of that variable with the natural logarithm of 1 plus the fund's distance to the mass shooting. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ ln(vol)		Δ mkt beta	
	(1)	(2)	(3)	(4)
I(MS dist. quartile 1)	-0.010***		-0.009***	
	(-2.65)		(-2.75)	
I(MS dist. quartile 2)	-0.003		-0.004	
	(-1.13)		(-1.40)	
I(MS dist. quartile 3)	-0.003		-0.003	
	(-0.92)		(-0.94)	
I(MS dist. quartile 4)	0.000		0.000	
	(0.10)		(0.11)	
I(MS dist. ≤ 100)		-0.014***		-0.013**
		(-2.80)		(-2.60)
I(MS dist. ≤ 100) $\times$ ln(1+ MS dist)		0.003**		0.003**
		(2.29)		(2.09)
Style-event FE	Yes	Yes	Yes	Yes
Adj-R-squared	0.93	0.93	0.50	0.50
N	74,670	74,670	74,670	74,670
Num. events	84	84	84	84

Table 7: The effect of fear on risk taking: holding-based risk measures

The table reports regression results from fixed effects regressions of changes in various holdings-based measures of risk taking on exposure to fear (Equation 3), using the pooled event sample of 84 high fatality mass shootings from the first quarter of 1999 through the second quarter of 2016. Observations are at the mutual fund-event level. The dependent variables are: the change in the holdings-based market beta, where this measure is defined in the text by Equation 1, in column 1, the change in the portfolio weight on equity securities, defined as the total value of equity securities divided by the total value of the portfolio, in column 2. Holdings come from the funds most recent quarterly holdings reports. To be included in the pre-period the report date must be between -60 and -1 trading days relative to the event and to be included in the post period the report date must be between 1 and 60 trading days relative to the event. The fear treatment variable, I(MS dist. ≤ 50), is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ mkt hbeta	Δ equity weight
	(1)	(2)
I(MS dist. ≤ 50)	-0.003**	-0.000
	(-2.03)	(-0.14)
Style-event FE	Yes	Yes
Adj-R-squared	0.12	0.02
N	55,836	55,818
Num. events	79	79

Table 8: The effect of fear on risk taking: by manager characteristics

The table reports regression results from fixed effects regressions of changes in risk taking on exposure to fear (Equation 3) interacted with various fund manager characteristics, using the pooled event sample of 84 high fatality mass shootings from the first quarter of 1999 through the second quarter of 2016. Observations are at the mutual fund-event level. The dependent variables are: the change in the natural logarithm of total fund risk in columns 1 through 3 and the change in market beta in columns 4 through 6. All of these changes are estimated using daily data from -30 to -1 trading days prior to the event to 1 to 30 trading days after the event. The fear treatment variable, I(MS dist. ≤ 50), is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location. In columns 1 and 4 the treatment variable is interacted with the proportion of the fund's management team that is female. In columns 2 and 5, the treatment variable is interacted with the natural logarithm of 1 plus the average experience of the management team. Experience is measured as the number of years since the manager's first management position documented in Morningstar Direct. In columns 3 and 6, the treatment variable is interacted with the natural logarithm of the average age of fund managers. Fund manager age comes from Pool et al. (2015) and is not available for all managers in the sample. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ ln(vol)			Δ mkt beta		
	(1)	(2)	(3)	(4)	(5)	(6)
I(MS dist. ≤ 50)	-0.004**	-0.025***	-0.150**	-0.005*	-0.022***	-0.106
	(-2.04)	(-3.13)	(-2.34)	(-1.88)	(-2.76)	(-1.47)
I(MS dist. ≤ 50) $\times$ Prop. female mgrs	-0.017*			-0.014		
	(-1.72)			(-1.50)		
Prop. female mgrs	0.003			0.002		
	(1.60)			(1.02)		
I(MS dist. ≤ 50) $\times$ ln(1+ mgr exp)		0.008**			0.007*	
		(2.33)			(1.80)	
ln(1+ mgr exp)		0.001			0.002**	
		(1.14)			(2.01)	
I(MS dist. ≤ 50) $\times$ ln(mgr age)			0.038**			0.026
			(2.27)			(1.41)
ln(mgr age)			0.004			0.005*
			(1.36)			(1.93)
Style-event FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.93	0.93	0.93	0.50	0.50	0.50
N	73,247	73,247	59,833	73,247	73,247	59,833
Num. events	84	84	84	84	84	84

Table 9: Treated and control fund characteristics

The table reports regression results from fixed effects regressions of fund characteristics (Panel A) and adviser ZIP code level demographic characteristics (Panel B) on the fear treatment variable, using the pooled event sample of 84 high fatality mass shootings from the first quarter of 1999 through the second quarter of 2016. Observations are at the mutual fund-event level. The fear treatment variable, I(MS dist. ≤ 50), is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location. Adviser ZIP code level demographic characteristics are based on the 2000 U.S. Census. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Fund characteristics

	ln(TNA)	ln(age)	exp ratio	turn ratio	prop fem	ln(mgr age)	ln(mgr exp)	ln(vol)	mkt beta	ln(idio vol)	ln(track err)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
I(MS dist. ≤ 50)	0.095	0.027	-0.000	-0.026	-0.001	-0.001	0.007	0.005	0.005	-0.003	0.002
	(1.06)	(0.82)	(-0.85)	(-0.92)	(-0.22)	(-0.16)	(0.39)	(1.21)	(0.99)	(-0.25)	(0.18)
Style-event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.04	0.09	0.10	0.07	0.01	0.08	0.11	0.91	0.48	0.65	0.66
N	74,670	74,670	74,670	74,670	73,247	59,833	73,247	74,670	74,670	74,670	74,670
Num. events	84	84	84	84	84	84	84	84	84	84	84

Panel B: Adviser ZIP code-level demographic characteristics

	rural%	ln(pop density)	female%	white%	married%	college%	ln(med income)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
I(MS dist. ≤ 50)	-0.041	0.166	-0.141	-0.265	0.522	0.106	0.031
	(-0.15)	(1.11)	(-0.22)	(-0.16)	(0.43)	(0.05)	(0.56)
Style-event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.01	0.01	0.01	0.02	0.02	0.00	0.01
N	62,145	63,405	62,145	62,145	61,526	61,381	61,381
Num. events	84	84	84	84	84	84	84

Appendix

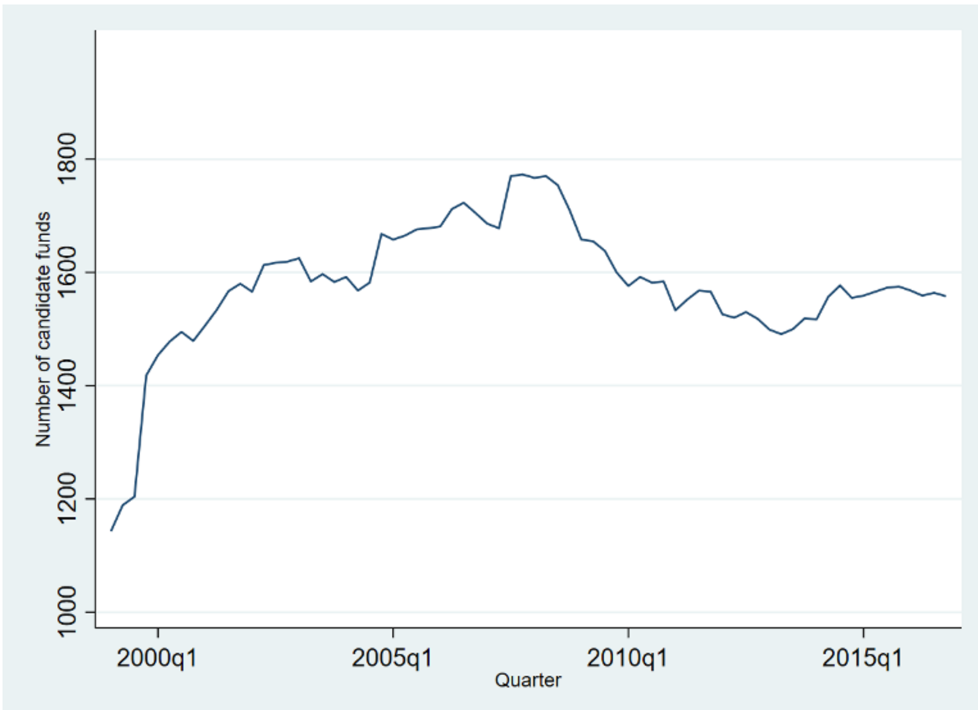
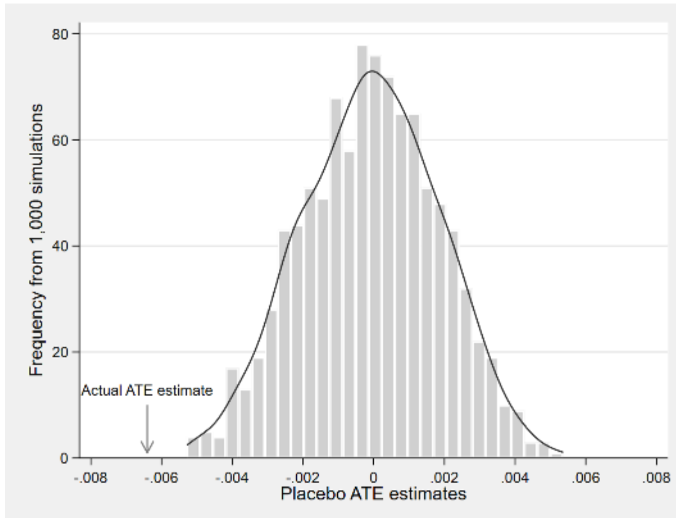


Fig. A.1: Candidate sample of funds by quarter

The figure plots the number of funds included each quarter in the candidate sample of funds described in Section 3.2.2.

Panel A: Treatment randomized by fund



Panel B: Treatment randomized by ZIP code

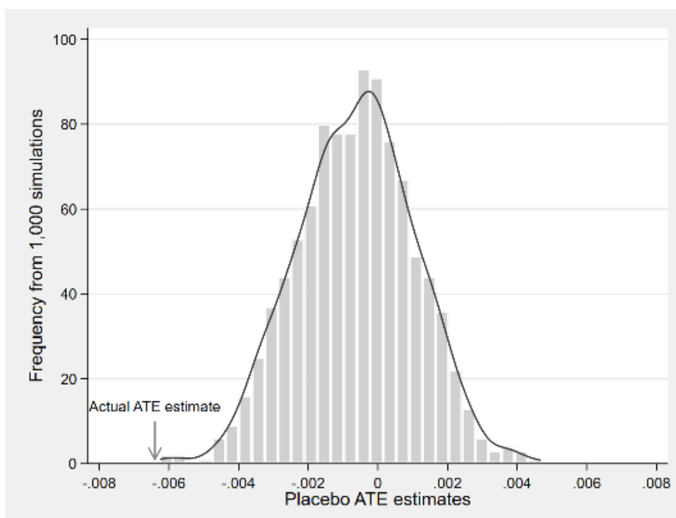


Fig. A.2: Distribution of boostrap estimates of the effect of fear on risk taking

The figure displays the distribution of estimates of β from Equation 3 for 1,000 bootstrap samples in which the treatment is randomized across funds (Panel A) and ZIP codes (Panel B) with the change in the natural logarithm of total realized risk as the dependent variable. The treatment is randomized within each event-style cluster with the same frequency in which they occur in the data. The simulations are based on the pooled event sample of 84 high fatality mass shootings from the first quarter of 1999 through the second quarter of 2016. Observations are at the mutual fund-event level. The frequency of the randomized treatment is based on I(MS dist. ≤ 50), which is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location.

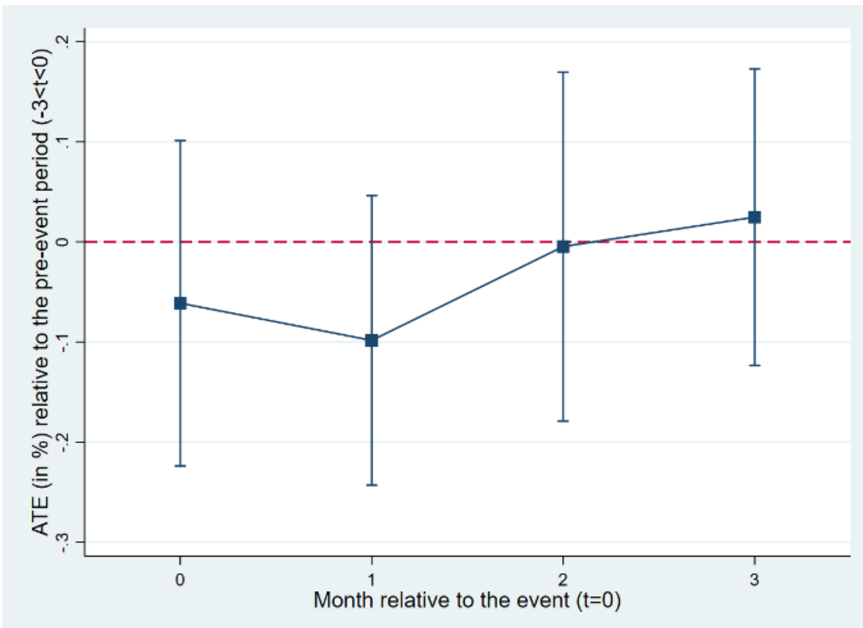


Fig. A.3: Fund flows and fund exposure to fear

The figure displays the estimates of the β_j's and their 90 percent confidence intervals from estimating Equation 4 with fund flows as the dependent variable and $T = 3$, using the pooled event sample of 84 high fatality mass shootings from the first quarter of 1999 through the second quarter of 2016. Observations are at the mutual fund-event-month level. The fear treatment variable, I(MS dist. ≤ 50), is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location. The pre-event period includes fund flows during two months prior to the event. Fund flows are estimated using monthly TNA and return data. Included months are $-2 \leq t \leq 3$, relative to the event.

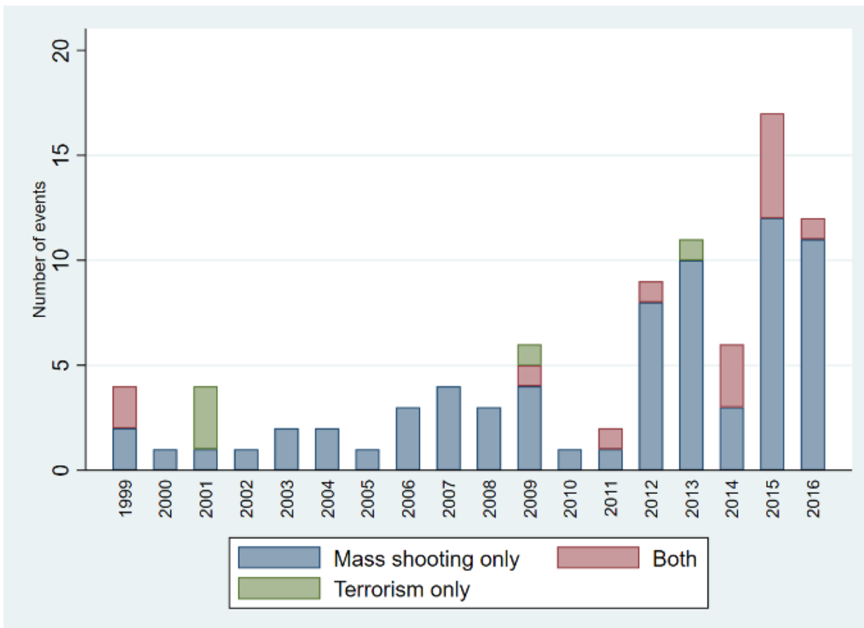


Fig. A.4: Overlap between terrorist attacks and high fatality mass shootings by year

The figure shows the relationship between high fatality domestic terrorist attacks and the mass shootings used in the main analysis by year. High fatality events are those with more than three fatalities. The number of events are broken down each year between mass shootings in the sample that are not covered by the terrorism databases outlined in section 5.7.9 of the text (Mass shooting only), mass shootings in our sample that are also covered by the terrorism databases (Both), and terrorist events that are not included in our sample of mass shootings (Terrorism only). There 89 events included in this analysis, 84 of which are events used in the main analysis.

Table A.1: Mutual fund candidate quarterly panel sample construction

The table lists the number observations at either the share class-quarter- or fund-quarter-level that exist after each subsequent criteria is added in the construction of the sample of actively managed U.S. domestic equity mutual fund candidates available each quarter to be utilized for the staggered pooled event panel.

Description	Observations	Unit of observation
Initial CRSP share class sample 4Q1998 - 4Q2017	1,729,211	share class quarter
Drop ETFs	1,674,543	share class quarter
Drop variable annuities	1,515,912	share class quarter
Keep if CRSP objective code = "E"	853,154	share class quarter
Drop share classes not merged to MS Direct	758,857	share class quarter
Drop index funds (defined)	716,672	share class quarter
Drop "index" funds (textual)	708,602	share class quarter
Drop if US Category Group = "Allocation"	642,287	share class quarter
Drop if US Category Group = "International Equity"	471,234	share class quarter
Keep if Lipper class is in 12 box styles	374,729	share class quarter
Collapse to the fund level	131,307	fund quarter
Drop funds with missing zip codes	127,513	fund quarter
Drop funds with missing control variables	119,477	fund quarter
Drop small funds	113,604	fund quarter

Table A.2: The effect of fear on risk taking: alternative controls

The table reports regression results from fixed effects regressions of changes in various measures of risk taking on exposure to fear (Equation 3) analogous to those reported in Table 5, using alternative sets of control variables. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ ln(vol)					Δ mkt beta				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
I(MS dist. ≤ 50)	-0.006***	-0.006***	-0.006**	-0.006**	-0.006**	-0.006**	-0.006**	-0.006**	-0.006**	-0.006**
	(-2.64)	(-2.65)	(-2.55)	(-2.56)	(-2.58)	(-2.42)	(-2.43)	(-2.44)	(-2.45)	(-2.45)
lag ln(TNA)	0.000	0.000		0.000	0.000	0.000	-0.000		0.000	-0.000
	(0.97)	(0.19)		(0.88)	(0.17)	(0.39)	(-0.34)		(0.23)	(-0.26)
lag ln(age)		0.000			0.000		0.001			0.000
		(0.80)			(0.38)		(0.78)			(0.18)
lag exp ratio		-0.321*			-0.326		-0.206			-0.199
		(-1.67)			(-1.63)		(-1.25)			(-1.17)
lag turn ratio		0.000			-0.000		-0.000			-0.000
		(0.03)			(-0.09)		(-0.38)			(-0.19)
Prop. female mgrs			0.003	0.003	0.003			0.001	0.001	0.001
			(1.35)	(1.37)	(1.39)			(0.77)	(0.78)	(0.80)
ln(1+ mgr exp)			0.001	0.001	0.001			0.002**	0.002**	0.002**
			(1.30)	(1.06)	(1.18)			(2.14)	(2.21)	(2.20)
Style-event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.93	0.93	0.93	0.93	0.93	0.50	0.50	0.50	0.50	0.50
N	74,670	74,670	73,247	73,247	73,247	74,670	74,670	73,247	73,247	73,247

Table A.3: The effect of fear on risk taking: alternative control groups

The table reports regression results from fixed effects regressions of changes in various measures of risk taking on exposure to fear (Equation 3) analogous to those reported in Table 5, using alternative control groups. In columns 1 and 3 funds that are exposed to another event in the previous quarter are excluded from control groups. In columns 2 and 4 funds that are exposed to another event in the previous or following quarter are excluded from control groups. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ ln(vol)		Δ mkt beta	
	(1)	(2)	(3)	(4)
I(MS dist. ≤ 50)	-0.006**	-0.006**	-0.006**	-0.006**
	(-2.50)	(-2.58)	(-2.35)	(-2.37)
Style-event FE	Yes	Yes	Yes	Yes
Adj-R-squared	0.93	0.92	0.49	0.47
N	68,477	60,965	68,477	60,965
Exposed in prev. qtr in control group	No	No	No	No
Exposed in following qtr in control group	Yes	No	Yes	No

Table A.4: The effect of fear on risk taking: alternative event horizons

The table reports regression results from fixed effects regressions of changes in various measures of risk taking on exposure to fear (Equation 3) analogous to those reported in Table 5, using alternative event horizons. The number of trading days over which risk in the pre-event (post-event) period is calculated is given by the first (second) number in the ordered pair. For example, the pre-event risk in columns 1 and 5 is estimated using 30 trading days prior to the event and post event risk is measured using the 45 trading days following the event. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ ln(vol)				Δ mkt beta			
	(1) (30,45)	(2) (30,60)	(3) (45,45)	(4) (60,60)	(5) (30,45)	(6) (30,60)	(7) (45,45)	(8) (60,60)
I(MS dist. ≤ 50)	-0.006*	-0.006*	-0.005**	-0.004*	-0.006*	-0.006*	-0.006**	-0.005**
	(-1.88)	(-1.97)	(-2.00)	(-1.83)	(-1.91)	(-1.84)	(-2.01)	(-2.23)
Style-event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.93	0.93	0.93	0.93	0.48	0.49	0.49	0.51
N	73,637	73,512	73,508	73,542	73,845	73,201	73,631	72,946

Table A.5: The effect of fear on risk taking: alternative risk measures

The table reports regression results from fixed effects regressions of changes in various measures of risk taking on exposure to fear (Equation 3) analogous to those reported in Table 5, using alternative measures of risk constructed from four factor models that include as factors excess returns on the market, size, value, and momentum factors in Fama and French (1993) and Carhart (1997). In Panel A the dependent variables are based on realized risk and are constructed as in Table 5. In Panel B, the dependent variables are holdings-based measures and are constructed as in Table 7. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

Panel A: Realized risk measures

	Δ mkt beta ff	Δ smb beta	Δ hml beta	Δ mom beta	Δ ln(idio vol ff)
	(1)	(2)	(3)	(4)	(5)
I(MS dist. ≤ 50)	-0.005*	0.003	-0.005	0.002	-0.005
	(-1.74)	(0.45)	(-0.83)	(0.47)	(-0.74)
Style-event FE	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.20	0.18	0.16	0.18	0.29
N	73,206	73,095	73,172	73,161	74,147
Num. events	84	84	84	84	84

Panel B: Holdings-based risk measures

	Δ mkt hbeta ff	Δ smb hbeta	Δ hml hbeta	Δ mom hbeta
	(1)	(2)	(3)	(4)
I(MS dist. ≤ 50)	-0.003***	-0.001	0.003	0.002*
	(-2.76)	(-0.58)	(1.32)	(1.89)
Style-event FE	Yes	Yes	Yes	Yes
Adj-R-squared	0.08	0.05	0.08	0.08
N	55,923	55,801	55,872	55,857
Num. events	79	79	79	79

Table A.6: The effect of fear on risk taking: by fund styles

The table reports regression results from fixed effects regressions of changes in various measures of risk taking on exposure to fear (Equation 3) interacted with fund style and size categories, analogous to those reported in Table 5. The omitted category in columns 1 and 3 is “All-cap fund”. The omitted category in columns 2 and 4 is “Blend fund”. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ ln(vol)		Δ mkt beta	
	(1) Size	(2) Val/Gr	(3) Size	(4) Val/Gr
I(MS dist. ≤ 50)	-0.010**	-0.007**	-0.010*	-0.008***
	(-2.30)	(-2.55)	(-1.86)	(-2.72)
I(MS dist. ≤ 50) $\times$ Small-cap fund	0.009		0.007	
	(1.53)		(0.94)	
I(MS dist. ≤ 50) $\times$ Mid-cap fund	0.004		0.002	
	(0.61)		(0.28)	
I(MS dist. ≤ 50) $\times$ Large-cap fund	0.003		0.004	
	(0.54)		(0.66)	
I(MS dist. ≤ 50) $\times$ Value fund		0.001		0.006
		(0.20)		(1.04)
I(MS dist. ≤ 50) $\times$ Growth fund		0.001		0.002
		(0.13)		(0.38)
Style-event FE	Yes	Yes	Yes	Yes
Adj-R-squared	0.93	0.93	0.50	0.50
N	74,670	74,670	74,670	74,670

Table A.7: The effect of fear on risk taking: alternative data source

The table reports regression results from fixed effects regressions of changes in various measures of risk taking on exposure to fear (Equation 3) analogous to those reported in Table 5, using an alternative event data source. The data source for the events comes from the *Mother Jones* data and includes 44 mass shootings events. Two different fear treatment variables are considered. I(MS dist. ≤ 50) is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location based on location information from the SMSA database. I(MS alt. dist. ≤ 50) is an indicator variable indicating if a fund's adviser is within 50 miles of the shooting location based on location information from the *Mother Jones* database. All regressions include style by event fixed effects. T-statistics based on robust standard errors double clustered by event and mutual fund adviser ZIP code are displayed in parentheses. Significance of two-sided hypothesis tests are indicated by ***, **, and *, which correspond to significance at the 1%, 5%, and 10% levels, respectively.

	Δ ln(vol)		Δ mkt beta		Δ ln(idio vol)		Δ ln(track err)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
I(MS dist. ≤ 50)	-0.008**		-0.008*		0.004		0.002	
	(-2.28)		(-1.96)		(0.67)		(0.22)	
I($0 \leq$ MS alt. dist. < 50)		-0.008**		-0.008**		0.005		0.003
		(-2.17)		(-2.11)		(0.75)		(0.44)
Style-event FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj-R-squared	0.91	0.91	0.41	0.41	0.39	0.39	0.42	0.42
N	44,236	44,236	44,236	44,236	44,236	44,236	44,236	44,236
Num. Events	44	44	44	44	44	44	44	44

References

Abadie, A., Athey, S., Imbens, G. W., Wooldridge, J., 2017. When should you adjust standard errors for clustering? Tech. rep., National Bureau of Economic Research.

Addington, L. A., 2003. Students' fear after columbine: Findings from a randomized experiment. Journal of Quantitative Criminology 19 (4), 367–387.

Alok, S., Kumar, N., Wermers, R., 2020. Do fund managers misestimate climatic disaster risk. The Review of Financial Studies 33 (3), 1146–1183.

Anderson, L. R., Mellor, J. M., 2008. Predicting health behaviors with an experimental measure of risk preference. Journal of Health Economics 27, 1260–1274.

Barberis, N., Huang, M., Santos, T., 2001. Prospect theory and asset prices. The Quarterly Journal of Economics 116 (1), 1–53.

Bernile, G., Bhagwat, V., Kecskes, A., Nguyen, P.-A., 2018. Are the risk attitudes of professional investors affected by personal catastrophic experiences? Available at SSRN 3024983.

Bernile, G., Bhagwat, V., Rau, P. R., February 2017. "What Doesn't Kill You Will Only Mke You More Risk-Loving: Early-Life Disasters and CEO Behavior." The Journal of Finance 72 (1), 167–205.

Blevins, C., Mullen, L., 2015. Jane, john... leslie? A historical method for algorithmic gender prediction. DHQ: Digital Humanities Quarterly 9 (3).

Bogan, V. L., Just, D. R., Wasink, B., July 2013. Do psychological shocks affect financial risk taking behavior? A study of U.S. veterans. Contemporary Economic Policy 31 (3), 457–467.

Bonin, H., Dohmen, T., Falk, A., Huffman, D., Sunde, U., 2007. Cross-sectional earnings risk and occupational sorting: The role of risk attitudes. Labour Economics 14, 926–937.

Brown, R., Montalva, V., Thomas, D., Velásquez, A., 2019. Impact of violent crime on risk aversion: Evidence from the Mexican drug war. Review of Economics and Statistics 101 (5), 892–904.

Callen, M., Isaqzadeh, M., Long, J. D., Sprenger, C., 2014. Violence and risk preference: Experimental evidence from Afghanistan. American Economic Review 104 (1), 123–48.

Cameron, L., Shah, M., 2015. Risk-taking behavior in the wake of natural disasters. Journal of Human Resources 50 (2), 484–515.

Campbell, J. Y., Cochrane, J. H., 1999. By force of habit: A consumption-based explanation of aggregate stock market behavior. Journal of Political Economy 107 (2), 205–251.

Carhart, M. M., 1997. On persistence in mutual fund performance. The Journal of Finance 52 (1), 57–82.

Chevalier, J., Ellison, G., 1997. Risk taking by mutual funds as a response to incentives. Journal of Political Economy 105 (6), 1167–1200.

Chevalier, J., Ellison, G., 1999. Career concerns of mutual fund managers. The Quarterly Journal of Economics 114 (2), 389–432.

Chuang, Y., Schechter, L., 2015. Stability of experimental and survey measures of risk, time, and social preferences: A review and some new results. Journal of Development Economics 117, 151–170.

Cohn, A., Engelmann, J., Fehr, E., Maréchal, M. A., 2015. Evidence for countercyclical risk aversion: An experiment with financial professionals. American Economic Review 105 (2), 860–85.

Coval, J. D., Moskowitz, T. J., 1999. Home bias at home: Local equity preference in domestic portfolios. The Journal of Finance 54, 2045–2073.

DellaVigna, S., June 2009. Psychology and economics: Evidence from the field. Journal of Economic Literature 47 (2), 315–372.

Dohmen, T., Lehmann, H., Pignatti, N., 2016. Time-varying individual risk attitudes over the great recession: A comparison of Germany and Ukraine. Journal of Comparative Economics 44 (1), 182–200.

Edmans, A., Garcia, D., Norli, Ø., 2007. Sports sentiment and stock returns. The Journal of Finance 62 (4), 1967–1998.

Epstein, L. G., Zin, S. E., 1990. 'first-order' risk aversion and the equity premium puzzle. Journal of Monetary Economics 26, 387–407.

Fama, E. F., French, K. R., 1989. Business conditions and expected returns on stocks and bonds. Journal of Financial Economics 25 (1), 23–49.

Fama, E. F., French, K. R., 1993. Common risk factors in the returns on stocks and bonds. Journal of Financial Economics 33, 3–56.

Gerrans, P., Faff, R., Hartnett, N., 2015. Individual financial risk tolerance and the global financial crisis. Accounting & Finance 55 (1), 165–185.

Grossman, S. J., Shiller, R. J., 1981. The determinants of the variability of stock market prices. The American Economic Review 71 (2), 222–227.

Guiso, L., Sapienza, P., Zingales, L., 2018. Time varying risk aversion. Journal of Financial Economics 128 (3), 403–421.

Haigh, M. S., List, J. A., 2005. Do professional traders exhibit myopic loss aversion? An experimental analysis. The Journal of Finance 60 (1), 523–534.

Hawdon, J., Rasanen, P., Oksanen, A., Vuori, M., 2014. Social responses to collective crime: Assessing the relationship between crime-related fears and collective sentiments. European Journal of Criminology 11 (1), 39–56.

Hirshleifer, D., Shumway, T., 2003. Good day sunshine: Stock returns and the weather. The Journal of Finance 58 (3), 1009–1032.

Hong, H., Kostovetsky, L., 2012. “Red and Blue Investing: Values and Finance." The Journal of Financial Economics 103, 1–19.

Hong, H., Kubik, J. D., Stein, J. C., 2005. Thy neighbor's portfolio: Word-of-mouth effects in the holdings and trades of money managers. The Journal of Finance 60 (6), 2801–2824.

Hsieh, C., Parker, S. C., vanPraag, C. M., 2017. Risk, balanced skills and entrepreneurship. Small Business Economics 48, 287–302.

Huang, J., Sialm, C., Zhang, H., 2011. Risk shifting and mutual fund performance. The Review of Financial Studies 24 (8), 2575–2616.

Huberman, G., 2001. Familiarity breeds investment. The Review of Financial Studies 14 (3), 659–680.

Ivković, Z., Weisbenner, S., 2005. Local does as local is: Information content of the geography of individual investors' common stock investments. The Journal of Finance 60 (1), 267–306.

Ju, N., Miao, J., 2012. Ambiguity, learning, and asset returns. Econometrica 80 (2), 559–591.

Kaminski, R. J., KoonsWitt, B. A., Thompson, N. S., Weiss, D., 2010. The impacts of the Virginia Tech and Northern Illinois University shootings on fear of crime on campus. Journal of Criminal Justice 38, 88–89.

Kamstra, M. J., Kramer, L. A., Levi, M. D., 2000. Losing sleep at the market: The daylight saving anomaly. American Economic Review 90 (4), 1005–1011.

Kamstra, M. J., Kramer, L. A., Levi, M. D., 2003. Winter blues: A SAD stock market cycle. American Economic Review 93 (1), 324–343.

Karolyi, G. A., 2008. An assessment of terrorism-related investing strategies. The Journal of Portfolio Management 34 (4), 108–123.

Karolyi, G. A., Martell, R., 2010. Terrorism and the stock market. International Review of Applied Financial Issues & Economics 2 (2).

Kuhnen, C. M., Knutson, B., 2005. The neural basis of financial risk taking. Neuron 47 (5), 763–770.

Kuhnen, C. M., Knutson, B., 2011. The influence of affect on beliefs, preferences, and financial decisions. Journal of Financial and Quantitative Analysis 46 (3), 605–626.

Levin, I. P., Hart, S. S., Weller, J. A., Harshman, L. A., 2007. Stability of choices in a risky decision-making task: A 3-year longitudinal study with children and adults. Journal of Behavioral Decision Making 20, 241–252.

List, J. A., 2003. Does market experience eliminate market anomalies? The Quarterly Journal of Economics 118 (1), 41–71.

List, J. A., 2004. Neoclassical theory versus prospect theory: Evidence from the marketplace. Econometrica 72 (2), 615–625.

Loewenstein, G., 2000. Emotions in economic theory and economic behavior. American Economic Review 90 (2), 426–432.

Loewenstein, G. F., Weber, E. U., Hsee, C. K., Welch, N., 2001. Risk as feelings. Psychological Bulletin 127 (2), 267.

Lowe, S. R., Galea, S., 2017. The mental health consequences of mass shootings. Trauma, Violence, & Abuse 18 (1), 62–82.

Malmendier, U., Nagel, S., 2011. Depression babies: Do macroeconomic experiences affect risk taking? The Quarterly Journal of Economics 126 (1), 373–416.

Malmendier, U., Nagel, S., 2016. Learning from inflation experiences. The Quarterly Journal of Economics 131 (1), 53–87.

Malmendier, U., Tate, G., Yan, J., 2011. Overconfidence and early-life experiences: the effect of managerial traits on corporate financial policies. The Journal of Finance 66 (5), 1687–1733.

Mehra, R., Prescott, E. C., 1985. The equity premium: A puzzle. Journal of Monetary Economics 15 (2), 145–161.

Moya, A., 2018. Violence, psychological trauma, and risk attitudes: Evidence from victims of violence in Colombia. Journal of Development Economics 131, 15–27.

Muschert, G. W., 2007. Research in school shootings. Sociology Compass 1 (1), 60–80.

Necker, S., Ziegelmeyer, M., 2016. Household risk taking after the financial crisis. The Quarterly Review of Economics and Finance 59, 141–160.

Pástor, L., Stambaugh, R. F., Taylor, L. A., 2015. Scale and skill in active management. Journal of Financial Economics 116 (1), 23–45.

Paulsen, D. J., Platt, M. L., Huettel, S. A., Brannon, E. M., April 2011. Decision-making under risk in children, adolescents, and young adults. Frontiers in Psychology 2 (72), 1–6.

Pool, V. K., Stoffman, N., Yonker, S. E., 2012. No place like home: Familiarity in mutual fund manager portfolio choice. The Review of Financial Studies 25 (8), 2563–2599.

Pool, V. K., Stoffman, N., Yonker, S. E., 2015. The people in your neighborhood: Social interactions and mutual fund portfolios. The Journal of Finance 70 (6), 2679–2732.

Pool, V. K., Stoffman, N., Yonker, S. E., Zhang, H., 2019. Do shocks to personal wealth affect risk-taking in delegated portfolios? The Review of Financial Studies 32 (4), 1457–1493.

Saunders, E. M., 1993. Stock prices and Wall Street weather. American Economic Review 83 (5), 1337–1345.

Schildberg-Hörisch, H., 2018. Are risk preferences stable? Journal of Economic Perspectives 32 (2), 135–54.

Shiller, R. J., 1981. Do stock prices move too much to be justified by subsequent changes in dividends? The American Economic Review 71 (3), 421–436.

Shu, T., Sulaeman, J., Yeung, P. E., 2016. Cost of bereavement: How does parental loss affect mutual fund managers. Tech. rep., working paper.

Shultz, J. M., Thoresen, S., Flynn, B. W., Muschert, G. W., Shaw, J. A., Espinel, Z., Walter, F. G., Gaither, J. B., Garcia-Barcena, Y., O'Keefe, K., Cohen, A. M., 2014. Multiple vantage points on the mental health effects of mass shootings. Curr Psychiatry Rep 16 (469), 1–17.

Stigler, G. J., Becker, G. S., 1977. De gustibus non est disputandum. American Economic Review 67 (2), 76–90.

Vieider, F. M., 2018. Violence and risk preference: experimental evidence from Afghanistan: comment. American Economic Review 108 (8), 2366–82.

Voors, M. J., Nillesen, E. E., Verwimp, P., Bulte, E. H., Lensink, R., Van Soest, D. P., 2012. Violent conflict and behavior: a field experiment in Burundi. American Economic Review 102 (2), 941–64.

Vuori, M., Oksanen, A., Räsänen, P., 2013. Fear of crime in local communities after school shootings. Journal of Scandinavian Studies in Criminology and Crime Prevention 14 (2), 154–171.

Wang, A. Y., Young, M., 2020. Terrorist attacks and investor risk preference: Evidence from mutual fund flows. Journal of Financial Economics 137, 491–514.