Internet Appendix to "Do Natural Disaster Experiences Limit Stock Market Participation?"

A. Robustness of the Core Results to the Imputation

We impute some missing values in the NLSY79 data to construct our main variables (the key dependent variables are risky asset market participation and risky asset share of the total portfolio for each household), thereby enhancing the power of our tests. In this section, we provide details of the imputation method and repeat our core results (Table III) using the data without imputation to confirm that such imputations do not have a material effect on our results.

According to the Bureau of Labor Statistics, some data are missing from the NLSY79 for several reasons. First, some respondents do not participate in the survey at all in certain years, so all information for those households is missing. The corresponding variables are coded as -5 (noninterview). Second, some respondents do not provide valid answers to some questions. The invalid answers are flagged as either -1 (refusal) or -2 (dont know).[1] Finally, data can be missing when an interviewer does not follow the survey flow as instructed and neglects to ask respondents a set of questions that should have been answered. These variables are coded as -3 (invalid skip).

If the missing value flags are either -1, -2, or -3 but respondents provide corresponding lower- and upper-bound values, we impute those missing values as the middle points of the lower and upper bounds to enhance the power of our tests. For example, if a household respondent says she doesn't know the value of her stock holdings but provides its lower and upper bounds ($80,000 and $100,000 respectively), we impute the missing value as $90,000. If (i) such bounds are not available or (ii) missing values are coded as -5, but the respondents do not permanently drop out of the sample (i.e., those respondents are included in the subsequent survey years), we use linear interpolation to fill in the missing data for in-between survey years. For example, if a household holds $0 and $26,588 in risky assets in survey years 2004 and 2008 respectively, we interpolate the missing risky assets variable for the survey year 2006 as $13,099. For those households whose equity market participation status changed over the survey years, we infer their participation status for the years between surveys in the following manner. Consider a household that didn't participate in the risky asset market in 1993 and did participate in the market in 1998. The corresponding participation status variables are missing for the between-survey years 1994 and 1996. We first linearly interpolate the participation variable uniformly between 1993 and 1998. This yields a value of 0.20 for 1994 and 0.60 for 1996. We use the integer value of the interpolation

[1] The Bureau of Labor Statistics notes that the assignment of refusals and don't knows is likely to vary across interviewers and hence is somewhat arbitrary.

result as the inferred participation variable to incorporate the sticky nature of risky asset market participation (e.g., the fixed costs of stock market participation). In our example, we code the missing risky asset market participation variable for the between-survey years of 1994 and 1996 as 0 and 1 respectively (integer values of 0.20 and 0.60). By doing so, we conservatively consider such households to be nonparticipants in the year 1994. This approach increases our sample size by 27.6% and increases the power of our tests.

[Insert Table IA.1 here.]

Panel A of Table IA.1 repeats Table III of the main text. All the coefficients on ln(1+CUMNUM_OF_DISASTERS) remain significant both statistically and economically. We conclude that our results are insensitive to the imputation methods. Panel B reports the number of observations of our data with and without imputation by survey year.

B. NLSY79 Sampling Weights

We illustrate the intuition of sampling weights in NLSY79 data using examples and examine the sensitivity of our main results to the use of sampling weights.

1. Sampling Weights: Examples

A common survey practice is to oversample minority group members (e.g., Hispanics). Suppose a survey samples Hispanics at a rate that is five times their population proportion ($\frac{\textit{Hispanics Sample Size (\%)}}{\textit{Hispanics Population Size (\%)}} = 5$), which is the "sampling fraction" for Hispanics. Further, suppose that the sampling fraction for the majority group members (e.g., Whites) is 1. Design weights are used to compensate for this disproportionate stratification.

Sampling Weights (Type 1): Design Weights

Ethnicity	Sampling Fraction	Design Weights
Hispanic	5	0.20
Whites	1	1.00

To compensate for the over-sampling of Hispanics in the sample and to represent the entire population, the survey assigns a design weight to each respondent, which is defined as the inverse of the corresponding sampling fraction. Therefore, the weight for Hispanic respondents is $1/5 = 0.20$, which corrects for differential probabilities of selection and allows researchers to reconfigure the sample as if it was a randomly drawn sample from the population. This value (0.20) indicates how much each Hispanic respondent counts in a statistical procedure; that is, each Hispanic respondent represents 0.20 individuals in the population.

A second weight, known as poststratification (or nonresponse) weight, is used to compensate for the fact that persons with certain characteristics are not as likely to respond to a survey. For example, assume that females and males are equally distributed in the population (i.e., 50% females and 50% males). However, 60% are females, and 40% are males in the sample of completed interviews, say due to a higher (lower) response rate of females (males). Then the poststratification weight is defined as a ratio of population proportion to sample proportion: the weight is $0.5/0.6 = 0.83$ for the female group and $0.5/0.4 = 1.25$ for the male group.

Sampling Weights (Type 2): Poststratification (Nonresponse) Weights

Gender	Population Proportion	Sample Proportion	Poststratification Weight
Female	0.5	0.6	0.83
Male	0.5	0.4	1.25

In this simple case, a total sampling weight for each survey response is calculated as the product of the design weight and the nonresponse weight. This intuition underlies the weighting procedures of NLSY79, although their methods are much more complicated, with multiple layers of subsampling. More specifically, weighting decisions for the NLSY79 are guided by the following two principles: (a) individual case weights are assigned for each year in such a way as to produce group population estimates when used in tabulations, and (b) the assignment of individual respondent weights involves at least three types of adjustments. These three adjustments account for (i) the probability of selection of a particular household at the first interview; (ii) differential response (cooperation) rates in both the screening phase and subsequent interviews (differential cooperation rates are computed and adjusted on the basis of geographic location and group membership, as well as within-group sub classification); and (iii) attempts to correct for certain types of random variation associated with sampling as well as sample "undercoverage." These adjustments are used to conform the sample to independently derived population totals. The details of these procedures can be found in Chapter 4 of the NLSY79 Technical Sampling Report (Frankel, McWilliams and Spencer, 1983).

2. Robustness of the Core Results to Sampling Weights

Following the convention in the finance and economics literature (e.g., Malmendier and Nagel, 2011), we apply the NLSY79 sampling weights in all our regression specifications in the main text. Solon, Haider and Wooldridge (2015) suggest using sampling weights when estimating causal effects in regressions (i) to correct for endogenous sampling in which the probability of selection varies with the dependent variable, and (ii) to identify average partial effects in the presence of heterogeneous effects. Solon et al. (2015) also suggest discussing the sensitivity of regression inferences to sample weights. We reestimate Table III in the main text without using sampling weights provided by NLSY79 and compare these results with those obtained from using sampling weights.

[Insert Table IA.2 here.]

Table IA.2 shows that the statistical significance of the key variable, ln(1+CUMNUM_OF_DISASTERS), is virtually the same across all specifications in both estimations. We also calculate the economic significance, which is defined as the change in the dependent variable for a one standard deviation change in the independent variable around the mean, expressed as a percentage of the sample mean of the dependent variable. The results indicate that the estimated economic significance and the corresponding inferences (columns 1 and 4) are not sensitive to the use of sampling weights.

C. UBS/Gallup Survey vs. NLSY79

Since the UBS/Gallup survey data consist of a distinct set of respondents from that of the NLSY79, we discuss some observable differences in the characteristics of NLSY79 and UBS/Gallup surveys based on their summary statistics.

[Insert Table IA.3 here.]

First, the UBS/Gallup survey data is purely cross-sectional, and the composition of survey respondents changes every survey month, whereas NLSY79 surveys follow the same cohort of respondents over time. Second, since the population universe of the UBS/Gallup survey is households with total savings and investments of $10,000 or more, the respondents of the UBS/Gallup survey are biased toward more wealthy households relative to those of NLSY79. This results in four differences between two data sets as shown in Table IA.3: (i) higher average income of respondents in UBS/Gallup data: $99,117 for UBS/Gallup and $82,958 for NLSY79 (December 2014 dollars). We note that since the income variable in the UBS/Gallup survey is categorical, we use the middle points of the corresponding ranges; (ii) the fraction of Hispanic (or Black) respondents is lower in UBS/Gallup data; (iii) the fraction of respondents who completed college education is higher in UBS/Gallup data; (iv) UBS/Gallup survey respondents are older relative to the NLSY79 sample. For these reasons, we use the respective survey weights provided by both data sets while estimating regression parameters in the main text to ensure that our inferences are applicable to the U.S. population.

D. Political Nature of FEMA Declaration and Measurement Errors

After a disaster hits, the governor of each state submits a declaration request to the president through FEMA. Once it is approved by the president, the disaster is recorded in the FEMA database. Hence the disaster declaration process may have a political aspect. This can be inferred from Panel A1 of Figure I in the main text: for example, there is a discontinuous decline in FEMA disaster declarations around the border between Georgia and Florida, even though these states are geographically adjacent. Indeed, the political science literature documents some evidence (although it is mixed) that disaster declarations may be motivated by politics rather than by need. For instance, Garrett and Sobel (2003) find that the rate of disaster declaration is statistically higher in those states that are politically important (i.e., battleground states) to the president. They also examine whether the governor's political alignment with the president affects the number of presidential disaster declarations at the state level. They find this factor to be positively related to declarations but statistically insignificant in their regressions (Table 3 of Garrett and Sobel (2003)). Therefore, such political alignment is unlikely to affect our estimates of the effect of disaster experiences on the risk-taking behavior of households. In the FEMA data, we observe only the approved requests (i.e., final disaster declarations) but not the declined ones. For this reason, existing research, e.g., Garrett and Sobel (2003), when examining whether political influences affect the FEMA declaration decisions, use the ex post declaration data (e.g., the number of disaster declarations). In fact, Garrett and Sobel (2003) note, *"It would be of interest to explore what percent of disaster declaration requests by state governors were honored by the president. However, the number of disaster declaration requests was not available."*

The political nature of disaster declarations, if any, can induce both type I and type II errors in our key independent variable: disasters classified as such when they should not be and disasters not classified as such when they should have been. Politically motivated disaster declarations will presumably not be factored by households into their portfolio choice considerations, but our econometric models include them, thereby introducing measurement error in our independent variable. The estimated coefficient on disaster experiences in our regressions will therefore be an upper bound (i.e., the bias will be positive if the true coefficient, β, is negative), which will result in an underestimate of the true economic significance of disasters on portfolio choice. Gasper (2015) documents that politics results in some disasters being denied aid, especially in presidential election years. Presumably, households will factor these disasters into their portfolio choice, while our regression models will ignore them, again leading to measurement error in our independent variable. And again, the coefficient estimates in our regressions will be an upper bound that underestimates the true impact of disasters on portfolio choice. Given both of these effects, we believe our economic significance calculations are conservative. In making this argument, we rely on the following OLS result: assuming the measurement error in x (with variance σ_x^2) is u with mean 0 and variance σ_u^2, the bias in β is given as $-\frac{\sigma_u^2}{\sigma_u^2+\sigma_x^2}\beta$. If the true β is negative, this bias is always positive, i.e., attenuating our estimated β coefficient toward zero.

E. Additional Tests on the Interpretation of the Core Results

1. Early Life vs. Later-Life Experiences

In this section, we compare the effects of early life and later-life experiences on households' risk taking behavior. To see if households' early life disaster experiences affect their portfolio choices when they are adults, we regress their early life experiences on stock market participation and share of risky assets. The results are reported in Panel A of Table IA.4. This analysis does not include household fixed effects because early life experiences, by definition, do not vary within a household. We note that all of our inferences in the main paper are derived from regression specifications that explicitly control for household fixed effects.

[Insert Panel A of Table IA.4 here.]

Columns 1 and 4 show that, early life disaster experiences do not have any effect on risky asset market participation or risky asset share (the coefficients are positive and statistically insignificant) in our setting. Columns 2 and 5 show that later-life experiences (age 15 onward) matter: the coefficients are negative and significant even after controlling for early life experiences. Finally combining all experiences into one variable in columns 3 and 6, we show that the estimated effect remains the same in magnitude and statistical significance for both risky asset market participation and risky asset share (compared to the coefficients on the later-life experiences variable in columns 2 and 5 respectively). We conclude from these results that early life experiences have little effect on households' asset allocation decisions.

2. Estimating the Weighting Scheme to Explain Households' Risk-Taking Behavior

Disaster experiences in the distant past may have a different influence on households' risk-taking behavior than more recent experiences. To estimate a weighting scheme of households in portfolio choice decisions, we use the following nonlinear regression model (Malmendier and

Nagel, 2011):

$$\text{RISKY\_SHARE}_{it} = \beta\, N_{it}(\lambda) + \gamma' X_{it} + a_{it} + \tau_t + \epsilon_{it} \tag{IA.1}$$

$$N_{it}(\lambda) = \sum_{k=0}^{\text{AGE}_{it}-1} w_{it}(k,\lambda) \cdot \text{NUM\_DISASTERS}_{i,t-k}$$

$$w_{it}(k,\lambda) = \frac{(\text{AGE}_{it} - k)^{\lambda}}{\sum_{k=0}^{\text{AGE}_{it}-1} (\text{AGE}_{it} - k)^{\lambda}}, \tag{IA.2}$$

where RISKY_SHARE refers to the fraction of liquid assets invested in risky assets, X_{it} is a vector of control variables, a_{it} and τ_t are age and year fixed effects, respectively, $\text{NUM\_DISASTERS}_{i,t-k}$ is the total number of disaster experiences of household i at year $t-k$, and $N_{it}(\lambda)$ is a weighted-average number of disaster experiences where the weights are given by $w_{it}(k,\lambda)$. The parameter λ determines the curvature of the relation between weights and how old the experiences are, and k refers to how many years ago a household experienced disasters. For example, as illustrated in Figure IA.1, if $\lambda < 0$, the weight is increasing (and always convex) in k. If $\lambda = 0$, the weight is constant; hence $N_{it}(\lambda)$ is a simple average of disaster experiences (not shown in the figure). If $\lambda > 0$, the weight is decreasing (concave for $\lambda < 1$, linear for $\lambda = 1$, and convex for $\lambda > 1$) in the lag k. We estimate β and λ simultaneously using nonlinear least squares.

[Insert Figure IA.1 and Panel B of Table IA.4 here.]

Panel B of Table IA.4 presents the estimated effect of the weighted average of disaster experiences on the fraction of liquid assets invested in risky assets. The weighted average number of disasters has a statistically significant negative effect on the percentage invested in risky assets. Importantly, the point estimate for λ, 3.939, implies that households put more weight on recent disaster experiences in making asset allocation decisions: the weight is decreasing and convex in the time lag between the disaster occurrence and the current survey year (Figure IA.1). This estimate is similar to that of Malmendier and Nagel (2011), who use a completely different data set about stock market return experiences, in the sense that both estimated weights are decreasing ($\lambda > 0$) and convex ($\lambda > 1$) in the time lag. The marginal effect of a one standard deviation increase in disaster experiences for a hypothetical 50-year-old household head is -2.3 percentage points, a 6.1% decrease relative to the sample mean.

F. Damages of U.S. Natural Disasters

In this section, we provide descriptive statistics on damages of natural disasters in the United States. The Federal Emergency Management Agency (FEMA), our main source of natural disaster data, unfortunately does not report any damages of FEMA-declared disasters. An alternative data we may consider is SHELDUS Presidential Disaster Declaration (PDD) database that provides several measures of disaster damages (property damage, property damage per capita, injuries, and fatalities). However, Gallagher (2014) raises a serious concern regarding the reliability of SHELDUS data. For example, only 99 out of 1,151 PDD flood events (8.6%) from 1960 to 2007 are labeled in SHELDUS and many of counties included in 99 PDD events are listed in SHELDUS with no flood damage (see the online appendix of Gallagher (2014) for details). We briefly describe some of the obvious, erroneous estimates of property damages we observe from SHELDUS data in Section F.1. We also report summary statistics on the damages of "super-severe" FEMA disasters that we identify using EM-DAT, which has been used in the literature (e.g., Boustan, Kahn, Rhode and Yanguas, 2019). EM-DAT is a global database on natural disasters that is maintained by the Centre for Research on the Epidemiology of Disasters (CRED) at the School of Public Health of the Université Catholique de Louvain, Brussels, Belgium. We discuss details about the data and procedure of identifying "super-severe" FEMA disasters in the following section.

1. Notes on Using SHELDUS Data

We document some of our observations on the erroneous estimates of property damages in SHELDUS-PDD data we download at the county-year-month level on Feb 11, 2019. We manually match corresponding natural disaster events from various sources and compare the damage estimates from SHELDUS with those from other sources.

i. Los Angeles county, California on January 1994:
This observation corresponds to "1994 Northridge earthquake" with estimated total damage amounting $13-$44 billion (equivalent to $22-74 billion 2000) according to Petak and Elahi (2000). However, in SHELDUS-PDD, this month (a single month for a single county) has property damage of $767 billion in 2014 dollars, which is much greater than $22-$74 billion.

ii. Monroe, Collier, Broward, and Miami-Dade counties, Florida on August 1992:
These observations correspond to "Hurricane Andrew" with estimated total damage amounting $27.3 billion (in 1994 dollars) according to the report, *Costliest U.S. tropical cyclones tables update*, by United States National Hurricane Center on January 12, 2018. However, in SHELDUS-PDD, this month for each of these counties has property damage of $75 billion in 1994 dollars. Even though we remove duplicate values in these counties, $75 billion (other counties in and near Florida on August 1992 have relatively negligible damage values) is far greater than $27.3 billion.

iii. Many counties in Louisiana, Mississippi, Alabama, and Florida on August 2005:
These observations correspond to "Hurricane Katrina" with estimated total damage amount-

ing \$125 billion (in 2005 dollars) according to Knabb, Rhome, Brown and Center (2005).[2] In SHELDUS-PDD, the total damage for this month across all counties (after removing duplicate values by manual inspection) amounts \$205.9 billion (in 2005 dollars).

iv. Kauai county in Hawaii on September 1992:
This observation corresponds to “Hurricane Iniki” with estimated total damage amounting \$3.1 billion (in 1992 dollars) according to the report, *Costliest U.S. tropical cyclones tables update*, by United States National Hurricane Center on January 12, 2018. The damage estimates in SHELDUS-PDD for this month-county amounts \$43.2 billion (in 1992 dollars).

v. Linn county in Iowa on June 2008:
This observation corresponds to “Iowa flood of 2008” with estimated total damage amounting \$6 billion (in 2008 dollars) according to the Mitigation Assessment Team Report by FEMA, *Midwest Floods of 2008 in Iowa and Wisconsin*, in 2009.[3] However, in SHELDUS-PDD, the total damage for this month-county amounts \$38.5 billion (in 2014 dollars).

vi. Skamania county in Washington on May 1980:
This observation corresponds to “1980 eruption of Mount St. Helens” with estimated total damage amounting \$1.1 billion (in 1980 dollars) according to the International Trade Commission.[4] In SHELDUS-PDD, the total damage for this month-county amounts \$13 billion (in 1980 dollars).

vii. Alameda county in California on October 1991:
This observation corresponds to “Oakland firestorm of 1991” with estimated total damage amounting \$1.5 billion (in 1991 dollars) according to Oakland Office of Fire Services (January 1992). In SHELDUS-PDD, the total damage for this month-county amounts \$20.4 billion (in 1991 dollars).

viii. Shelby county in Tennessee on May 2011:
This observation corresponds to “2011 Super Outbreak” with estimated total damage amounting \$11 billion (in 2011 dollars) according to the report by A.M. Best Company Inc. on May 18, 2011. In SHELDUS-PDD, the total damage for this month-county amounts \$28 billion (in 2011 dollars).

Based on the above observations and a cautionary note on the use of SHELDUS data in Gallagher (2014), we do not use SHELDUS-PDD data in reporting damages of disasters.

[2]See also `https://www.nhc.noaa.gov/news/UpdatedCostliest.pdf`
[3]`https://www.fema.gov/media-library-data/20130726-1722-25045-0903/fema_p_765.pdf`
[4]`https://www.usitc.gov/publications/332/pub1096.pdf`

2. Descriptive Statistics of "Super-Severe" FEMA Disasters based on EM-DAT

To shed some light on damages of "Super-Severe" FEMA disasters used in our analysis, we rely on the damage estimates from EM-DAT. EM-DAT includes all disasters from 1900 until the present and each event must satisfy at least one of the following criteria to be entered into the database: (i) 10 or more people dead, (ii) 100 or more people affected/injured/homeless, or (iii) declaration by the country of a state of emergency and/or an appeal for international assistance. We note that such selection is not an issue for our analysis since we only need to identify super-severe disasters. The database contains start/end dates, affected areas (only up to state-level), disaster type, total deaths, total affected (the sum of the injured, affected and left homeless after a disaster), total damage, disaster name (if any), etc.

[Insert Table IA.5 here.]

We manually match FEMA disasters with EM-DAT disasters to draw information on fatalities from the EM-DAT data set using the following procedures: First, we use disaster name in EM-DAT and disaster title in FEMA data to match these two data sources (note that there could be multiple FEMA declarations per EM-DAT disaster); second, we use disaster type and begin date and manually double check matches using affected area information in EM-DAT (if necessary, we reclassify disaster type variable in EM-DAT to make it consistent with that in FEMA, for example, replacing landslides with mud/landslide or volcanic activity with volcano, etc.); finally, for all unmatched EM-DAT disasters, we manually inspect them to find corresponding FEMA disasters. We then define disasters as super-severe if their deaths exceed the median (11 deaths) of the total death distribution of non-zero observations in EM-DAT from 1964 to 2013. A similar approach is employed by Boustan et al. (2019) who use the same database, EM-DAT. From 1964 to 2013, the total number of FEMA disaster declarations is 3,061 and that of EM-DAT super-severe disasters (with at-or-above 11 total deaths) is 264. Out of 3,061 FEMA disasters, we identify 709 FEMA disasters (23%) as super-severe disasters using EM-DAT, with a corresponding number of EM-DAT disasters being 166 (63% out of 264 EM-DAT super-severe disasters). Panel A of Table IA.5 summarizes the coverage of FEMA and EM-DAT super-severe disasters. In Panel B of the same table, we report summary statistics on damages of super-severe FEMA disasters identified by EM-DAT disasters. The median (mean) of CPI-adjusted cost is $716.7 billion ($4,039.9 billion) and its standard deviation is $13,690.9 billion, which indicates the damage distribution is highly right-skewed. Similarly, the death distribution is right-skewed.

G. Details on Using the NCEI Disaster Data

In the main text, we assess whether and how political considerations in the FEMA disaster declaration affect our inferences by utilizing the NCEI disaster database.[5] We provide details on the procedure of matching the FEMA database and the NCEI database below.

The NCEI database is maintained by the U.S. National Oceanic and Atmospheric Administration's National Centers for Environmental Information (NCEI). It consists of two distinct data sets: the NCEI Billion-Dollar Disasters database and the NCEI Storm Events database. The NCEI Billion-Dollar Disasters data cover super-severe natural disasters in the United States from 1980 to the present day. Each disaster's losses exceed 1 billion in today's dollars. To detect the impact of political biases if any, in the declaration of super-severe disasters, we manually match each NCEI billion-dollar disaster with the FEMA database using the following procedures. First, we match the disaster name in the NCEI database and the disaster title in the FEMA database. We note that there could be multiple FEMA declarations for one NCEI billion-dollar disaster event. Second, we match the disaster type and the disaster beginning date across the two databases. If necessary, we rationalize the incident type in the FEMA database to make it consistent with the description in the NCEI database. For example, we replace tornado or coastal storm in the FEMA database with severe storm in the NCEI database and replace snow in the FEMA database with severe ice storm in the NCEI database. Finally, for all NCEI billion-dollar disasters not matched with the FEMA database, we manually inspect each entry in the FEMA database around the time period of the NCEI event to find the corresponding FEMA database entries.

This matching exercise suggests that the FEMA disasters cover 96% of the 160 NCEI billion-dollar disasters by number for the period from 1980 to 2013. This overlap corresponds to 99% of the total losses ($0.968 trillion) of these 160 NCEI billion-dollar disasters. Panel A in Table IA.6 summarizes these findings. We conclude that almost all super-severe disasters in the NCEI database are covered by the FEMA data.

[Insert Table IA.6 here.]

We then examine the overlap between the NCEI and FEMA database in coverage of disasters with below $1 billion in losses. To this end, we use the NCEI Storm Events database (version 3.0) that provides granular information on U.S. disasters at the county level. The data contain the occurrence of storms and other significant weather phenomena from January 1950 to October 2020,

[5] See Section D for the detailed discussion on the political nature of declaration procedure and consequential econometric biases due to measurement errors.

as entered by the NOAA's National Weather Service (NWS). The database has gone through significant changes in the data collection and processing procedures over time. The database only recorded tornadoes between 1950 and 1954 and only three types of disaster events (tornado, thunderstorm wind, and hail) between 1955 and 1995. Starting from 1996, the coverage of Storm Events database was significantly expanded —an additional 45 different types of disaster events were included in the database. Therefore, from 1964 to 1995 (which is 64% of our entire sample period), the Storm Events database does not cover 15 out of the 17 types of disasters included in our FEMA database (hail is not included in the FEMA database). The events missing in the NCEI database are very significant natural disasters in the United States and include floods, snow, hurricanes, droughts, fires, and earthquakes among other events. Therefore, for the period 1964–1995, the FEMA database provides the best coverage among all available data.

For the period from 1996 to 2013, we first identify severe and super-severe disasters in the NCEI Storm Events database that are not covered by the FEMA database. Panel B of Table IA.6 outlines the entire step-by-step procedure we used. The unit of observation of disaster events in the NCEI database is at the county level. It appears that 0.14% (1,400 county events) to 0.64% (6,462 county events) of the NCEI database consists of severe and super-severe disasters that are not covered by the FEMA database, depending on the assumptions and criteria used. However, adding these events to the FEMA database (a total of 43,350 county events) increases its coverage between 3.23% (= 1,400 / 43,350) and 14.91% (= 6,462 / 43,350)—a modest to a sizeable increase depending on the assumptions used.

We now consider the reverse scenario: Some declared disasters in the FEMA database might be politically motivated, but not severe or super severe enough to merit inclusion in the NCEI database. For the period from 1996 to 2013, we identify county-level events in the FEMA database that are not covered by the NCEI Storm Events database. Many of these county-level events happen at the same point in time (pertaining to the same natural disaster) and thus we obtain 155 unique natural disasters that span multiple counties at the same time. By tracking newspaper stories on these natural disasters (using LexisNexis), we confirm 76 out of the 155 natural disasters are either severe or super-severe events in the United States, but these are not the types of disasters covered by the NCEI Storm Events database. For instance, the list includes earthquakes, the West Nile virus outbreak, power outages due to storms or low temperatures (similar to the recent power outage in Texas in February 2021), explosions, gas leaks, bridge collapse, warehouse fires, etc. These 76 natural disasters correspond to 534 county-level events in the FEMA database. We are unable to obtain independent confirmation from the newspapers for the remaining 79 out of the 155 natural disasters that correspond to 132 county-level events. Thus, in our estimation at most 0.30% (132 county events out of 43,350 county events in the FEMA database) might be potentially politically motivated.

The above process effectively purges any concerns about the political nature of FEMA declarations clouding our inferences. Using these revised data (including 6,462 county events from the NCEI database and excluding 132 county events in the FEMA database for the period 1996–2013), we find that our results continue to be statistically significant, and they uniformly increase in economic significance across all specifications. These results are documented in Section III.D of the main text.

H. Propensity Score Matching and Rosenbaum Bounds

We explore additional robustness of our findings in the main text by using the matched sample. This exercise is to make sure that households experiencing a large number of disasters and those experiencing a small number of disasters have more balanced and overlapped distributions of pre-treatment intensity variables. We note, however, that while regression, in general, gives us a variance-based weighted average treatment effect, matching provides a distribution-weighted average treatment effect, thus differing only in their weighting schemes (Angrist, 1998). Moreover, as Angrist and Pischke (2009, pp.69–70) note, "*In other words, matching and regression are both control strategies. Since the core assumption underlying causal inference is the same for the two strategies, it's worth asking whether or to what extent matching really differs from regression. Our view is that regression can be motivated as a particular sort of weighted matching estimator, and therefore the differences between regression and matching estimates are unlikely to be of major empirical importance.*" Nevertheless, we use propensity score matching method to provide a useful robustness test for our regression-based main analysis.

[Insert Table IA.7 here.]

Since the exposure to the number of natural disasters is a continuous variable, we modify the standard matching procedure as follows: We designate households (with the same age) that have cumulatively experienced above-median number of disasters in any given year as the high-treatment-intensity group. We designate households that have cumulatively experienced below-median number of disasters in the same year as the low-treatment-intensity group. We then predict high-treatment-intensity group using a logistic regression (which includes all observable characteristics of our households as covariates) and generate propensity scores for the two samples. We use 1-nearest neighbor matching with a caliper of 0.1 to find a matched household in the low-treatment-intensity group for every household in the high-treatment-intensity group. The histograms in Panel B of Table IA.7 show the propensity scores (in logit scale) of the two samples before and after the matching exercise. As can be seen from these histograms, our matching procedure produces two samples that closely overlap with each other on all observed dimensions after the exercise (using the propensity score as a summary statistic for matching).

We then estimate the Average Treatment Effect of the Treated (ATT) for both the participation rate and the fraction of liquid assets invested in risky assets. We estimate standard errors for the ATT using bootstrapping with 100 replications. The results are presented in Panel A of Table IA.7. The results clearly show a greater number of disaster experiences is associated with more conservative portfolio choices (negative ATT in columns 1 and 3), and the results are strongly significant economically and statistically. As a robustness check, we also designate households (with residents of the same age) that experience disasters in the top quartile in any given year as the high-treatment-intensity group and households that experience disasters in the bottom quartile in the same year as the low-treatment-intensity group. The results in columns 2 and 4 are stronger both economically and statistically than those in columns 1 and 3.

In Panel C of the same table, we report the results of a Rosenbaum bounds sensitivity analysis to examine whether unobserved factors can alter inference about the treatment effects reported in Panel A (Rosenbaum, 2002) (also known as hidden bias). We report the Mantel-Haenszel statistics under the assumptions of positive (negative) selections while setting the level of hidden bias to a certain value of $\exp(\gamma)$ where γ captures the marginal effect of unobserved variable on households being treated. If our analysis is free from hidden bias, γ will be zero and the propensity score will be solely determined by observables. In other words, $\exp(\gamma)$ of 1 refers that unobservable factor(s) has no effect on how each household-year observation is assigned to the high-treatment-intensity group or to the low-treatment-intensity group. For values of $\exp(\gamma)$ higher than 1, for example 2, households who appear to be similar in terms of observables could differ in their odds of receiving the treatment by as much as a factor of 2.

Under the positive (negative) selection, households who have experienced above the median number of disasters in any given year-age also have a higher (lower) probability of participating in risky asset markets: the estimated treatment effects will be positively (negatively) biased relative to the true treatment effect. Since our estimated treatment effect is negative, the bounds under the positive assumption (in columns 1 and 3) are less of our interest: potential hidden bias goes against finding the negative effect. The p-values in column 4 indicate that our analysis is insensitive to a potential hidden bias due to negative selection.

I. Indirect Disaster Experiences using Adjacent Counties

It is possible that individuals change their risk-taking behavior simply by observing salient events, without having experienced such events in person (e.g., the results documented in Dessaint and Matray (2017) for the case of firm managers). To examine whether households change their asset allocation in response to an "indirect" disaster experience, we construct a measure for each household by counting the number of disasters that occurred in the adjacent counties of the current residence of households. We count such disasters to form this measure only when these disasters did not affect the county of the current residence of the household (i.e., the household is just outside the disaster-affected area). We then run a horse-race regression that includes both the indirect and the direct (our original measure) disaster experience variable to explain the portfolio choices of the household.

[Insert Table IA.8 here.]

Table IA.8 presents the results: disasters in the adjacent counties lower households' risky asset market participation (negative sign on the coefficient); however, the effects are not statistically significant. It is important to note that the coefficient on the direct disaster experiences, our main measure, remains significant at the 1% level, and the magnitude of the effect barely changes (from −0.029 in the baseline specification to −0.029 in this test). We obtain similar results in the risky asset share regression. Overall, we conclude that personal experiences matter in portfolio choice. However, observing disasters in the adjacent counties seems to have some, albeit statistically weak, impact on the individuals' portfolio choice decisions, possibly by updating their expectations.

J. Additional Tests on Shocks to Local Economy

Disasters may affect local economic conditions and uncertainty by damaging the local economy, and consequently households equity holdings. Therefore, we include county-by-year fixed effects in our main specifications in the main text. In this section, we additionally examine whether disaster experiences proxy for this effect by including a another set of geographic location fixed effects: state, state-by-year fixed effects. These fixed effects control for any state-level unobserved factors that might drive both disaster experiences and stock market participation. We also explicitly control for macroeconomic conditions at the state level by including GDP growth, population, population density, and unemployment rates in our specifications. We obtain annual state-level GDP growth rates from the Bureau of Economic Analysis, state population and population density from the U.S. Census Bureau, and state-level unemployment rates from the Bureau of Labor Statistics for our sample period. We report these results in Table IA.9. We find robust evidence that household stock market participation and risky asset share decrease following disaster experiences in all specifications. These results demonstrate that potential changes in the local economic conditions cannot drive the effect of disaster experiences.

[Insert Table IA.9 here.]

K. The Effects of Socioeconomic Status and Its Changes Due to Disasters

Motivated by Das, Kuhnen and Nagel (2020), we examine, in the main text, the socioeconomic status of households as one of the channels through which disaster experiences affect households' risk-taking behavior. In this section, we provide the details of the estimation procedures and results. Individuals with higher socioeconomic status (SES) tend to become more optimistic about future macroeconomic variables, such as business conditions, the unemployment rate, and stock market returns (Das et al., 2020). Hence we explore if there is any differential effect of disaster experiences on portfolio choice at different levels of SES and if disaster-induced changes in SES then lead to changes in the risk-taking behavior. To this end, we divide our sample of households into four groups.

First, following Das et al. (2020), we use household income as a measure of SES and classify households in the same age into two groups based on the median of income distribution for each survey year: high- and low SES groups.[6] Next, we divide our households into two groups based on how sensitive their income is to disaster experiences. The first group consists of households with positive SES_SENSITIVITY. These households experience an increase in income (on average) following disaster shocks. The second group consists of households with negative SES_SENSITIVITY. These households experience a decrease in income (on average) following disaster shocks. Thus we obtain four groups of households used in our analysis: households in the low SES group with negative SES_SENSITIVITY, households in the low SES group with positive SES_SENSITIVITY, households in the high-SES group with negative SES_SENSITIVITY, and households in the high-SES group with positive SES_SENSITIVITY. We outline the procedure used to calculate SES_SENSITIVITY below.

SES_SENSITIVITY is calculated in three steps. In step one, we run a pooled regression of the log of income on the same set of demographic control variables used in Table III of the main

[6] We opt not to use college education, another measure of SES used in Das et al. (2020), as a measure of SES in our analysis since there is little variation in college education within a household.

text with household, age, and year fixed effects:

$$ln(1 + \text{INCOME}_{it}) = \gamma' Y_{it} + h_i + a_{it} + \tau_t + \epsilon_{it}, \tag{IA.3}$$

where $ln(1 + \text{INCOME}_{it})$ is log of income for household i at year t, Y_{it} is a vector of control variables (number of children, number of children squared, liquid assets, liquid assets squared, indicator variables for completed high school or college education, marital status, race, and gender), and h_i, a_{it}, and τ_t indicate household, age, and year fixed effects, respectively.

In step two, for each household-year observation, we calculate a ratio of changes in the residuals (ϵ_{it}) to changes in the disaster experience variable as long as the denominator is non-zero, i.e., $\text{RATIO}_{it} = \frac{\Delta\epsilon_{it}}{\Delta ln(1+\text{CUMNUM\_OF\_DISASTERS}_{it})}$.

In step three, for each household, we define SES_SENSITIVITY as a time-series average of these ratios. We interpret a positive (negative) sensitivity as a household whose SES is positively (negatively) affected by disasters. The cross-sectional average of the sensitivity is -0.104, which implies that an average household suffers income loss due to natural disasters.

Results: We first rerun our baseline regressions (columns 2 and 5 in Table III of the main text) by conditioning the log of cumulative disaster experiences on high and low SES group indicators. We expect the households in the low SES group to be more affected by disaster experiences than those in the high-SES group in making their portfolio choice decisions.

[Insert Panels A and B of Table IA.10 here.]

The results are reported in Panel A of Table IA.10. For the risky asset market participation decision (column 1), the coefficients of the cumulative disaster experiences for the low- and high-SES groups are -0.043 and -0.020, respectively. These coefficients are statistically significant and economically large. This suggests that all households are affected by their disaster experiences and make conservative portfolio choice decisions after disaster shocks regardless of their SES. We also observe that households in the low SES group *are more conservative* than households in the high SES group in their participation decisions. The difference between these coefficients (-0.023) is statistically significant at the 1% level and economically large. The results are similar for risky asset share as reported in column 2. We conclude that disaster experiences affect portfolio choices disproportionately more for low SES households than for high SES households.

We next rerun our baseline regressions (column 2 in Table III of the main text) by conditioning the log of cumulative disaster experiences on both SES and SES_SENSITIVITY. We expect that within a socioeconomic class, the negative SES sensitivity households make portfolio choices that are more conservative than the positive SES sensitivity households to guard against disaster-induced income shocks. The results are reported in Panel B of Table IA.10. We first discuss the results for the low SES group. For the risky asset market participation (column 1), the coefficients of the cumulative disaster experiences for the negative and positive SES sensitivity groups are -0.052 and -0.034, respectively. These coefficients are statistically significant at the 1% level and economically large. This suggests that for all low SES households, disaster experiences are associated with conservative portfolio choice. However, by comparing the coefficients between the negative and positive SES sensitivity groups, we show that households with negative income shocks *are more conservative* than households with positive income shocks in their participation decisions. Indeed, a formal test of the difference between these coefficients (-0.018) is statistically significant at the 5% level. These results indicate that among the low SES group, the negative SES sensitivity group is 53% much less likely to participate in the stock market than the positive SES sensitivity group, even if they had the same disaster experiences: 53% (=[0.052 - 0.034] / 0.034) is obtained by comparing the relative magnitude of the coefficients for the two groups.

Next, we turn to the results for the high-SES group. All high SES households are conservative in their participation decisions regardless of the income shocks. The coefficients of the cumulative disaster experiences for the negative and positive SES sensitivity groups are -0.022 and -0.021, respectively, which are statistically significant. However, the difference in coefficients between the negative and positive SES sensitivity groups is -0.001 and insignificant. This suggests that negative income shocks do not have differential effects on the behavior of high SES households.

The risky asset share results in column 2 show that households affected by negative income shocks are more conservative in their risky asset share compared to households with positive income shocks. This result holds for both low and high SES groups. Based on these results, we conclude that socioeconomic status is an important conditioning variable that sheds light on how households react to their disaster experiences in making portfolio choice decisions.

L. Construction of Risk-Aversion and Expectations Measures

1. Risk-Aversion Measure

The risk-aversion measure that we use in the main text has four distinct categories ranging from 1 (least risk averse) to 4 (most risk averse). It is constructed from the following sequence of three survey questions on the NLSY79 of 1993, 2002, 2004, and 2006: "Suppose that you are the only income earner in the family and you have a good job guaranteed to give you your current (family) income every year for life. You are given the opportunity to take a new and equally good job, with a 50–50 chance that it will double your (family) income and a 50–50 chance that it will cut your (family) income (i) by one-third, (ii) by half, and (iii) by 20 percent. Would you take the new job?" If respondents accept the first offer (i), they are given the second offer (ii); if they reject the first offer (i), they receive the third offer (iii). Respondents who accept the second offer receive a risk-aversion measure of 1; respondents who only accept the first offer have a risk-aversion measure of 2; respondents who accept the third offer receive a risk-aversion measure of 3; respondents who do not accept any offer have a risk-aversion measure of 4.

2. Expectations about Stock Market Return and Volatility over the Next Twelve Months

Households' expectations about the stock market over the next twelve months are obtained from the UBS/Gallup survey through the Roper Center at the University of Connecticut. We work with the responses to the following two questions about stock return expectations: (i) "Thinking about the stock market more generally, what overall rate of return do you think the stock market will provide investors during the coming 12 months?" and (ii) "(INTERVIEWER: Do NOT ask; code only whether a 'positive' or 'negative' number. If you are unsure whether the number is positive or negative, then ask the respondent. As a general rule, you should ASSUME it to be POSITIVE, unless the respondent explicitly says 'Minus'; or in some other way indicates the number is NEGATIVE)." The first question is an open-ended question and coded as the actual percentage, whereas the second question indicates only the signs of answers to the first question (1 = Positive; 2 = Negative). The answers to both questions are available for every month from January 2000 to April 2003. However, we drop four monthly data sets for January 2003 through April 2003 since they do not contain state-level residence information after January 2003. We eliminate observations with expected stock returns greater than 75% or less than -75%.

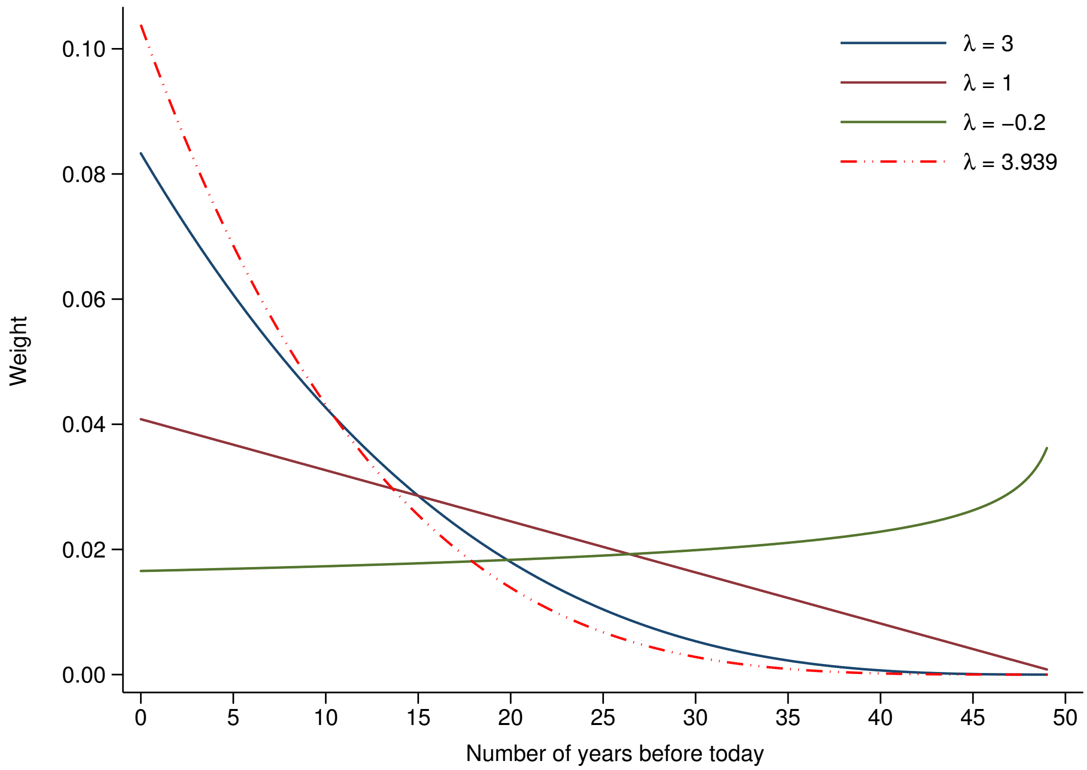
The survey also asks respondents about the expected stock market volatility over the next twelve months using the following question: "Do you think the amount of volatility in the marketplace during the next twelve months will increase, stay at the same level, or decrease from what it has been during the last several months?". The response to the question has three categories: 1 (Increase), 2 (Stay at the same level), and 3 (Decrease). The expected stock market volatility dummy variable is set to one if respondents expect an increase in volatility, and zero otherwise. The answers to the question are available for every month from May 1998 to December 2000; in 1998, however, the data are available only for May, September, and November.

M. Coefficients on Control Variables in Table III

In Table III of the main text, for brevity, we do not report the coefficients on control variables. Table IA.11 reports the complete table with all coefficients on control variables.

[Insert Table IA.11 here.]

Figure IA.1: Household Weighting Scheme on Disaster Experiences: In Case of a 50-year-old Household Head



The figure is based on the following weighting method as in Malmendier and Nagel (2011):

$$w_{it}(k, \lambda) = \frac{(\text{AGE}_{it} - k)^{\lambda}}{\sum_{k=0}^{\text{AGE}_{it}-1} (\text{AGE}_{it} - k)^{\lambda}}$$

The y-axis shows weights and the x-axis is the time lag (k) between disaster occurrence and today. By changing the value of λ, we can change the shape of weights as a function of how old the experiences are. Specifically, if $\lambda < 0$, the weight is increasing (and always convex) in k. If $\lambda = 0$, the weight is constant; hence $N_{it}(\lambda)$ is a simple average of disaster experiences (not shown in the figure). If $\lambda > 0$, the weight is decreasing (concave for $\lambda < 1$, linear for $\lambda = 1$, and convex for $\lambda > 1$) in k. λ of 3.939 depicted in this figure (red dotted line) is the actual estimate from nonlinear least squares (equation IA.1 and Table IA.4).

Table IA.1: Risky Taking Behavior and Disaster Experiences: Without Imputation

Panel A repeats Table III in the main text using the data without imputation: columns 1 and 2 report results from linear probability models of risky asset market participation on households' disaster experiences; columns 3 repeats column 2 by restricting the sample to households with above-median financial assets based on the survey year of 1988; columns 4 and 5 show OLS regressions of fraction of liquid assets invested in risky assets on households' disaster experiences. To conserve space, we omit the estimates of the other control variables. The average fitted values are calculated at various levels of the disaster experience variable, keeping all the other predictor variables at their sample mean. Numbers in square brackets under *Diff. between two fitted values* indicate the difference between two fitted values relative to the unconditional sample mean of the dependent variable (which is 0.429 and 0.346 in the sample). The sample period runs from 1988 to 2012. Numbers in parentheses are standard errors that are clustered by county. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. Panel B reports the number of observations for the data with and without imputation by survey year.

Panel A. Without Imputation

	PARTICIPATION			RISKY_SHARE	
	Whole Sample		Above-Median Wealth	Whole Sample	
	1	2	3	4	5
ln(1+CUMNUM_OF_DISASTERS)	-.024***	-.022**	-.024*	-.020**	-.018**
	(.009)	(.010)	(.015)	(.008)	(.009)
Income & Liquid Assets	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Household F.E.	Yes	Yes	Yes	Yes	Yes
Age and Year F.E.	Yes	No	No	Yes	No
Age and County-by-Year F.E.	No	Yes	Yes	No	Yes
Fitted values at $\mu - \sigma$ of DE (=2)	0.458			0.370	
Fitted values. at μ of DE (=7)	0.427			0.345	
Diff. between two fitted values	-0.030*** [-7.00%]			-0.024** [-6.94%]	
Fitted values at μ of DE (=7)	0.427			0.345	
Fitted values at $\mu + \sigma$ of DE (=12)	0.414			0.335	
Diff. between two fitted values	-0.013*** [-3.03%]			-0.011** [-3.18%]	
Unconditional sample mean	0.429			0.346	
# Obs.	84,436	84,436	42,754	60,359	60,359
Adjusted R^2	.621	.627	.582	.600	.606

Panel B. Number of Observations by Survey Year

Years	1988	1989	1990	1991	1992	1993	1994	1996	1998
w/o Imputation	8,991	9,020	8,771	-	7,478	7,413	7,036	6,820	6,571
w/ Imputation	8,991	9,020	8,771	7,469	7,478	7,413	7,204	6,887	6,607

Years	2000	2002	2004	2006	2008	2010	2012	Total
w/o Imputation	6,202	-	5,687	-	5,515	-	4,932	84,436
w/ Imputation	6,217	5,016	5,771	5,528	5,581	4,891	4,932	107,776

Table IA.2: Risk-Taking Behavior and Disaster Experiences: With/Without Sampling Weights

This table repeats Table III in the main text without using sampling weights and presents these results with the original results obtained from using sampling weights: columns 1 and 2 report results from linear probability models of risky asset market participation on households' disaster experiences; columns 3 repeats column 2 by restricting the sample to households with above-median financial assets based on the survey year of 1988; columns 4 and 5 show OLS regressions of fraction of liquid assets invested in risky assets on households' disaster experiences. To conserve space, we omit the estimates of the other control variables. We calculate the economic significance, which is defined as the change in the dependent variable for a one standard deviation change in the independent variable around the mean, expressed as a percentage of the sample mean of the dependent variable. The sample period runs from 1988 to 2012. Numbers in parentheses are standard errors that are clustered by county. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	PARTICIPATION			RISKY_SHARE	
	Whole Sample		Above-Median Wealth	Whole Sample	
	1	2	3	4	5
[With Sampling Weights]					
ln(1+CUMNUM_OF_DISASTERS)	-.032***	-.029***	-.034**	-.023***	-.021**
	(.009)	(.010)	(.015)	(.008)	(.009)
[Without Sampling Weights]					
ln(1+CUMNUM_OF_DISASTERS)	-.025***	-.027***	-.027**	-.021***	-.021***
	(.009)	(.010)	(.013)	(.008)	(.008)
Income & Liquid Assets	Yes	Yes	Yes	Yes	Yes
HH Characteristics / HH F.E.	Yes	Yes	Yes	Yes	Yes
Age and Year F.E.	Yes	No	No	Yes	No
Age and County-by-Year F.E.	No	Yes	Yes	No	Yes
Economic Significance					
With Sampling Weights	[-7.89%]			[-6.76%]	
Without Sampling Weights	[-7.52%]			[-6.47%]	
# Obs.	107,776	107,776	54,774	81,566	81,566

Table IA.3: Summary Statistics: UBS/Gallup vs. NLSY79 Survey Data

This table provides summary statistics of UBS/Gallup survey data with those of NLSY79 (excerpted from Panel A1 of Table II in the main text). The population universe of UBS/Gallup survey is national *cross-section* of head of households or spouse in any household with total savings and investments of $10,000 or more. These monthly surveys are based on telephone interviews with a sample of approximately 1,000 adult investors, aged 18 or older. The surveys used a random-digit dial telephone method. Since the income variable in these surveys is categorical, we use the middle points of the corresponding ranges. The income variable is deflated by the CPI-U inflation rates into December 2014 dollars. INCOME is household's total annual income last year before taxes. HIGH_SCHOOL (COLLEGE) is an indicator that equals one if the respondent completed high school (college) education. HISPANIC (BLACK) indicates whether the respondent is Hispanic (Black). FEMALE is set to one if the respondent is female. Observations are weighted by the survey sample weights. The sample period is 2000-2002.

	Median		Mean	
Variables	UBS/Gallup	NLSY79	UBS/Gallup	NLSY79
INCOME ($)	88,841	66,079	99,117	82,958
HIGH_SCHOOL	1	1	0.94	0.91
COLLEGE	1	0	0.53	0.27
HISPANIC	0	0	0.02	0.06
BLACK	0	0	0.05	0.14
FEMALE	0	0	0.41	0.48
AGE	49	34	49.98	36.09

Table IA.4: Additional Tests on the Interpretation of the Core Results

Panel A compares the effects of early life (between the ages of 5 and 15 years) and later-life (after the age of 15 years) disaster experiences on risk-taking behavior. Columns 1–3 repeat column 1 in Table III in the main text, and columns 4–6 repeat column 4 in Table III without household fixed effects. Observations are weighted by the NLSY79 sample weights. The sample period runs from 1988 to 2012. Numbers in parentheses are standard errors that are clustered by household. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Early Life (Between the Ages of 5 and 15 years) vs. Later-Life (After the Age of 15 years) Experiences

	PARTICIPATION			RISKY_SHARE		
	1	2	3	4	5	6
ln(1+CUMNUM_OF_EARLY_DISASTERS)	.004	.005		.001	.002	
	(.004)	(.004)		(.003)	(.003)	
ln(1+CUMNUM_OF_LATER_DISASTERS)		-.010**			-.013***	
		(.005)			(.005)	
ln(1+CUMNUM_OF_EARLY_AND_LATER_DISASTERS)			-.010**			-.013***
			(.005)			(.005)
Income, Liquid Assets, HH Characteristics	Yes	Yes	Yes	Yes	Yes	Yes
Age and County-by-Year F.E.	Yes	Yes	Yes	Yes	Yes	Yes
# Obs.	107,776	107,776	107,776	81,566	81,566	81,566
Adjusted R^2	.563	.563	.563	.534	.535	.535

Table IA.4: Additional Tests on the Interpretation of the Core Results (continued)

Panel B reports estimated parameters from the following model using nonlinear least squares:

$$\text{RISKY\_SHARE}_{it} = \beta\, N_{it}(\lambda) + \gamma' X_{it} + a_{it} + \tau_t + \epsilon_{it}$$

$$N_{it}(\lambda) = \sum_{k=0}^{\text{AGE}_{it}-1} w_{it}(k,\lambda)\cdot \text{NUM\_DISASTERS}_{i,t-k}; \qquad w_{it}(k,\lambda) = \frac{(\text{AGE}_{it}-k)^{\lambda}}{\sum_{k=0}^{\text{AGE}_{it}-1}(\text{AGE}_{it}-k)^{\lambda}}$$

where RISKY_SHARE refers to the fraction of liquid assets invested in risky assets, X_{it} is a vector of control variables, a_{it} and τ_t are age and year fixed effects, respectively, $\text{NUM\_DISASTERS}_{i,t-k}$ is the total number of disaster experiences of household i at year $t-k$, and $N_{it}(\lambda)$ is a weighted-average number of disaster experiences, where the weights are $w_{it}(k,\lambda)$. We estimate β and λ simultaneously using nonlinear least squares. The sample period runs from 1988 to 2012. Observations are weighted by the NLSY79 sample weights. Numbers in parentheses are standard errors that are robust to heteroskedasticity. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

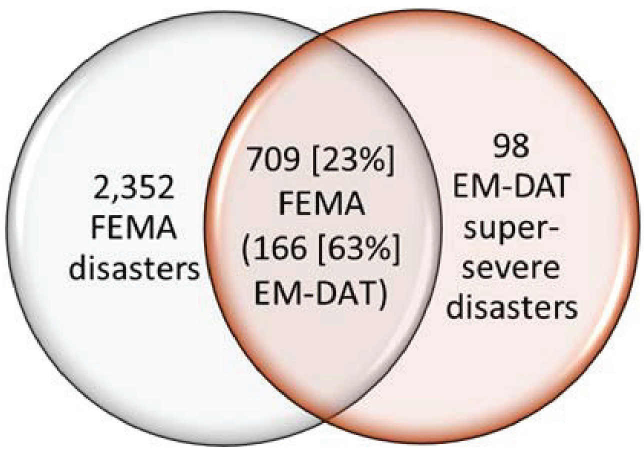
Panel B. Household Weighting Scheme

	RISKY_SHARE
WEIGHTED_DISASTER_EXPERIENCES ($N_{it}(\lambda)$)	-.037***
	(.007)
λ (weighting parameter)	3.939**
	(1.687)
ln(1+INCOME)	.031***
	(.002)
[ln(1+INCOME)]^2	-.002***
	(.000)
NUM_OF_CHILDREN	.015***
	(.002)
(NUM_OF_CHILDREN)^2	-.002***
	(.001)
HIGH_SCHOOL	.015***
	(.004)
COLLEGE	.000
	(.002)
ln(1+LIQUID_ASSETS)	-.011***
	(.002)
[ln(1+LIQUID_ASSETS)]^2	.004***
	(.000)
HISPANIC	-.005*
	(.003)
BLACK	0.023***
	(.003)
MARRIED	-.006**
	(.003)
FEMALE	-.005**
	(.002)
Age and Year F.E.	Yes
Marginal Effect of σ (=6) Increase in Disaster Experiences	-0.023
for Hypothetical 50-year-old Household Head	[-6.08%]
Unconditional sample mean	0.379
# Obs.	81,566
Adjusted R^2	.510

Table IA.5: Damages of Super-Severe FEMA Disasters based on EM-DAT

Panel A shows the Venn Diagram of FEMA disasters and EM-DAT super-severe disaster over the sample from 1964 to 2013. Out of all 3,061 FEMA disasters during this period, 709 FEMA disasters (23%) are covered by EM-DAT super-severe disasters (with at-or-above 11 total deaths) with the corresponding number of EM-DAT disasters being 166 (63% out of 264 EM-DAT disasters). We manually match EM-DAT disasters with FEMA disasters using the procedures described in Section F.2. Note that there could be multiple FEMA declarations for one EM-DAT disaster. Panel B reports summary statistics on the damages of super-severe FEMA disasters identified by EM-DAT. The economic damage is total amount of damage to property, crops, and livestock and it is deflated by the CPI-U inflation rates into December 2014 dollars.

Panel A. Venn Diagram of FEMA Disasters and EM-DAT Super-Severe Disasters (1964-2013)



Panel B. Summary Statistics on Damages of Super-Severe FEMA Disasters identified by EM-DAT

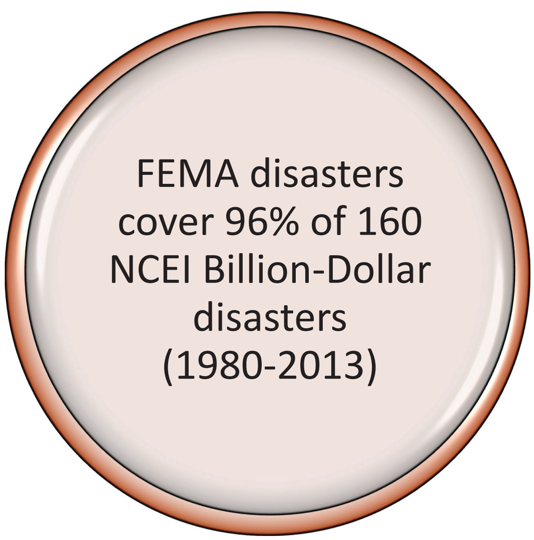
Damage Variables	Median	Mean	Std.Dev.	Number of Disasters	
				FEMA	EM-DAT
CPI-Adjusted Estimated Cost (million $)	716.7	4,039.9	13,690.9	709	166
Deaths	25.5	51.5	148.0		

Table IA.6: Augmenting the FEMA Database Using the NCEI Database

Panel A illustrates the fraction of NCEI billion-dollar disasters (available only from 1980 to 2013) that were covered by the FEMA disasters based on the number of disasters and the total dollar losses. The outer orange circle indicates the total number of NCEI billion-dollar disasters in panel (a) and the total dollar losses of these NCEI disasters in panel (b). In Panel B, we identify severe and super-severe disasters in the NCEI Storm Events database that were not covered by the FEMA database. We focus on the time period 1996–2013 because the NCEI Storm Events database only recorded three types of disaster events (tornado, thunderstorm wind, and hail) for the period 1955–1995. Panel B outlines the entire step-by-step procedure we used. The unit of observation of disaster event in the NCEI database is at the county level (County Events).

Panel A. The Coverage of the FEMA Database and NCEI Billion-Dollar Disasters Database

(a) number of disasters



(b) total $ loss

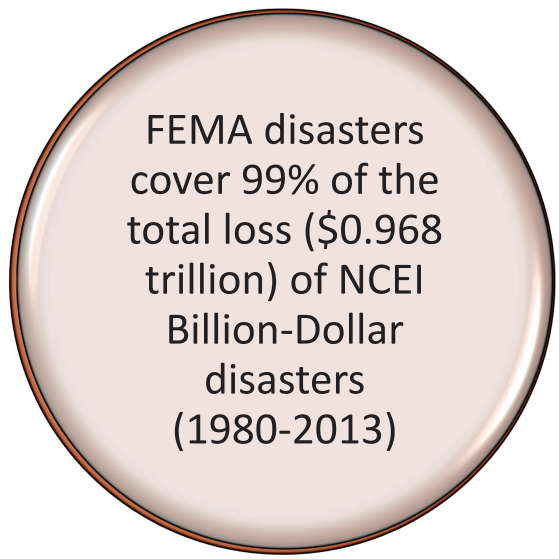


Table IA.6: Augmenting the FEMA Database Using the NCEI Database (continued)

Panel B. Identifying Severe / Super-Severe Disasters in NCEI Storm Events Not Covered by FEMA

Steps	Number of County Events
Storm Events database from 1996 to 2013	1,009,451
Less events with missing property damage	-453,852
Remaining events	555,599
Less marine events	-239
Remaining events	555,361
Less events covered by the FEMA database (manual matching)	-107,204
(We note that there could be multiple NCEI county events for one FEMA county-event)	
Remaining events	448,157
Method I. Severe / super-severe disasters measured using fatalities	
Remaining events	448,157
Less events with zero deaths	-445,621
Remaining events	2,536
Retain events with > 0 and < 10 deaths	2,519
(or) Retain events with >= 10 deaths	17
(Boustan et al. (2019) define a severe (super-severe) disaster as a disaster that caused 10 (100) or more deaths)	
Severe / super-severe county events not covered by FEMA	
Lower bound (using Boustan et al. (2019) cutoff)	$\boxed{17}$
Upper Bound (including events with > 0 deaths)	$\boxed{2,536}$
Method II. Severe / super-severe disasters measured using county-level property damage	
Remaining events	448,157
Less events with zero property damage	-256,196
Remaining events	191,961
Discard events with property damage < $ 1 mn (December 2014 dollars)	-187,869
(or) Discard events with property damage < 0.3% of county GDP†	-190,576
(The average of global disaster losses as a share of GDP from 1996 to 2013 is 0.3% (Pielke, 2019).)	
Severe / super-severe county events not covered by FEMA	
Lower bound (using property damages >= 0.3% GDP)	$\boxed{1,385}$
Upper bound (using property damages >= $1 mn)	$\boxed{4,092}$
Total unique severe / super-severe county events not covered by FEMA	
Lower bound	$\boxed{1,400}$
% of severe / super-severe county events in Storm Events not covered by FEMA	$\boxed{0.14\%}$
Upper bound	$\boxed{6,462}$
% of severe / super-severe county events in Storm Events not covered by FEMA	$\boxed{0.64\%}$

† 27,563 events that do not have county-level GDP data are also excluded.

Table IA.7: Risk-Taking Behavior and Disaster Experiences: Using Matched Sample

Panel A presents the average treatment effect of the treated using matched sample. We designate households (with the same age) that have cumulatively experienced above the median (or top quartile) number of disasters in any given year as the high-treatment-intensity group. We designate households that have cumulatively experienced below the median (or bottom quartile) number of disasters in the same year as the low-treatment-intensity group. We then predict the high-treatment-intensity group using a logistic regression (which includes all observable characteristics of our households as covariates) and generate propensity scores for the two samples. We use 1-nearest neighbor matching with a caliper of 0.1 to find a matched household in the low-treatment-intensity group for every household in the treated group. We then estimate the average treatment effect of the treated (ATT) for both the participation rate and the risky asset share. We estimate standard errors for the ATT using bootstrapping with 100 replications and report them in parentheses. Panel B plots the distribution of logit propensity scores of the two samples before and after the matching exercise (top vs. bottom quartile). The sample period runs from 1988 to 2012. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Risk-Taking Behavior: Using Matched Sample

Average Treatment Effect of the Treated			
PARTICIPATION		RISKY_SHARE	
Above vs. Below Median	Q1 vs. Q4	Above vs. Below Median	Q1 vs. Q4
1	2	3	4
-0.012***	-0.023***	-0.019***	-0.028***
(.002)	(.004)	(.002)	(.003)

Panel B. Distribution of Logit Propensity Scores for High- and Low-Treatment-Intensity Groups before and after Matching

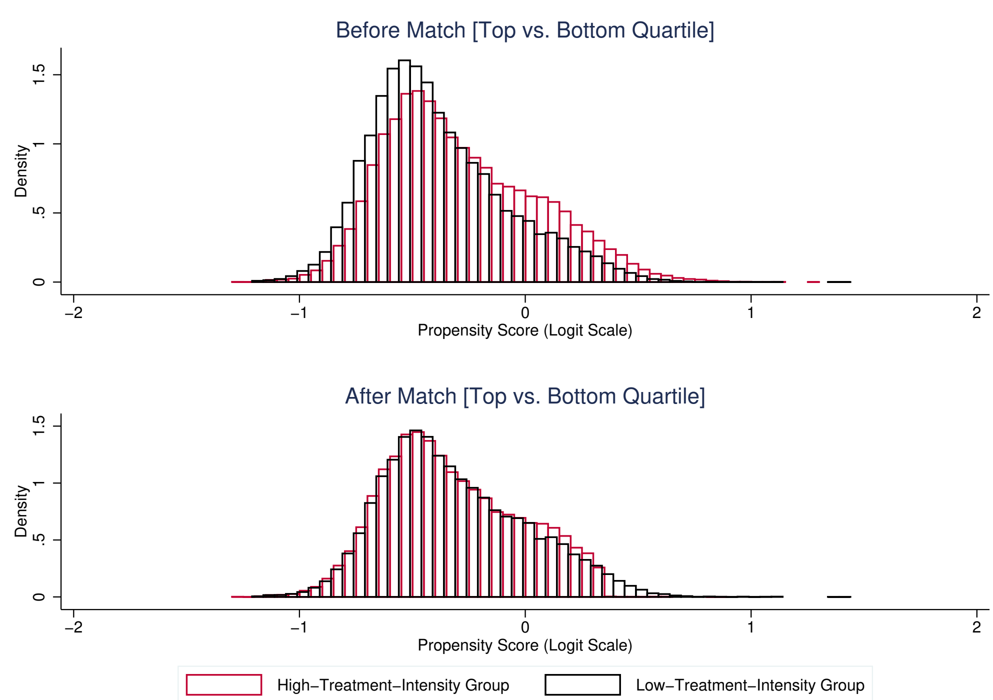


Table IA.7: Risk-Taking Behavior and Disaster Experiences: Using Matched Sample (continued)

Panel C presents the results of a Rosenbaum bounds sensitivity analysis (Rosenbaum, 2002) to examine whether unobserved factors can alter inference about treatment effects (hidden bias), the effect of disaster experiences on risky asset market participation. Columns 1 and 2 report the Mantel-Haenszel statistics under the assumptions of positive and negative (unobserved) selections while setting the level of hidden bias to a certain value $\exp(\gamma)$. Under the positive (negative) selection, households who have experienced above the median number of disasters in any given year-age also have a higher (lower) probability of participating in risky asset markets: the estimated treatment effects will be positively (negatively) biased relative to the true treatment effect. $\exp(\gamma)$ of 1 refers that unobservable factor(s) has no effect on how each household-year observation is assigned to the high-treatment-intensity group or to the low-treatment-intensity group. For values of $\exp(\gamma)$ higher than 1, for example 2, households who appear to be similar in terms of observables could differ in their odds of receiving the treatment by as much as a factor of 2. Columns 3 and 4 report the corresponding p-values.

Panel C. Rosenbaum Bounds: Sensitivity Analysis of Average Treatment Effect

	Mantel-Haenszel statistic		p-critical	
$\exp(\gamma)$	positive selection 1	negative selection 2	positive selection 3	negative selection 4
1	3.656	3.656	.000128	.000128
2	55.215	47.757	0	0
3	86.204	78.540	0	0
4	108.866	100.996	0	0
5	126.972	118.902	0	0
6	142.188	133.929	0	0
7	155.403	146.963	0	0
8	167.147	158.534	0	0
9	177.764	168.985	0	0
10	187.486	178.548	0	0

Table IA.8: Indirect Disaster Experiences using Adjacent Counties

This table examines the effects of direct and indirect disaster experiences of households on their risk-taking behavior. ln(1+CUMNUM_OF_DIRECT_DISASTERS) is the log transformation of a household's total number of disaster experiences up to the current survey year. ln(1+CUMNUM_OF_INDIRECT_DISASTERS) is the log transformation of a household's total number of indirect disaster experiences. The total number of indirect disaster experiences is defined as the number of disasters that occurred *only* in the adjacent counties of the current residence of households. Observations are weighted by the NLSY79 sample weights. Numbers in parentheses are standard errors that are clustered by county. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	PARTICIPATION	RISKY_SHARE
	1	2
ln(1+CUMNUM_OF_DIRECT_DISASTERS)	-.029***	-.020**
	(.010)	(.009)
ln(1+CUMNUM_OF_INDIRECT_DISASTERS)	-.0008	-.006
	(.008)	(.007)
Income & Liquid Assets Controls	Yes	Yes
Household Characteristics	Yes	Yes
Household F.E.	Yes	Yes
Age and County-by-Year F.E.	Yes	Yes
# Obs.	107,776	81,566
Adjusted R^2	.635	.648

Table IA.9: Additional Tests on Shocks to Local Economy

Panel C reports the results of additional robustness tests regarding shocks to local economy. Columns 1–4 repeat column 1 in Table III in the main text and columns 5–8 repeat column 4 in Table III with various forms of geographic location fixed effects (state, state-by-year, and county-by-year) and state-level macroeconomic variables: GDP growth rate from the Bureau of Economic Analysis, population and population density from the U.S. Census Bureau, and unemployment rate from the Bureau of Labor Statistics for the county the household resides in.

	PARTICIPATION				RISKY_SHARE			
	1	2	3	4	5	6	7	8
ln(1+CUMNUM_OF_DISASTERS)	-.032***	-.027**	-.031***	-.031***	-.024***	-.023**	-.023**	-.024***
	(.010)	(.013)	(.010)	(.010)	(.009)	(.011)	(.009)	(.009)
GDP_GROWTH			.0007	.0007			.001*	.001*
			(.0009)	(.0009)			(.0008)	(.0008)
ln(1+POPULATION)			-.002				-.005	
			(.006)				(.005)	
POPULATION_DENSITY				-1.759				3.297
				(4.114)				(4.225)
UNEMPLOYMENT			.0009	.0009			.0005	.0002
			(.002)	(.002)			(.002)	(.002)
Income & Liquid Assets	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age and Household F.E.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year F.E.	Yes	No	Yes	Yes	Yes	No	Yes	Yes
State F.E.	Yes	No	No	No	Yes	No	No	No
State-by-Year F.E.	No	Yes	No	No	No	Yes	No	No
# Obs.	107,776	107,776	107,776	107,776	81,566	81,566	81,566	81,566
Adjusted R^2	.458	.464	.457	.457	.558	.563	.557	.557

Table IA.10: The Effects of Socioeconomic Status and Its Changes Due to Disasters

In Panel A, column 1 repeats column 2 in Table III of the main text, and column 2 repeats column 5 in Table III of the main text by conditioning ln(1+CUMNUM_OF_DISASTERS) on a socioeconomic-status (SES) measure (based on income). We define HIGH_SES as households whose income is above median of the income distribution of year-age group and LOW_SES as households whose income is below median. Observations are weighted by the NLSY79 sample weights. The sample period runs from 1988 to 2012. Numbers in parentheses are standard errors that are clustered by county. We test the null hypothesis (H_0), the equality of the coefficients on ln(1+CUMNUM_OF_DISASTERS) in the regression across the LOW_SES and HIGH_SES, using a two-tailed test. Note that LOW_SES indicator is omitted as a baseline. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. The Effects of Socioeconomic Status

	PARTICIPATION	RISKY_SHARE
	1	2
(a) ln(1+CUMNUM_OF_DISASTERS) | (LOW_SES)	-.043***	-.032***
	(.010)	(.009)
(b) ln(1+CUMNUM_OF_DISASTERS) | (HIGH_SES)	-.020*	-.017*
	(.011)	(.009)
COLLEGE	-.003	-.011
	(.015)	(.013)
HIGH_SES	-.015	-.026**
	(.012)	(.013)
LOW_SES	(omitted)	(omitted)
Liquid Assets & Household Characteristics	Yes	Yes
Household F.E.	Yes	Yes
Age and County-by-Year F.E.	Yes	Yes
H_0: (a) - (b) = 0	-.023***	-.015**
# Obs.	107,776	81,566
Adjusted R^2	.635	.648

Table IA.10: The Effects of Socioeconomic Status and Its Changes Due to Disasters (continued)

In Panel B, column 1 repeats column 2 in Table III of the main text, and column 2 repeats column 5 in Table III of the main text by conditioning ln(1+CUMNUM_OF_DISASTERS) on both a socioeconomic-status (SES) measure (based on income) and its changes due to disaster experiences (measured by SES_SENSITIVITY). We define HIGH_SES as households whose income is above median of the income distribution of year-age group and LOW_SES as households whose income is below median. We estimate each household's SES_SENSITIVITY as follows: first, we run a pooled regression of the log of income on the same set of control variables with household, age and year fixed effects; second, for each household-year observation, we calculate a ratio of changes in residuals to changes in ln(1+CUMNUM_OF_DISASTERS) as long as the denominator is non-zero; third, for each household, we define the sensitivity as the time-series average of these ratios. We classify households as either NEGATIVE_SS or POSITIVE_SS based on the sign of their SES_SENSITIVITY. Observations are weighted by the NLSY79 sample weights. The sample period runs from 1988 to 2012. Numbers in parentheses are standard errors that are clustered by county. We test the null hypothesis (H_0), the equality of the coefficients on ln(1+CUMNUM_OF_DISASTERS) in the regression across the NEGATIVE_SS and POSITIVE_SS for a given level of SES, using a two-tailed test. Note that NEGATIVE_SS & LOW_SES indicator is omitted as a baseline and POSITIVE_SS & HIGH_SES dummy is additionally omitted due to household fixed effects. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel B. The Effects of Socioeconomic Status and its Changes due to Disaster Experiences

	PARTICIPATION	RISKY_SHARE
	1	2
(a) ln(1+CUMNUM_OF_DISASTERS) | (NEGATIVE_SS & LOW_SES)	-.052***	-.042***
	(.010)	(.012)
(b) ln(1+CUMNUM_OF_DISASTERS) | (POSITIVE_SS & LOW_SES)	-.034***	-.023**
	(.012)	(.009)
(c) ln(1+CUMNUM_OF_DISASTERS) | (NEGATIVE_SS & HIGH_SES)	-.022*	-.030***
	(.011)	(.010)
(d) ln(1+CUMNUM_OF_DISASTERS) | (POSITIVE_SS & HIGH_SES)	-.021*	-.009
	(.012)	(.010)
COLLEGE	-.003	-.011
	(.015)	(.013)
POSITIVE_SS & LOW_SES	-.004	.026*
	(.015)	(.015)
NEGATIVE_SS & HIGH_SES	-.031*	-.018
	(.018)	(.020)
POSITIVE_SS & HIGH_SES	(omitted)	(omitted)
Liquid Assets & Household Characteristics	Yes	Yes
Household F.E.	Yes	Yes
Age and County-by-Year F.E.	Yes	Yes
Summary Statistics of SES_SENSITIVITY	Median	0.164
	Mean	-0.104
	Std.Dev.	5.464
H_0: (a) - (b) = 0	-.018**	-.019*
H_0: (c) - (d) = 0	-.001	-.021***
# Obs.	102,890	78,366
Adjusted R^2	.636	.650

Table IA.11: Risky Taking Behavior and Disaster Experiences: Coefficients on Control Variables

This table reports Table III in the main text with coefficients on control variables. We use the FEMA Disaster Declarations Database to measure disasters. CUMNUM_OF_DISASTERS is a household's total number of disaster experiences up to current survey year, and ln(1+CUMNUM_OF_DISASTERS) is the log transformation of CUMNUM_OF_DISASTERS. Observations are weighted by the NLSY79 sample weights. The sample period runs from 1988 to 2012. Numbers in parentheses are standard errors that are clustered by county. ***, **, * indicate significance at the 1%, 5%, and 10% levels, respectively.

	PARTICIPATION			RISKY_SHARE	
	Whole Sample		Above-Median Wealth	Whole Sample	
	1	2	3	4	5
ln(1+CUMNUM_OF_DISASTERS)	-.032***	-.029***	-.034**	-.023***	-.021**
	(.009)	(.010)	(.015)	(.008)	(.009)
ln(1+INCOME)	.010***	.009***	.016***	.013***	.012***
	(.003)	(.003)	(.005)	(.003)	(.003)
$\left[\ln(1+\text{INCOME})\right]^2$	-.0005*	-.0004	-.0009**	-.001***	-.001***
	(.0003)	(.0003)	(.0004)	(.0003)	(.0003)
NUM_OF_CHILDREN	.004	.005	-.005	.011**	.011**
	(.004)	(.005)	(.007)	(.004)	(.004)
$(\text{NUM\_OF\_CHILDREN})^2$	-.0008	-.0008	.00006	-.001	-.001
	(.001)	(.001)	(.002)	(.001)	(.001)
HIGH SCHOOL	-.026*	-.022	.028	-.001	-.008
	(.015)	(.015)	(.030)	(.018)	(.019)
COLLEGE	.002	.0002	-.019	-.006	-.009
	(.015)	(.015)	(.020)	(.013)	(.013)
ln(1+LIQUID_ASSETS)	-.022***	-.022***	.002	-.010***	-.010***
	(.002)	(.002)	(.004)	(.003)	(.003)
$\left[\ln(1+\text{LIQUID\_ASSETS})\right]^2$	.007***	.007***	.005***	.005***	.005***
	(.0002)	(.0002)	(.0003)	(.0002)	(.0002)
MARRIED	-.006	-.008	-.014	-.013*	-.013*
	(.008)	(.008)	(.012)	(.007)	(.007)
Income & Liquid Assets	Yes	Yes	Yes	Yes	Yes
Household Characteristics	Yes	Yes	Yes	Yes	Yes
Household F.E.	Yes	Yes	Yes	Yes	Yes
Age and Year F.E.	Yes	No	No	Yes	No
Age and County-by-Year F.E.	No	Yes	Yes	No	Yes
# Obs.	107,776	107,776	54,774	81,566	81,566
Adjusted R^2	.629	.635	.595	.642	.648

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