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Do Natural Disaster Experiences Limit Stock Market Participation?

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Abstract

We examine whether natural disaster experiences affect households' portfolio choice decisions. Using data from the National Longitudinal Survey of Youth 1979, we find that adversely affected households are less likely to participate in risky asset markets. After a disaster shock, households become more risk-averse and lower their expectations on future stock market returns. Such conservative portfolio choices persist even after households relocate to less disaster-prone areas, consistent with risk preferences being altered by disaster experiences. Overall, our evidence suggests that transient but salient experiences can be an important factor in explaining the limited participation puzzle.

I. Introduction

A large body of literature in finance and economics asks why households do not participate in the risky asset markets to the extent that traditional financial theories suggest. The limited stock market participation puzzle is geographically prevalent and extends to the indirect ownership of equity (Guiso and Sodini (2013),

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Gomes, Haliassos, and Ramadorai (2021)).[1] While the presence of fixed participation costs (Vissing-Jorgensen (2003), Gomes and Michaelides (2005)) can partially explain the limited stock market participation puzzle, a significant part of variation in the propensity to participate in risky asset markets still remains unexplained. It is especially challenging to rationalize why a large fraction of wealthy people choose to keep themselves out of the risky asset markets even with affordable participation costs (Campbell (2006)). Various explanations such as reinforcement learning, loss aversion, mental accounting, and trust, among others, have been shown as additional factors that help explain individuals' asset allocation decisions and thus address this puzzle.[2] In a different vein, traumatic experiences such as living through the Great Depression and high inflation periods have been shown to significantly change economic agents' expectations and thus possibly their reluctance to hold stocks (Malmendier and Nagel (2011), (2016)). The psychology literature points out that personal experiences (e.g., crime, natural disasters, and epidemics) exert a great influence on an individual's preferences and behavior (Weinstein (1989), Fung and Carstensen (2006)). Motivated by this insight, in this article, we examine the relation between a households' exposure to natural disasters and their subsequent risky asset market participation. We find that in the United States, a household's natural disaster experience has significant explanatory power (rivaling or exceeding current explanatory factors) for their risky asset market participation. Importantly, we find that wealthy households who are also strongly influenced by disaster experiences stay out of the risky asset market, thus potentially offering a resolution to the puzzle raised by Campbell (2006).

We study the relation between a households' lifetime exposure to natural disasters and their subsequent financial decisions, that is, whether to participate in risky asset markets and how much of their liquid assets to invest in risky assets. We use detailed household micro-panel data from the National Longitudinal Survey of Youth 1979 (NLSY79) cohort and match those data with the geographic locations of the respondents using the confidential geocode data from the Bureau of Labor Statistics. We also assemble a set of severe county-level natural disaster events in the United States over the period from 1964 to 2013 from the Federal Emergency Management Agency (FEMA) Disaster Declarations database. Using the combined data, we measure a household's lifetime exposure to natural disasters as the total number of disaster experiences from the respondent's birth date to the current survey date.

We find that a household's exposure to natural disasters is transient, with a median duration of 5 days. Strikingly, we find unambiguous evidence that even these transient personal experiences have an economically significant, long-lasting impact on households' portfolio choice decisions. Specifically, individuals lower their financial risk by participating less in the risky asset market and investing a smaller fraction of their wealth in risky assets after experiencing a natural disaster shock. In estimating these effects, we exploit *within-household* variation in a

[1]The household finance literature documents that equity market participation rates are below 50% for most of the developed countries (Gomes et al. (2021)). Badarinza, Campbell, and Ramadorai (2016) point out that the participation rate in the United States in 2010 was closer to 20% once the assets in defined contribution accounts are excluded in estimating the rate.

[2]See Benartzi (2001), Gomes (2005), Barberis, Huang, and Thaler (2006), Guiso, Sapienza, and Zingales (2008), Kaustia and Knüpfer (2008), Choi, Laibson, Madrian, and Metrick (2009), Knüpfer, Rantapuska, and SarvimäkiI (2017), and Anagol, Balasubramaniam, and Ramadorai (2021).

household's lifetime disaster experiences. One-standard-deviation increase in disaster experiences from below the mean to the mean decreases a household's risky asset market participation rate by 3.5 percentage points, a 7.9% decrease relative to the sample mean participation rate. To obtain the same economic impact in our sample, a household would have to suffer a decrease in income (liquid assets) by 33% (23%) relative to the sample mean income (liquid assets). This comparison suggests that our effect is quantitatively very important in addressing the limited stock market participation puzzle. Correspondingly, the risky asset share decreases by 2.6 percentage points, a 6.8% decrease relative to the average share of risky assets in the sample. Crucially, such disaster effects on risky asset market participation remain strong for households with above-median wealth. This can partly explain why a large fraction of wealthy people do not participate in risky asset markets even though the fixed participation cost is relatively trivial for them, complementing the trust-based story by Guiso et al. (2008) as a resolution of the puzzle posed by Campbell (2006). These results are robust to using age and county-by-year fixed effects and controlling for state-level macroeconomic variables. These specifications effectively purge potential time trends, time-varying aggregate risk aversion, other time-specific determinants such as delayed portfolio rebalancing, life-cycle effects such as retirement considerations, and any county-level unobserved time-specific factors driving both disaster experiences and asset allocation decisions.

It is important to note that our findings based on the NLSY79 sample have strong external validity. First, any inferences from the NLSY79 representative sample are designed to apply to the U.S. population, and we use the sample weights in all our regression specifications to derive the point estimates as recommended by Solon, Haider, and Wooldridge (2015). Second, we confirm, in our data, that the majority of the U.S. population is exposed to natural disaster shocks: we calculate that a household's probability of experiencing at least one disaster over a 10-year period is 84.48%–93.19%, depending on the calculation method employed. In our sample, 98% of households are exposed to at least two severe natural disasters over their lifetime. Finally, all U.S. counties have at least one FEMA disaster declaration, and only 5.6% of counties have less than five declarations for the entire sample period. Therefore, our findings potentially have important implications for explaining the limited stock market participation puzzle. We also note that our findings are not driven by a select few households that experienced an extreme number of disasters or households that were exposed to a few catastrophic disasters (such as Hurricane Katrina) since we control for household fixed effects in our regressions. To examine the impact of different weighting schemes on such "super-severe" disasters in households' portfolio choice decisions, we conduct sensitivity analyses. The results from the sensitivity analyses indicate that both severe and super-severe disasters are economically important in determining households' portfolio choice decisions. Super-severe disaster experiences are economically more important than severe ones in a household's risky asset market participation decision, but both types of disasters seem to equally matter in determining the fraction of risky assets in the household's portfolio. To examine whether and how possible political considerations in the FEMA declaration affect our inferences, we exploit the National Centers for Environmental Information (NCEI) disaster database and newspaper stories from LexisNexis. Using these additional sources, we confirm that the

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political biases if any (which might either include or exclude certain disasters) are unlikely to alter our conclusions.

We then investigate potential mechanisms by which disaster experiences affect households' risk-taking behavior. We find that income and wealth shocks suffered by households reduce the economic impact of disasters on risky asset market participation by about 25%; for risky asset share, they reduce the impact by about 11%. These results indicate that a relatively small part of the disaster experience-induced variation in households' risk-taking behavior can be explained by income or wealth shocks. Additional tests indicate that the disaster experience effect can neither be explained by homeownership nor subsumed by changes in health and socioeconomic status of households. We conjecture that disaster experiences matter for their own sake and affect portfolio choice by changing households' risk aversion and expectations.

Finally, we provide evidence consistent with this view that even transient but salient disaster experiences make households more conservative in their portfolio choice decisions by increasing their risk aversion *and* decreasing their expectations for favorable future stock market returns. We find that households that move from a high-disaster-prone county to a low-disaster-prone one continue to be conservative in their portfolio choices (even more than 6 years after the move). This result suggests that shocks to risk aversion due to disasters may influence future portfolio choices. Constructing a risk-aversion measure from the sequence of survey questions in the NLSY79, we indeed find that 1-standard-deviation increase in the cumulative number of disasters increases the likelihood of being more risk averse by 20.5 percentage points. Similarly, using the UBS/Gallup survey data, we provide evidence that disaster experiences decrease households' expectations about the stock market return over the next year by 50 basis points. We also decompose the relative importance of the risk preferences and expectations channels on portfolio choices when an individual is faced with natural disasters. Adopting the simple portfolio choice model of Merton (1969) and Samuelson (1969), we show that 25% of the changes in portfolios can be attributed to revised expectations, and the balance of 75% of the changes can be attributed to changes in risk aversion following disaster experiences.

The main contribution of this study is threefold. First, our analysis contributes to the literature on the limited stock market participation of households (e.g., Mankiw and Zeldes (1991), Haliassos and Bertaut (1995), and Vissing-Jorgensen (2002)) by identifying a new significant determinant (i.e., adverse personal experiences due to natural disasters) on household portfolio choices. It has predictive strength that is comparable to that of commonly analyzed variables (e.g., income and liquid assets). The empirical fact that the majority of the U.S. population is exposed to natural disaster shocks, coupled with the population estimates of our effects based on the NLSY79 sample weights, indicates that our findings have important implications for the limited participation puzzle. Importantly, our findings do not fade away with the higher wealth of households, which can partly explain why a significant fraction of wealthy people does not participate in risky asset markets. This finding is obtained without imposing an unrealistic level of participation costs in economic models (Vissing-Jorgensen (2003)), which is a significant challenge to the existing literature, as pointed by Campbell (2006).

Second, it adds to a growing body of literature that analyzes the relation between personal experiences and financial decisions. The literature has shown that salient, long-lived experiences affect individuals' financial decision-making.[3] Complementing these findings, we show that disaster experiences, even if they are transient in nature and even if they occur in adulthood, can have long-lasting effects on household portfolio choice decisions. Our final contribution is to identify mechanisms that lead to a conservative asset allocation decision after a disaster shock. Disaster experiences alter *both* the risk aversion of individuals and their expectations about future stock market returns.[4] Our article thus complements the literature that relies on experimental data (Callen et al. (2014), Cameron and Shah (2015)) by providing consistent results using real-life asset allocation data and adding to the picture of how experiences shape individual risk-taking behavior.

II. Data Sources and Main Variables

A. Household Survey Data

The key dependent variables for our analysis are risky asset market participation and risky asset share of the total portfolio for each household. These household microdata are sourced from the NLSY79. The NLSY79 survey is specifically designed to interview and track a nationally representative sample of 12,686 young men and women who were born between 1957 and 1964 and were between 14 and 22 years of age when they were first interviewed in 1979. The survey tracks these individuals and provides unbroken information about them, from when they were minors (within their parents' households) to when they later formed their own households as adults. The respondents became adults, and between 23 and 31 years of age, they were considered the heads of their own households when they were first asked about their financial assets in 1988. We use the NLSY79 sample data for these adults from 1988 to 2012, which include information about financial assets. The respondents were interviewed annually through 1994 and every 2 years after 1994.

In defining RISKY_ASSETS and SAFE_ASSETS, we follow Angerer and Lam (2009): RISKY_ASSETS consist of common stocks, preferred stocks, government and corporate bonds, and mutual funds; SAFE_ASSETS include checking and savings accounts, money market funds, certificates of deposit, U.S. saving bonds, and personal loans. As noted by Angerer and Lam (2009), individual retirement accounts (IRAs) and tax-deferred accounts (401(k), 403(b), and others) are included in RISKY_ASSETS from 1994 onward (before 1994 the survey reported these with other safe assets as a sum; hence we include them in SAFE_ASSETS). The sum of RISKY_ASSETS and SAFE_ASSETS is defined as LIQUID_ASSETS.

[3]For instance, macroeconomic experiences, such as the Great Depression (Malmendier and Nagel (2011)) and realized inflation over a lifetime (Malmendier and Nagel (2016)), shape households' financial decisions such as risky asset market participation and borrowing or lending behavior.

[4]The latter effect is consistent with findings by Cameron and Shah (2015) and Malmendier and Nagel (2011), who attribute their results to changes in beliefs. Specifically, Cameron and Shah (2015) use experiments in rural Indonesia to demonstrate that risk aversion increases after a flood or an earthquake. The former effect is supported by recent experimental evidence that frightening recollections of individuals who were exposed to violence in Afghanistan trigger changes in their risk and certainty preferences (Callen, Isaqzadeh, Long, and Sprenger (2014)).

RISKY_SHARE is defined as the ratio of RISKY_ASSETS to LIQUID_ASSETS. These variables are subject to a missing data issue from time to time. As discussed by the Bureau of Labor Statistics, some data are missing from the NLSY79 for several reasons. First, some respondents did 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 did not provide valid answers to some questions. The invalid answers are flagged as either −1 (refusal) or − 2 (do not know).[5] 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). We impute these missing values in our main variables to enhance the power of our tests. The details of the imputation methods and the robustness of the main results to the use of imputation are provided in Supplementary Material Section A.

In each survey year, the NLSY79 constructs a set of sampling weights. These weights indicate how much each respondent counts in a statistical procedure; that is, how many individuals in the United States each respondent represents. Thus inferences using the NLSY79 sample are applicable to the entire population of the U.S., provided that the researcher uses the sample weights to derive population moments. We provide simple examples to illustrate the intuition of sampling weights and discuss the sensitivity of our main results to these sampling weights in Supplementary Material Section B.

We also obtain the confidential geocode data of the NLSY79 respondents from the Bureau of Labor Statistics, which enables us to match households in NLSY79 and their geographic locations with natural disaster data. The NLSY79 confidential geocode data (which we merge with the NLSY79 public data) provide the specific county and state of residence of the respondents in the survey. Residence is coded using the Federal Information Processing Standards (FIPS) codes, a 2-digit code for the state and a 3-digit code for the county of residence. This information is available at the following points in time for each respondent: at birth, at age 14 years, and at the time of the survey interviews (beginning in 1979). In addition, the geocode data are updated with the time and residence information for up to nine residences (or moves) since the last interview date, starting from 1979. This information is crucial in our construction of the natural disaster experiences variables for each survey respondent. Thus we track virtually the entire location history of all respondents in our data set starting from age 14 of the respondent. For location information of individuals from ages 5 to 14 years, we use the respondents' county of birth as a proxy for their location of residence between the ages of 5 and 14. Using the combined data, we construct a variable, CUMNUM_OF_DISASTERS, which is a household's total number of disaster experiences from the household respondent's birth date up to the current survey date. This variable is measured for each household for each survey year in the sample.

Finally, to examine whether disaster experiences affect households' expectations about future stock market returns and volatility (one of the potential channels

[5]The Bureau of Labor Statistics notes that the assignment of "refusals" and "do not knows" is likely to vary across interviewers and hence is somewhat arbitrary.

investigated in our article), we use UBS/Gallup surveys from the Roper Center at the University of Connecticut. The UBS/Gallup survey data are purely cross-sectional, and the composition of survey respondents changes over time. 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 wealthier households relative to those of NLSY79. We provide summary statistics of UBS/Gallup survey data and discuss a couple of observations on the differences in the characteristics of NLSY79 and UBS/Gallup surveys in Supplementary Material Section C. Even though the UBS/Gallup survey data differ on certain dimensions from the NLSY79 data, they also provide survey weights (similar to the NLSY79). Therefore, we use the respective survey weights provided by both data sets while estimating regression parameters in the article. The UBS/Gallup data provide each household's expected rate of return on the stock market over the next 12 months and its expectations for whether the volatility in the marketplace over the same period will increase or decrease. These data are available for a limited period for the years 2000–2002 for the expected rate of return and for the years 1998–2000 for the expectations on volatility.

B. Natural Disaster Data

1. FEMA Disaster Declarations Database

The set of natural disaster events in the United States that we use is obtained from the Federal Emergency Management Agency (FEMA) Disaster Declarations database dating back to 1964. To be included in our sample, FEMA must have declared an event to be a natural disaster, as determined by the Robert T. Stafford Disaster Relief and Emergency Assistance Act, 42 U.S.C. §§ 5121–5207 (the Stafford Act) § 401, which states in part: "*All requests for a declaration by the President, that a major disaster exists shall be made by the Governor of the affected State.*" After a disaster hits, the governor may submit a declaration request to the president through the state's FEMA regional office to show that the disaster is of such severity and magnitude that an effective response is beyond the capabilities of the state and that supplemental federal assistance is necessary. When evaluating a state's request and making recommendations to the president, FEMA considers the following factors: estimated cost of the assistance, localized impacts of the disaster, insurance coverage, whether there have been other recent disasters, other federal agency assistance programs, concentration of damage, trauma, special populations, damaged residences, and others. We note that all emergency and major disaster declarations are made at the discretion of the president of the United States.[6] The political nature of the declaration process may introduce selection issues for a disaster to be included in our database, thereby affecting our key independent variable, CUMNUM_OF_DISASTERS. We discuss such political nature and econometric biases due to measurement errors in Supplementary Material

[6]The governor can appeal the denial of a major disaster or emergency declaration request. The appeal must be submitted within 30 days of the date of the denial letter and should include additional information justifying the need for supplemental federal assistance. FEMA has codified the declaration process at 44 C.F.R. Part & 206, Subpart B (https://www.fema.gov/disaster-declaration-process).

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Section D. We also conduct a thorough investigation on the impact of this political consideration on our estimates in Section III.D. The FEMA database documents a variety of details for each declaration, including the declaration date, incident beginning and end dates, disaster type, incident type, and location (at the county level).

2. EM-DAT Disaster Database

As discussed in Supplementary Material Section D, the political science literature documents some evidence (although it is mixed) that the FEMA disaster declarations may be motivated by political influence. For example, Garrett and Sobel (2003) document that those states having a higher electoral importance (i.e., battleground states) have a statistically higher rate of disaster declaration. However, they find the governor's political alignment with the president to be a statistically insignificant factor. Therefore, such political alignment is unlikely to affect our estimates of the effect of disaster experiences on the risk-taking behavior of households. Nevertheless, to alleviate potential concerns about the political nature of FEMA disaster declaration process, we also use the Emergency Events Database (EM-DAT), 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.

The EM-DAT data include all disasters from 1900 until the present as long as each event conforms to at least one of the following criteria: i) 10 or more people have died; ii) 100 or more people have been affected, injured, or made homeless; iii) a state of emergency has been declared; or iv) a call for international assistance has been issued. Although the EM-DAT data still suffer from the same subjectivity problem as the FEMA data because of the criteria (iii) and (iv), they are less prone to the selection bias due to political considerations. The database contains start and end dates, affected areas (at the state level), disaster type, total deaths, total damage, total affected, uninsured losses, and disaster name (if any), among others. The total damage is the amount of damage to property, crops, and livestock; the total affected is the sum of the injured, affected, and left homeless after a disaster; the total uninsured losses is the difference between total damage and insured losses in which insured losses are those covered by the insurance sector and paid directly to the owner of the damaged or destroyed property or crops and livestock.

The main disadvantage of the EM-DAT data is that very limited information is available on the disaster-affected areas. Further, even this information is available only at the state level and thus is coarse in nature. For example, Hurricane Katrina caused enormous total damages. However, households that were not in the direct path of the hurricane are unlikely to have suffered heavily. It is crucial in our empirical design to use data that are further granulated to accurately identify the disaster experiences of each household. The FEMA data provide such granular county-level information. Therefore, we opt to use the FEMA data as the main source for empirical tests and supplement it with additional tests using the EM-DAT database. This procedure ensures that our main results are robust to any biases introduced by the political nature of the FEMA disaster declaration process.

C. Summary Statistics

From 1964 to 2013, 3,061 separate disasters were declared by the FEMA across the 50 states. Graph A1 of Figure 1 shows a heatmap of the total number of disaster declarations from 1964 to 2013 at the county level using quartiles as cutoffs. Darker colors indicate that a greater number of disasters were declared in that county. In Graph A2 of Figure 1, we visualize counties with 0 (in red), less than 5 (in green), and at or above 5 (in blue) disasters where the 5th percentile of distribution is four disasters. All counties have at least one disaster declaration, and only 5.6% of counties have less than five declarations for the sample period.

Table 1 presents summary statistics for disaster characteristics. Panel A shows the summary statistics of FEMA disasters by incident type. The median duration of all disasters is only 5 days, which indicates that most households' disaster experiences are transient. We also note that the distribution of disaster duration is highly right-skewed: the mean duration (17 days) is much higher than the median. This can be seen in the upper part of Graph B in Figure 1: about 30% of disasters lasted for 1 day. Severe storms, fires, and floods are the three most frequent types of incidents declared by FEMA; these account for almost three-quarters of the total number of disaster declarations in the sample period. Panel B of Table 1 presents the 10 most disaster-prone states (and counties) according to the total number of disaster declarations. Texas is the most disaster-prone state, followed by California and Oklahoma.

In Panel C of Table 1, we compute the likelihood that a typical household in the United States experiences a disaster in a typical year and over a 10-year period. We calculate the fraction of households that experienced disasters in a given year from our household sample and then take the time-series average to obtain the annual probability. Moreover, we calculate 10-year probabilities in the following two ways: first, assuming the annual probability is independent over time, we calculate $1-(1-\text{annual probability})^{10}$; second, allowing for the dependency, we calculate the fraction of households that experienced disasters over the past 10 years and then take time-series average. In estimating these probabilities, we use the NLSY79 sample weights to infer population probabilities. The estimated population annual probability of experiencing at least one disaster is 23.56%. This number rises to 93.19% (under independence assumption) and 84.48% (allowing for dependency) over a 10-year period. These probabilities indicate that the majority of the U.S. population is exposed to natural disaster shocks.

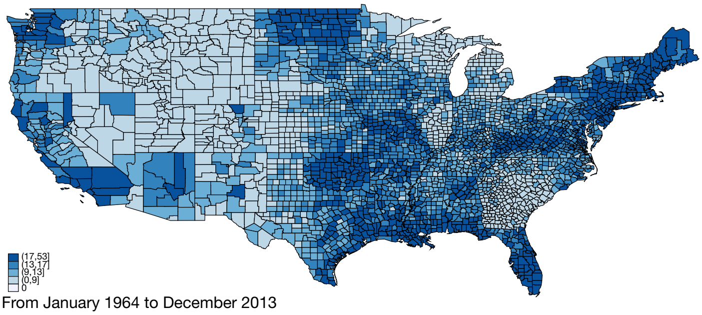
Panel A1 of Table 2 presents summary statistics of the variables used in our analysis for all households. Most of the variables in this table exhibit large variations, notably LIQUID_ASSETS and FINANCIAL_ASSETS. It is important to note that CUMNUM_OF_DISASTERS is highly right-skewed: its mean is approximately 7.63 disasters, which is greater than the median of 6 disasters. Therefore, a few households having a large number of disaster experiences may have a disproportionate influence on the parameter estimates in our regressions. To alleviate a concern that outliers in disaster experience variable may drive our results, we employ a log transformation of CUMNUM_OF_DISASTERS, ln(1 + CUMNUM_OF_DISASTERS), in our all regressions. The lower part of Graph B in Figure 1 shows that the transformed variable is a lot more symmetrical around the mean. This can also be seen by comparing the mean and median of

FIGURE 1

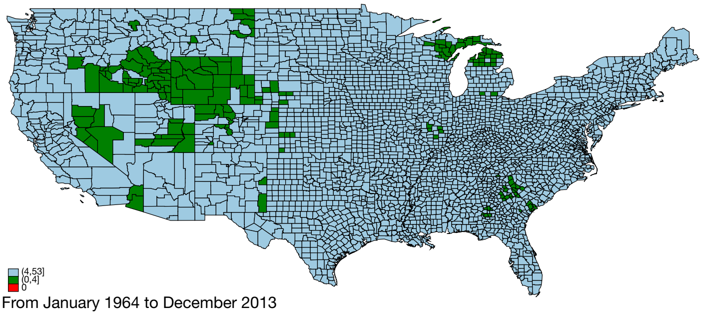
Distributional Characteristics of Natural Disasters in the USA (1964–2013)

Graph A1 of Figure 1 shows the total number of Federal Emergency Management Agency (FEMA) disaster declarations that occurred from 1964 to 2013 at the county level. The cutoffs (9, 13, and 17) are based on the 25th, 50th, and 75th percentiles, respectively. Graph A2 visually identify counties with zero (red), less than five (green), and at or above five (blue) disasters, where four disasters are the 5th percentile of the distribution. All counties have at least one disaster declaration and only 5.6% of counties have less than five declarations for the sample period. Graph B1 shows disaster duration, with the extreme 174 observations (with duration of more than 50 days) excluded from this histogram for clarity. The median disaster duration is 5 days. The disaster with the longest duration was the lava flow at the Kīlauea Volcano in Hawaii from Jan. 24, 1983 to Jan. 27, 1997. Graph B2 provides the log transformation of cumulative number of disaster experiences of households who are the respondents of the National Longitudinal Survey of Youth 1979 (NLSY79). We report the number of disasters (in red) that corresponds to the sample mean (8 disasters), 1-standard-deviation (6 disasters) below and above the mean, on each of the corresponding bars. Observations are weighted by the NLSY79 sample weights.

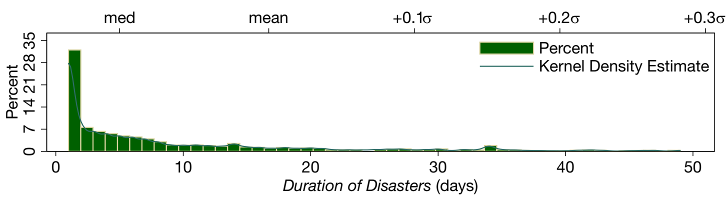
Graph A1. Total Number of Disasters by State-County: Quartiles as Cutoffs



Graph A2. Total Number of Disasters by State-County: 5th Percentile as Cutoff



Graph B1. Duration of Disasters



Graph B2. Cumulative Number of Disaster Experiences

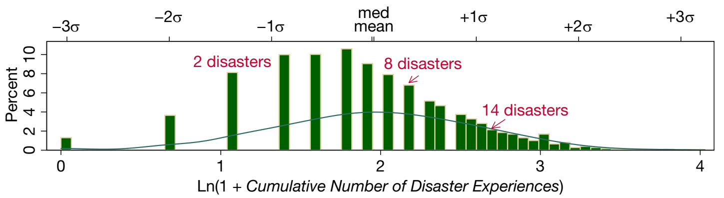


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TABLE 1

Disaster Characteristics

Panel A of Table 1 presents characteristics of FEMA disasters by incident type. We use the FEMA Disaster Declarations database for the period from 1964 to 2013. The total number of FEMA disasters is 3,061. Duration of disasters is calculated as the amount of time between the start and end dates of disasters. Panel B shows the top 10 most disaster-prone areas based on the total number of disaster declarations. Panel C presents annual and 10-year population probabilities that households will experience at least one disaster. We calculate the fraction of households that experienced disasters in a given year from our household sample and take time-series average to obtain the annual probability. We use the NLSY79 sample weights to infer population probability. We calculate 10-year probabilities in the following two ways: first, assuming the population annual probability is independent over time, we calculate $1-(1-\text{annual probability})^{10}$ in column 2; second, allowing for the dependency, we calculate the fraction of households that experienced disasters over the past 10 years and take time-series average in column 3. †Others include coastal storms, typhoons, fishing losses, and so forth.

Panel A. Summary Statistics of FEMA Disasters by Incident Type

Incident Type	*N*	Density (%)	Duration (Days)		
			Med.	Mean	Std. Dev.
Severe storm(s)	824	26.9	8.0	16.8	22.6
Fire	809	26.4	7.0	14.9	25.1
Flood	639	20.9	1.0	18.9	134.0
Hurricane	279	9.1	10.0	17.1	46.1
Snow	175	5.7	3.0	7.2	15.9
Tornado	142	4.6	1.0	3.6	7.4
Drought	41	1.3	1.0	2.9	12.0
Severe ice storm	41	1.3	7.0	10.3	9.2
Earthquake	26	0.8	7.0	39.5	68.1
Others†	85	2.8	3.0	84.2	555.0
All	3,061	100	5.0	17.6	117.3

Panel B. Top 10 Most Disaster-Prone States/State-Counties

	Total Number of Disasters			
Rank	State	Number	County, State	Number
1	Texas	236	Los Angeles, California	53
2	California	196	San Bernardino, California	45
3	Oklahoma	148	Riverside, California	44
4	Florida	114	Oklahoma, Oklahoma	39
5	New York	86	San Diego, California	36
6	Washington	84	McClain, Oklahoma	35
7	Alabama	76	Essex, Massachusetts	34
8	New Mexico	74	Collier, Florida	34
9	Colorado	68	Ventura, California	34
10	Louisiana	66	Logan, Oklahoma	33

Panel C. Population Probabilities of Experiencing at Least One Disaster

Population Annual Probability	Population 10-Year Probability (Independence Over Time)	Population 10-Year Probability (10-Year Window)
1	2	3
23.56%	93.19%	84.48%

ln(1 + CUMNUM_OF_DISASTERS) in Panel A1 of Table 2: The mean is about 1.96, which is almost the same as the median of 1.95. The panel also shows that, on average, a household experiences 12.92 disasters as of the end of sample period.[7] The average risky asset market participation rate for households is 44%, whereas the average safe asset market participation rate is over 75%: some households hold both types of assets, and some hold neither type. The average share of risky assets in a household's portfolio is 38%, and, consequently, the safe assets share is 62%.

[7]This household experience number (12.92) is larger than the mean of CUMNUM_OF_DISASTERS (7.63) because the latter is measured every survey year (and increases continuously over time as more disasters are added to the database), while the former is measured at the end of the sample period for each household.

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TABLE 2
Summary Statistics

Panel A1 of Table 2 provides summary statistics for all households. Panel A2 provides summary statistics by risky asset market participants and nonparticipants. Panel B provides summary statistics by households that reside in high-disaster-prone (HD) and low-disaster-prone (LD) counties, categorized as an HD (LD) county if the total number of disasters that occurred in that county is above (below) the median value of the distribution (in the sample of disasters from 1964 to 2013). Panel C provides summary statistics by households that experienced an above-median and below-median average number of disasters. We first calculate time-series average number of disasters for each household. Then we classify each household into either the above-median or below-median group based on the cross-sectional distribution of these time-series averages. CUMNUM_OF_DISASTERS is the total number of disaster experiences of households for every survey year. CUMNUM_OF_DISASTERS [household-level] indicates the total number of disasters experienced by each household using the last available survey year the household appeared in our sample. INCOME is calculated as the sum of military income, wages, salaries, tips, farm and business income, unemployment compensation, Aid to Families with Dependent Children payments, food stamps, Supplemental Security Income, and other welfare payments. SAFE_ASSETS consist of checking and savings accounts, money market funds, certificates of deposit, U.S. saving bonds, and personal loans. Individual retirement accounts and tax-deferred accounts (401(k), 403(b), and others) are included in safe assets before 1994 and in risky assets starting from 1994. RISKY_ASSETS include common stocks, preferred stocks, government, and corporate bonds, and mutual funds. LIQUID_ASSETS are the sum of RISKY_ASSETS and SAFE_ASSETS. NON-LIQUID_FINANCIAL_ASSETS are residential properties, farms and proprietary businesses, investment trusts, vehicles, and other assets. The sum of LIQUID_ASSETS and NON-LIQUID_FINANCIAL_ASSETS is FINANCIAL_ASSETS. PARTICIPATION (SAFE_PARTICIPATION) is an indicator that equals 1 if the household participates in the risky (safe) asset markets. RISKY_SHARE is the fraction of liquid assets invested in risky assets. HIGH_SCHOOL (COLLEGE) is an indicator that equals 1 if the respondent completed high school (college) education. HISPANIC (BLACK) indicates whether the respondent is Hispanic (Black). MARRIED equals 1 if the respondent is married, FEMALE is set to 1 if the respondent is female. All dollar-valued variables are deflated by the CPI-U inflation rates into Dec. 2014 dollars. ***, **, and * indicate significance of mean difference tests between the participants and nonparticipants (Panel A2), the HD and LD households (Panel B), and the two types of households (Panel C) at the 1%, 5%, and 10% levels, respectively. The standard errors in mean difference tests are clustered by household. Observations are weighted by the NLSY79 sample weights. The sample period is 1988–2012.

Panel A1. All Households

	Variables					
	All Households					
	P25	Median	Mean	P75	Std. Dev.	No. Obs.
CUMNUM_OF_DISASTERS	4	6	7.63	10	5.78	107,776
ln(1 + CUMNUM_OF_DISASTERS)	1.61	1.95	1.96	2.40	0.64	107,776
CUMNUM_OF_DISASTERS [household-level]	7	12	12.92	17	8.33	10,515
INCOME ($)	37,132	66,079	82,958	101,786	202,440	107,776
SAFE_ASSETS ($)	302	4,041	28,520	18,219	111,969	107,776
RISKY_ASSETS ($)	0	0	80,359	33,264	528,524	107,776
LIQUID_ASSETS ($)	685	9,819	108,879	63,480	564,742	107,776
NON-LIQUID_FINANCIAL_ASSETS ($)	3,811	28,726	102,186	94,887	423,443	66,950
FINANCIAL_ASSETS ($)	6,925	43,309	146,784	134,046	513,541	66,692
PARTICIPATION	0	0	0.44	1	0.50	107,776
SAFE_PARTICIPATION	1	1	0.77	1	0.42	107,776
RISKY_SHARE	0	0	0.38	0	0.40	81,566
NUM_OF_CHILDREN	0	1	1.20	2	1.23	107,776
HIGH_SCHOOL	1	1	0.91	1	0.28	107,776
COLLEGE	0	0	0.27	1	0.45	107,776
HISPANIC	0	0	0.06	0	0.25	107,776
BLACK	0	0	0.14	0	0.34	107,776
MARRIED	0	1	0.74	1	0.44	107,776
FEMALE	0	0	0.48	1	0.50	107,776
TIME_SPAN (=Last − First Survey Year) [household-level]	6	20	15.72	24	9.10	10,515
CUMNUM_OF_SURVEYS_PARTICIPATED_IN [household-level]	6	11	10.23	15	5.16	10,515
AGE [household-level in 1988]	25	27	27.16	29	2.31	10,515

Panel A2. Risky Asset Market Participants Versus Nonparticipants

	Variables							
	Risky Asset Market Participants				Risky Asset Market Nonparticipants			
	Median	Mean	Std. Dev.	No. Obs.	Median	Mean	Std. Dev.	No. Obs.
CUMNUM_OF_DISASTERS	8	9.44***	6.28	39,603	5	6.21	4.92	68,173
INCOME ($)	91,167	113,646***	193,674	39,603	48,918	58,937	205,879	68,173
SAFE_ASSETS ($)	13,295	53,452***	161,173	39,603	1,000	9,005	33,910	68,173
RISKY_ASSETS ($)	43,899	180,582***	786,026	39,603	0	0.00	0.00	68,173
LIQUID_ASSETS ($)	70,924	234,034***	834,374	39,603	1,000	9,005	33,910	68,173
NONLIQUID_FINANCIAL_ASSETS ($)	75,276	174,723***	586,700	18,924	13,851	63,118	293,480	48,026
FINANCIAL_ASSETS ($)	129,081	288,368***	757,423	18,869	18,283	70,321	282,253	47,823
PARTICIPATION	1	1.00***	0.00	39,603	0	0.00	0.00	68,173
SAFE_PARTICIPATION	1	0.93***	0.26	39,603	1	0.64	0.48	68,173
RISKY_SHARE	1	0.67***	0.30	39,603	0	0.00	0.00	41,963
NUM_OF_CHILDREN	1	1.30***	1.22	39,603	1	1.12	1.23	68,173
HIGH_SCHOOL	1	0.97***	0.16	39,603	1	0.87	0.34	68,173
COLLEGE	0	0.40***	0.49	39,603	0	0.18	0.38	68,173
HISPANIC	0	0.05***	0.21	39,603	0	0.08	0.27	68,173
BLACK	0	0.08***	0.27	39,603	0	0.18	0.39	68,173
MARRIED	1	0.85***	0.35	39,603	1	0.66	0.48	68,173
FEMALE	0	0.49	0.50	39,603	0	0.48	0.50	68,173
AGE	40	39.81***	7.28	39,603	31	33.19	6.70	68,173

(continued on next page)

TABLE 2 (continued)

Summary Statistics

Panel B. Households Who Reside in High- Versus Low-Disaster-Prone Counties

	Variables							
	Households in High Disaster-Prone Area				Households in Low Disaster-Prone Area			
	Median	Mean	Std. Dev.	No. Obs.	Median	Mean	Std. Dev.	No. Obs.
CUMNUM_OF_DISASTERS	7	8.71***	6.27	73,750	5	5.31	3.60	34,026
INCOME ($)	68,735	86,603***	215,333	73,750	61,572	75,177	171,452	34,026
SAFE_ASSETS ($)	4,498	30,224***	115,995	73,750	3,266	24,883	102,753	34,026
RISKY_ASSETS ($)	0	87,392***	608,519	73,750	0	65,344	291,275	34,026
LIQUID_ASSETS ($)	10,480	117,617***	642,855	73,750	8,415	90,228	342,140	34,026
NONLIQUID_FINANCIAL_ ASSETS ($)	27,180	105,706	461,856	46,286	32,392	94,413	322,684	20,664
FINANCIAL_ASSETS ($)	42,897	153,263**	548,251	46,103	43,850	132,484	426,733	20,589
PARTICIPATION	0	0.44	0.50	73,750	0	0.44	0.50	34,026
SAFE_PARTICIPATION	1	0.77	0.42	73,750	1	0.76	0.43	34,026
RISKY_SHARE	0	0.38**	0.40	55,728	0	0.39	0.40	25,838
NUM_OF_CHILDREN	1	1.17***	1.22	73,750	1	1.27	1.24	34,026
HIGH_SCHOOL	1	0.92**	0.27	73,750	1	0.91	0.29	34,026
COLLEGE	0	0.29***	0.45	73,750	0	0.24	0.42	34,026
HISPANIC	0	0.07***	0.26	73,750	0	0.05	0.21	34,026
BLACK	0	0.14**	0.35	73,750	0	0.13	0.33	34,026
MARRIED	1	0.73***	0.44	73,750	1	0.77	0.42	34,026
FEMALE	0	0.49	0.50	73,750	0	0.47	0.50	34,026
AGE	34	35.96***	7.68	73,750	35	36.38	7.74	34,026

Panel C. Households Who Experienced Above-Median and Below-Median Number of Disasters

	Variables							
	Households With Above-Median Disaster Exp.				Households With Below-Median Disaster Exp.			
	Median	Mean	Std. Dev.	No. Obs.	Median	Mean	Std. Dev.	No. Obs.
CUMNUM_OF_DISASTERS	9	10.41***	6.34	53,657	4	4.71	3.12	54,119
INCOME ($)	71,687	88,624***	170,064	53,657	61,093	77,027	231,384	54,119
SAFE_ASSETS ($)	5354	32,370***	118,983	53,657	3,029	24,490	103,970	54,119
RISKY_ASSETS ($)	570	93,300***	607,876	53,657	0	66,812	429,635	54,119
LIQUID_ASSETS ($)	13,938	125,670***	642,701	53,657	6,718	91,302	468,825	54,119
NONLIQUID_FINANCIAL_ ASSETS ($)	31,583	112,464**	477,994	30,945	26,826	92,732	365,914	36,005
FINANCIAL_ASSETS ($)	49,747	164,624***	578,375	30,818	37,914	130,377	445,041	35,874
PARTICIPATION	0	0.48***	0.50	53,657	0	0.40	0.49	54,119
SAFE_PARTICIPATION	1	0.78***	0.41	53,657	1	0.75	0.43	54,119
RISKY_SHARE	0	0.41***	0.40	41,513	0	0.35	0.40	40,053
NUM_OF_CHILDREN	1	1.18*	1.23	53,657	1	1.22	1.23	54,119
HIGH_SCHOOL	1	0.92***	0.26	53,657	1	0.90	0.29	54,119
COLLEGE	0	0.30***	0.46	53,657	0	0.25	0.43	54,119
HISPANIC	0	0.07***	0.26	53,657	0	0.06	0.23	54,119
BLACK	0	0.13	0.34	53,657	0	0.14	0.35	54,119
MARRIED	1	0.74	0.44	53,657	1	0.75	0.43	54,119
FEMALE	0	0.48	0.50	53,657	0	0.48	0.50	54,119
AGE	35	36.98***	7.88	53,657	33	35.16	7.39	54,119

To see whether certain types of households systematically differ from the rest of the population, we also present summary statistics for different groups and conduct mean difference tests where standard errors are clustered by household. First, Panel A2 of Table 2 separately presents summary statistics for risky asset market participants and nonparticipants. On almost all observable dimensions, the households participating in risky asset markets seem to differ from the nonparticipant households. The average cumulative number of disasters is larger for risky asset market participants, which implies that they choose to live in locations where they are likely repeatedly subjected to disasters. The means of all dollar-valued variables are also greater for risky asset market participants. Most risky asset market participants (93%) also participate in the safe asset market. Second, Panel B provides the same summary statistics for households grouped by their residence: high-disaster-prone and low-disaster-prone counties. We categorize each county as either a high-disaster-prone county or a low-disaster-prone county based on whether the

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total number of disasters that occurred in that county is above or below the median value of the total number of disasters for all counties over the sample period. As expected, households in high-disaster-prone areas, on average, experienced more disasters (8.71 disasters vs. 5.31 disasters). The means of all dollar-valued variables are greater for households residing in high-disaster-prone areas. Third, Panel C repeats the same analysis for households that experienced an above-median and below-median average number of disasters over the sample period. By definition, the average cumulative number of disasters is greater for households with above-median disaster experiences. All dollar-valued variables are, on average, greater for these households.

III. Risk-Taking Behavior and Disaster Experiences

A. The Effect of Disaster Experiences on Risky Asset Market Participation

We examine the effect of disaster experiences on the decision to participate in the risky asset market using the following linear probability model:

$$\text{(1)} \qquad \text{PARTICIPATION}_{it} = \beta \ln(1 + \text{CUMNUM\_OF\_DISASTERS}_{it}) + \gamma' X_{it} + h_i + a_{it} + \tau_{ct} + \varepsilon_{it},$$

where i indexes household and t year; PARTICIPATION is an indicator of risky asset market participation for household i at year t, $\boldsymbol{X}_{it}$ is a vector of control variables (log income, log income squared, 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) commonly included in the literature (e.g., Malmendier and Nagel (2011)), and h_i, a_{it}, and τ_{ct} indicate household, age, and county-by-year fixed effects, respectively. Nonliquid financial assets (e.g., residential properties) may affect risky asset market participation decision, and we examine this possibility in Section IV.C.[8] The coefficient of interest is β, and we expect it to be negative. We use the sample weights in all our regression specifications to derive the point estimates. Specifically, the point estimates are calculated from the transformed variables that are obtained by multiplying every original variable in the regression by the square root of the corresponding sample weight. We cluster the standard errors by county and check robustness of the main results by estimating spatial heteroskedasticity and autocorrelation consistent (HAC) standard errors following Conley (1999).[9]

Certain types of households (e.g., risky asset market participants, households in high-disaster-prone counties, and households that experienced an above-median number of disasters) differ systematically from the rest of the population on many observed dimensions (Table 2). Therefore, these households are likely to differ from

[8]An additional control variable that is potentially important is private insurance, especially related to housing. Unfortunately, we do not observe homeowners' housing insurance data in the NLSY79 sample. One might expect the negative effect of disaster experiences on risk-taking behavior to be weaker for those who have such insurance. However, in Section IV, we find that only a small fraction of the main effect is explained by income and wealth shocks, which suggests that insurance likely plays little role in explaining our results.

[9]Clustering standard errors by household produces nearly identical statistics for our estimated coefficients.

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their counterparts on unobservables as well. We note that they choose to live in locations where they are repeatedly subjected to disasters. Furthermore, the set of locations where disasters recur is geographically concentrated (Texas, California, Oklahoma, Florida, New York, and Washington according to Panel B of Table 1). This is one of the important potential sources of endogeneity. An ideal test would be to compare the risk-taking behavior of two households that are similar on all observable and unobservable dimensions but experienced different numbers of disasters. The identifying assumption is that any household-level unobservable factors that might simultaneously affect households' risk-taking behavior and disaster experiences are time-invariant. To implement such a test, we include household fixed effects in all specifications and use within-household variation in disaster experiences to estimate the economic impact. We additionally include county-by-year fixed effects to control for regional trends that might drive both disaster experiences and risky asset market participation.

Columns 1 and 2 of Table 3 present results from the linear probability models with household fixed effects. The coefficient on ln(1 + CUMNUM_OF_DISASTERS) remains negative and significant at the 1% level even when we include county-by-year fixed effects in column 2. In all specifications, county-level

TABLE 3

Risk-Taking Behavior and Disaster Experiences

Columns 1 and 2 of Table 3 present the 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 (we obtain similar results using data in other years); columns 4 and 5 show OLS regressions of the fraction of liquid assets invested in risky assets on households' disaster experiences. We use the FEMA Disaster Declarations database. CUMNUM_OF_DISASTERS is a household's total number of disaster experiences up to the current survey year, and ln(1 + CUMNUM_OF_DISASTERS) is the log transformation of CUMNUM_OF_DISASTERS. Definitions of other variables are described in Table 1. To conserve space, we do not report the estimates of other control variables (a full version is available in Supplementary Material Section K). 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 0.439 and 0.379 in the sample). 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 (*SE clustered by County*) or spatial heteroskedasticity and autocorrelation consistent (HAC) standard errors (*Spatial HAC SE*) with the distance cutoff of 250 kilometers following Conley (1999). ***, **, 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
ln(1 + CUMNUM_OF_DISASTERS)	−.032***	−.029***	−.034**	−.023***	−.021**
(*SE clustered by county*)	(.009)	(.010)	(.015)	(.008)	(.009)
(*Spatial HAC SE*)	(.008)	(.008)	(.011)	(.007)	(.007)
Income and liquid assets	Yes	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes	Yes
Age and year FE	Yes	No	No	Yes	No
Age and county-by-year FE	No	Yes	Yes	No	Yes
Fitted values at $\mu - \sigma$ of DE (=2)	0.466			0.400	
Fitted values. at μ of DE (=8)	0.432			0.374	
Diff. between two fitted values	−0.035***			−0.026***	
	[−7.89%]			[−6.76%]	
Fitted values at μ of DE (=8)	0.432			0.374	
Fitted values at $\mu + \sigma$ of DE (=14)	0.415			0.362	
Diff. between two fitted values	−0.016***			−0.012***	
	[−3.67%]			[−3.14%]	
Unconditional sample mean	0.439			0.379	
No. of obs.	107,776	107,776	54,774	81,566	81,566
Adj. R^2	0.629	0.635	0.595	0.642	0.648

clustered standard errors are slightly larger than the spatial HAC standard errors with the distance cutoff of 250 km. Based on column 1, we gauge the economic significance of our identified effect. As we move from 1-standard-deviation below the average to the average of disaster experiences, the participation rate falls from 46.6% to 43.2%, about 3.5 percentage points, a 7.9% decrease relative to the sample mean of PARTICIPATION (43.9%), which is a sizable effect and significant at the 1% level. Such a decrease in risky asset market participation rate is roughly comparable to that associated with a decrease in INCOME (LIQUID_ASSETS) by 33% (23%) relative to the mean INCOME (LIQUID_ASSETS) of $82,958 ($108,879). Because of the nonlinearity of the independent variable, moving from the sample average to 1-standard-deviation above the average yields a different economic magnitude: 1.6 percentage points, a 3.7% decrease relative to the sample mean.

It is important to note that our estimated economic impact manifests for all households that experience the average number of disasters in our sample and is not driven by a select few who experienced an extreme number of disasters. Moreover, the effect of natural disaster experiences on risky asset market participation does not fade away with wealth. This can be one of the explanations for why a significant fraction of wealthy people do not participate in risky asset market, even if the fixed participation cost is relatively negligible to them. In column 3, we repeat column 2 by restricting the sample to households with above-median FINANCIAL_ASSETS based on the survey year of 1988.[10] The effect of disaster experiences on wealthy households remains strong both economically and statistically. In fact, the effect is somewhat larger (−0.034) than that (−0.029) in the whole sample, while it is now significant at the 5% level since the number of observations becomes half of the overall sample. In order for natural disaster experiences to explain the puzzling lack of participation for the wealthy, the exposure to disasters must be pervasive even at high levels of wealth. The mean (standard deviation) of cumulative number of disasters for all households is 7.63 (5.78), and the same statistic for wealthy households is 7.71 (6.19), which indicates that there is little difference in the degree to which the wealthy are exposed to natural disaster events. This makes sure that our findings can partly account for the lack of participation even among wealthy households.

B. The Effect of Disaster Experiences on Risky Asset Share

The second analysis regresses the fraction of liquid assets invested in risky assets on the same set of covariates[11]:

$$\text{(2)} \quad \text{RISKY\_SHARE}_{it} = \beta \ln(1 + \text{CUMNUM\_OF\_DISASTERS}_{it}) + \gamma' X_{it} + h_i + a_{it} + \tau_{ct} + \varepsilon_{it}.$$

It is especially important to include county-by-year fixed effects and liquid assets as controls in this specification because of local bias (Coval and Moskowitz

[10]We obtain similar results using financial assets data in other years to define "wealthy households" because wealthy classification is highly persistent over time.

[11]To control for the potential fixed costs of risky asset market participation, we also run these regressions conditioning on participation – that is, only for risky asset market participants. We obtain similar results.

(1999)) and the real effect (output losses) of natural disasters on local firms (Barrot and Sauvagnat (2016)). Households residing in a high-disaster prone area may hold a disproportionately larger fraction of firms in the same local area in their risky asset portfolios compared to households residing in a low-disaster prone area. The real output (e.g., sales growth) of those firms drops significantly following a disaster, which might cause a decrease in their market value. Therefore, households in a high-disaster prone area are more likely to face a loss in their risky asset portfolios (hence, a decrease in our dependent variable), which consist of disproportionately more stocks of local firms after a disaster shock, even when they do not change their portfolios. County-by-year fixed effects and LIQUID_ASSETS (which is the sum of RISKY_ASSETS and SAFE_ASSETS) in the regression explicitly capture the effect of such a potentially mechanical relation.

Columns 4–5 in Table 3 show the results. Similar to columns 1–2 of the same table, disaster experiences are negatively associated with risky asset shares. The parameter estimates in column 4 imply that a 1-standard-deviation increase in disaster experiences to the mean from below (from the mean to above) decreases the risky asset share by 2.6 percentage points (1.2 percentage points), a 6.8% decrease (3.1% decrease) relative to the sample mean.

Figure 2 summarizes our main findings. The figure shows the fitted risky asset market participation rates in Graph A and risky asset shares in Graph B as a function of the cumulative number of disaster experiences based on the regression results in columns 1 and 4 of Table 3. We calculate the predicted values by holding all other variables constant at their sample mean while varying the cumulative number of disaster experiences. Shaded areas represent 90% confidence intervals of the point estimates. As shown in this figure, most of the impact on participation rates comes from a change in the number of disasters around its mean. Therefore, our finding is not driven by a select few who experienced an extreme number of disasters. We also note that the marginal effect of disaster experiences on risk-taking behavior decreases as the cumulative level of disaster experiences increases. This indicates that households may assign different weights on each disaster they have experienced. In the next section, we examine and discuss, in great detail, a framework for households' weighting scheme (both across time and the severity of disasters) regarding their disaster experiences consistent with our regression results.

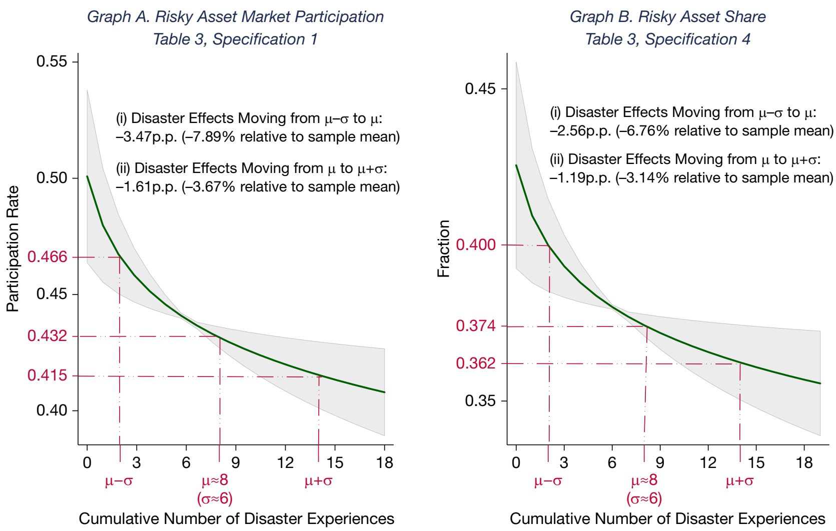
C. Interpretation of the Results

According to the model of memory by Bordalo, Gennaioli, and Shleifer (2020), when households think about natural disasters, they automatically retrieve past experiences from memory and aggregate them into a norm. Once the households encounter another (actual) disaster, their norms respond to these new stimuli, and they accordingly adjust their preferences based on the discrepancies between the norm and reality. In this framework, for the households who already have experienced many disasters, the reality (new disaster) is close to their retrieved norms; hence its salience is low, and their adjustment in risk-taking behavior is minimal ("adaptation from experience"). Likewise, for the households with fewer disaster experiences, the discrepancy between the new disaster and their retrieved norms is salient, and adjustment is large. Motivated by this theory, we conjecture

FIGURE 2

Risk-Taking Behavior and Disaster Experiences

Graph A of Figure 2 shows the relation between fitted risky asset market participation rates (*y*-axis) and the cumulative number of disaster experiences (*x*-axis) using the regression specification in column 1 of Table 6. Graph B presents the relation between the fitted fraction of the liquid assets invested in risky assets (risky asset share, *y*-axis) and the cumulative number of disaster experiences (*x*-axis) using the regression specification in column 4 of Table 6. In both regression models, we calculate the predicted values by holding all other variables constant at their sample mean, while varying the cumulative number of disaster experiences. Shaded areas represent 90% confidence intervals of the point estimates. The range of the *x*-axis, cumulative number of disaster experiences, is chosen according to the sample mean and standard deviation of the data. The figures also present the economic significance of the *y*-variable (relative to its sample mean) for a standard deviation change in the disaster experiences around its mean. Observations are weighted by the NLSY79 sample weights.



that for a given household, the twentieth disaster experience has a lower marginal effect on its risk-taking behavior than the second. The results presented in Figure 2 and discussed in Section III.B are indeed consistent with this model.

This theory also implies that the past disaster experiences matter less because they have already been aggregated into a norm, and, thus, it is only deviations from the norm that matter for future behavior. To examine how much higher the weights are that households assign to recent experiences compared to earlier ones, we develop two tests. First, we compare the effects of early life (between the ages of 5 and 15 years) and later-life (after the age of 15 years) experiences on households' risk-taking behavior. According to this theory, we conjecture that later-life experiences are likely more informative than early life experiences in explaining portfolio choice decisions. We find that later-life experiences are statistically and economically significant, whereas early life experiences are not important in influencing households' risk-taking behavior. These regression results are presented and discussed in Supplementary Material Section E.1.

As a second test, we formally estimate a weighting scheme of household experiences in portfolio choice decisions. Toward this end, we adopt a nonlinear regression model that is used in Malmendier and Nagel (2011). Supplementary Material Section E.2 provides the details of the estimation procedure and a discussion of the results. These results suggest that the households' weighting scheme is decreasing and convex in the time lag between the disaster occurrence and the current survey year. This means households weight the recent disaster experiences much more heavily compared to the earlier experiences in making asset allocation decisions. Taken together, these tests are consistent with the theoretical model of Bordalo et al. (2020).

According to Smith and Katz (2013), only 133 "billion-dollar" disasters (those with losses exceeding $1 billion each across the United States) account for about 80% of the total U.S. losses ($881 billion out of $1,100 billion) for all severe weather and climate events during the 1980–2011 period. This indicates that the damage of natural disasters exhibits highly right-skewed distribution with a large variation. Hence, we examine the relative weighting of such "super-severe" and severe disaster experiences in shaping households' risk-taking behavior. Based on the theory (Bordalo et al. (2020)), we conjecture that super-severe disasters have a greater impact than severe ones on households' portfolio choice decisions because the discrepancy between the new disaster and their retrieved norms would be more salient when households experience super-severe disasters.

FEMA, unfortunately, does not report any damages of FEMA-declared disasters. Instead, we use the EM-DAT database and manually match the FEMA disasters with the EM-DAT disasters to draw information on disaster severity from the EM-DAT data set. The detailed matching procedure is provided in Supplementary Material Section F. We also explain, in the same section, why the Spatial Hazard Events and Losses Database for the United States (SHELDUS) is unreliable to calculate damages of U.S. natural disasters. We use four measures of disaster severity available in the EM-DAT database: total death, total damage, total affected, and total uninsured losses. We then classify each disaster as super severe if its severity metric exceeds the median of the severity distribution (only using nonzero observations for the distribution). We illustrate this procedure using total deaths as a metric. Out of the 784 disasters in the EM-DAT database, 513 have recorded nonzero fatalities. The median of these nonzero fatalities distribution is 11, and we classify about 264 above-median disasters (accounting for multiple ties) as super-severe ones. Handmatching these 264 EM-DAT disasters with the FEMA database produces 709 corresponding observations (about 23% of the total of 3,061 FEMA declarations).

Our next step is to construct a weighted disaster-experience variable for each household. We emphasize that we do not opt to run a horse race comparing the severe and super-severe disaster experiences. Our hypothesis relies on how the *cumulative* experiences of *both* severe and super-severe disasters of households affect their behavior rather than on an arbitrary dichotomy of either super-severe or severe disaster experiences alone affecting their choices. An ideal weighted disaster experiences variable would have weights determined by the severity of disasters. As noted earlier, the information on disaster-induced damages is only available for disasters that are covered by the EM-DAT database. Moreover, these severity measures (hence weights) are only available at the disaster level, not at the county or household level (which is needed for the regressions), which introduces measurement errors. Finally and most importantly, we lack any theoretical guidance on how households weight super-severe and severe disasters. For these reasons, we conduct sensitivity analyses to shed light on how varying households' weighting schemes affect their risk-taking behavior.

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Specifically, we examine the impact of different weighting schemes on super-severe disasters (using the total deaths as a metric) in households' portfolio choice decisions. We assign weights on super-severe disasters from $\frac{1}{10}$ to 10, relative to severe disasters (i.e., the weight of severe disasters = 1). For each scenario, we construct ln(1 + WCUMNUM_OF_DISASTERS) as the log transformation of a household's total weighted number of disaster experiences up to the current survey year. We then reestimate our main regression specification on risky asset market participation (column 2 of Table 3). Panel A of Table 4 tabulates the

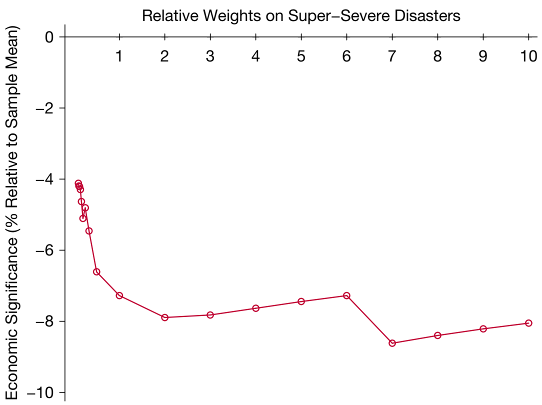
TABLE 4

Relative Importance of Super-Severe and Severe Disaster Experiences

Table 4 examines how the relation between the households' disaster experiences either with their risky asset market participation (Panel A) or with the fraction of risky assets in their portfolio (Panel B) depends on different weighting schemes on super-severe disasters relative to severe disasters. Panels C1 and C2 examine the effect of weighted disaster experiences on households' risk-taking behavior using various measures of disaster severity in identifying super-severe disasters. We first classify each disaster as super-severe if its total death, which is extracted from the EM-DAT, exceeds the median of the total death distribution (Panels A and B) or if its severity metric exceeds the median of the corresponding severity distribution (Panel C), using only nonzero observations. We then classify all other disasters in the FEMA database as severe. We use four measures which are extracted from the EM-DAT: total death, total damage, total affected, and total uninsured losses. The definition of these metrics is provided in Section II.B.2. We assign weights ranging from $\frac{1}{10}$ to 10 to super-severe disasters, relative to severe disasters (i.e., the weight of severe disasters is 1). For each scenario, we construct ln(1 + WCUMNUM_OF_DISASTERS) as the log transformation of a household's total weighted number of disaster experiences up to the current survey year, in which we assign a weight of 2 to super-severe disasters, relative to severe disasters. We reestimate columns 2 and 5 of Table 6 using the weighted disaster experiences. We calculate the economic significance as the change in the dependent variable for a 1-standard-deviation change in the independent variable around the mean, expressed as a percentage of the sample mean of the dependent variable. Observations are weighted by the NLSY79 sample weights. Numbers in parentheses are standard errors that are clustered by county, and *t*-statistics are based on those numbers. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively. The graphs below the panels in this table plot the economic significance of weighted disaster experiences as a function of relative weights on super-severe disasters.

Panel A. Weighting Schemes on Super-Severe Versus Severe Disasters: Risky Asset Market Participation

More Weights on Super-Severe Disasters			Less Weights on Super-Severe Disasters		
Relative Weights	Coefficient Estimates	*t*-Stat	Relative Weights	Coefficient Estimates	*t*-Stat
1	−0.029	−2.933***	1	−0.029	−2.933***
2	−0.027	−3.215***	1/2	−0.023	−2.599***
3	−0.023	−3.249***	1/3	−0.020	−2.433**
4	−0.021	−3.219***	1/4	−0.018	−2.337**
5	−0.019	−3.174***	1/5	−0.017	−2.276**
6	−0.017	−3.127***	1/6	−0.016	−2.234**
7	−0.016	−3.083***	1/7	−0.015	−2.202**
8	−0.015	−3.043***	1/8	−0.014	−2.178**
9	−0.014	−3.006***	1/9	−0.014	−2.158**
10	−0.014	−2.973***	1/10	−0.013	−2.142**



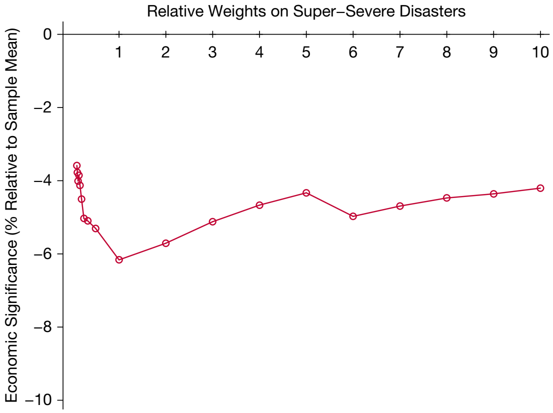
(continued on next page)

TABLE 4 (continued)

Relative Importance of Super-Severe and Severe Disaster Experiences

Panel B. Weighting Schemes on Super-Severe Versus Severe Disasters: Risky Asset Share

More Weights on Super-Severe Disasters			Less Weights on Super-Severe Disasters		
Relative Weights	Coefficient Estimates	*t*-Stat	Relative Weights	Coefficient Estimates	*t*-Stat
1	−0.021	−2.482**	1	−0.021	−2.482**
2	−0.017	−2.451**	1/2	−0.018	−2.252**
3	−0.013	−2.302**	1/3	−0.016	−2.110**
4	−0.011	−2.149**	1/4	−0.016	−2.019**
5	−0.009	−2.017**	1/5	−0.013	−1.956*
6	−0.008	−1.907*	1/6	−0.013	−1.909*
7	−0.008	−1.817*	1/7	−0.012	−1.872*
8	−0.007	−1.742*	1/8	−0.011	−1.842*
9	−0.006	−1.680*	1/9	−0.011	−1.817*
10	−0.006	−1.627	1/10	−0.011	−1.796*



Panel C1. Weighted Experiences Using Various Severity Measures: Risky Asset Market Participation

	PARTICIPATION				
	Death	Damage	Affected	Uninsured Losses	Unweighted
	1	2	3	4	5
ln(1 + WCUMNUM_OF_DISASTERS)	−.027*** (.008)	−.025*** (.008)	−.028*** (.008)	−.025*** (.008)	
ln(1 + CUMNUM_OF_DISASTERS)					−.029*** (.010)
Income/Liquid assets/HH Characteristics	Yes	Yes	Yes	Yes	Yes
Household, age, and county-by-year FE	Yes	Yes	Yes	Yes	Yes
Economic significance	[−7.89%]	[−7.38%]	[−10.21%]	[−7.40%]	[−7.28%]
No. of obs.	107,776	107,776	107,776	107,776	107,776
Adj. R^2	0.635	0.635	0.635	0.635	0.635

Panel C2. Weighted Experiences Using Various Severity Measures: Risky Asset Share

	RISKY_SHARE				
	Death	Damage	Affected	Uninsured Losses	Unweighted
	1	2	3	4	5
ln(1 + WCUMNUM_OF_DISASTERS)	−.017** (.007)	−.017** (.007)	−.021*** (.007)	−.018*** (.007)	
ln(1 + CUMNUM_OF_DISASTERS)					−.021** (.009)
Income/Liquid assets/HH characteristics	Yes	Yes	Yes	Yes	Yes
Household, age, and county-by-year FE	Yes	Yes	Yes	Yes	Yes
Economic significance	[−5.71%]	[−5.80%]	[−8.96%]	[−6.28%]	[−6.16%]
No. of obs.	81,566	81,566	81,566	81,566	81,566
Adj. R^2	0.648	0.648	0.648	0.648	0.648

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estimated coefficients on the weighted disaster experiences with the corresponding t-statistics.

As shown in Panel A of Table 4, in all scenarios, the estimated coefficients on weighted disaster experiences are negative and statistically significant: the estimates range from −0.013 to −0.029. These results indicate that disaster experiences have a significant statistical impact on portfolio choice decisions regardless of how the households weigh super-severe disasters relative to severe disasters. To gauge the economic significance of disaster experiences at various weighting schemes, we calculate the change in the dependent variable for a 1-standard-deviation change in the independent variable around the mean, expressed as a percentage of the sample mean of the dependent variable. The graphs below the panels in Table 4 plot the economic significance as a function of relative weights on super-severe disasters. As shown in this figure, the impact of disaster experiences on risky asset market participation generally increases in magnitude as the relative weight accorded by households on super-severe disasters increases. This result suggests that super-severe disaster experiences are perhaps more important in a household's risky asset market participation decision, consistent with theoretical framework of (Bordalo et al. (2020)). In Panel B of Table 4, we repeat the same exercise for the fraction of risky assets in the household's portfolio. We find the results to be statistically significant and economically important in almost all cases. Interestingly, the economic significance seems to be relatively constant over a wide range of weights on the super-severe disasters.

We also examine the robustness of these results to the use of other disaster-severity measures (e.g., total damage, total affected, and total uninsured losses) from the EM-DAT data in identifying super-severe disasters. Since the effect of disaster experiences on portfolio choice is quite robust to the relative weights on super-severe disasters (as shown in Panels A and B of Table 4), we assign a relative weight of two to super-severe disasters for brevity. We then reestimate columns 2 and 5 of Table 3 using the weighted disaster experiences for each severity measure.

Panel C1 of Table 4 reports the estimated coefficients on the weighted disaster experiences for risky asset market participation. The coefficients in columns 1–4 are all negative and statistically significant at the 1% level. These results suggest that disaster experiences adversely affect households' risky asset market participation regardless of the method used to identify super-severe disasters and assigning a higher weight to super-severe disasters. The estimated coefficients in columns 1–4 are similar in magnitude (ranging from −0.025 to −0.028) to that on unweighted disaster experiences (−0.029) reported in column 5. Furthermore, the economic impact of weighted disaster experiences is comparable to that of unweighted disaster experiences except for column 3 in which we use total affected to identify super-severe disasters. We obtain qualitatively similar findings for the risky asset share. The results are tabulated in Panel C2. The main conclusion we draw from these regressions is that both severe and super-severe disasters seem to matter for portfolio choice, with super-severe disasters eliciting a slightly stronger effect if the households weight them more.

We now rationalize how disasters with less ex post economic damage (i.e., severe disasters in our study) can still induce a nontrivial response in households by noting that households might simply develop an aversion to dealing with the uncertainty of how disasters could affect their financial condition, thereby going forward to allocate a lower proportion of their portfolio to risky assets. In parallel, Ekeberg, Seeberg, and Ellertsen (1989) show that 27% of their survey respondents have aviophobia (fear of flying) mainly due to the anticipated effects of turbulence or engine trouble. In addition, *The Economist* (2016) notes that these people are usually aware that air travel is one of the safest forms of transportation yet are unable to shake off anxieties about crashing or losing self-control. In a similar vein, even ex post, severe disasters could cause as much financial anxiety as super-severe disasters could. To deal with such anxiety, households might avoid risky investments in the future, just as fliers might minimize or eschew air travel altogether.

D. Potential Bias Due to Political Nature of the FEMA Declaration: Examination of the NCEI Database

As noted in Section II.B.1, our results may be affected by the political nature of the FEMA disaster declaration process. To examine this issue further, we undertake a detailed investigation utilizing one other database, the NCEI disaster database.[12] 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.

We first examine the overlap between the NCEI billion-dollar disasters and the FEMA disasters to detect the impact of political biases if any, in the declaration of super-severe disasters. The NCEI Billion-Dollar Disasters data cover super-severe natural disasters in the United States, but only from 1980 to the present day (Our FEMA data sample spans 1964–2013). Each disaster's losses exceed 1 billion in today's dollars. We manually match each NCEI billion-dollar disaster with the FEMA database. The details on the matching procedure are provided in Supplementary Material Section G. Our assembled database of FEMA disasters cover 96% by number of the 160 NCEI billion-dollar disasters for the period from 1980 to 2013. This overlap corresponds to 99% by amount of the total losses ($0.968 trillion) of these 160 NCEI billion-dollar disasters. We conclude that virtually all super-severe disasters in the NCEI database are covered by the FEMA data during 1980–2013, the period of overlap between the two databases.

We then examine the overlap between the NCEI Storm Events database and the FEMA database in coverage of disasters with below $1 billion in losses. The NCEI Storm Events database provides granular information on U.S. disasters at the county level. The data contain the occurrence of storms and other significant weather phenomena from Jan. 1950 to Oct. 2020. From 1964 to 1995 (which is 64% of our entire sample period of the FEMA database), the Storm Events database only recorded three types of disaster events, including tornado, thunderstorm wind, and hail. In contrast, the FEMA database we use covers 17 different types of disasters for the same period. Therefore, for the period of 1964 to 1995, the FEMA database provides the best coverage among all available data.

[12]We thank the referee for suggesting this approach.

Starting from 1996, the NCEI Storm Events database significantly expanded its coverage to include 48 different types of disaster events. Therefore, for the period of 1996 to 2013, we identify severe and super-severe disasters in the NCEI Storm Events database that are not covered by the FEMA database. The detailed step-by-step procedure we used is outlined in Supplementary Material Section G. 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. Including these additional events to the FEMA database (which has a total of 43,350 county events during our sample period) 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 also examine the scope of the reverse sort of bias: whether there are declared disasters in the FEMA database that might be politically motivated, but are not severe or super severe events to merit inclusion in the NCEI database. We identify 132 county-level disaster events in the FEMA database (0.30% of the total FEMA data) that are not covered by the NCEI Storm Events database and cannot be independently verified by newspaper stories from LexisNexis.

Armed with this exhaustive revision, we examine the impact of correcting both types of biases on our baseline inferences. For the period of 1996 to 2013, we conservatively include 6,462 county events from the NCEI Storm Events database that are excluded in the FEMA database (the upper bound of events potentially excluded by FEMA) and also exclude 132 county events in the FEMA database that could not be found in the NCEI database or in newspaper stories from LexisNexis. We recalculate our disaster experience variable for each household and reestimate the effect of disaster experiences on households' risk-taking behavior. We use the same regression specifications as in Table 3. Table 5 presents the estimation results using the new data set. We report our baseline results from Table 6 that use the original FEMA database for easy comparison across the estimates. The estimated effect of disaster experiences on portfolio choice is statistically robust to the adjustment of the FEMA database using the NCEI database (where available). The economic significance of the disaster experience variable uniformly becomes stronger than our baseline results as shown in the comparison in Table 5 across all specifications. Based on these results, we conclude that the political nature of the FEMA declarations process to systematically include or exclude disasters in the database (to the extent that the data are available in both databases from 1996 to 2013) is unlikely to impact our inferences materially. Since the NCEI database does not cover events during the time period from 1964 to 1995, we revert to our original data set for further tests in the rest of this article.

E. Additional Robustness Tests

We first conduct a placebo test to evaluate whether randomly created pseudo disaster experiences of households affect their participation in risky asset markets. We randomly assign the history of disaster experiences during the entire sample period to each household to construct a pseudo disaster experience variable, $\text{CUMNUM\_OF\_DISASTERS}^{\text{Pseudo}}$. We note that it is important to maintain the

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TABLE 5

Potential Bias Due to Political Nature of the FEMA Declaration

Table 5 examines whether and how political considerations in the FEMA disaster declaration affect the effect of disaster experiences on households' risk-taking behavior. To this end, we adjust the FEMA data using the NCEI Storm Events database. We first include additional 6,462 county events in the NCEI Storm Events database that are not covered by the FEMA database. We then exclude 132 county events in the FEMA data that are not covered by the NCEI Storm Events database and cannot be verified by newspaper stories from LexisNexis. With this new data set, we reestimate the effect of disaster experiences on households' risk-taking behavior using the same regression specifications in Table 3. CUMNUM_OF_DISASTERS is a household's total number of disaster experiences up to the current survey year, and ln(1 + CUMNUM_OF_DISASTERS) is the log transformation of CUMNUM_OF_DISASTERS. Definitions of other variables are described in Table 1. To conserve space, we do not report the estimates of other control variables. We calculate the economic significance as the change in the dependent variable for a 1-standard-deviation change in the independent variable around the mean, expressed as a percentage of the sample mean of the dependent variable. 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. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

	PARTICIPATION		RISKY_SHARE	
	1	2	3	4
Baseline results using the FEMA database				
ln(1 + CUMNUM_OF_DISASTERS)	−.032***	−.029***	−.023***	−.021**
	(.009)	(.010)	(.008)	(.009)
Adjusting FEMA database using NCEI storm events				
ln(1 + CUMNUM_OF_DISASTERS)	−.039***	−.032***	−.021***	−.019**
	(.011)	(.011)	(.008)	(.009)
Income and liquid assets	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Household FE	Yes	Yes	Yes	Yes
Age and year FE	Yes	No	Yes	No
Age and county-by-year FE	No	Yes	No	Yes
Economic significance: Change in Y-variable (as a % of the mean) for a change in disaster experiences by 1σ around μ				
	Column 1		Column 3	
Change in disaster experiences→	$\mu-\sigma$ to μ	μ to $\mu+\sigma$	$\mu-\sigma$ to μ	μ to $\mu+\sigma$
Baseline results using the FEMA database	[−7.89%]	[−3.67%]	[−6.76%]	[−3.14%]
Adjusting FEMA database using NCEI storm events	[−14.15%]	[−5.17%]	[−8.80%]	[−3.21%]

spatial distribution of natural disasters and the panel structure of residence locations of households in this random assignment. We then run the same regression, as in column 1 of Table 3, 1,000 times and save the coefficients on the pseudo cumulative disaster experience variable. Figure 3 plots a density of these coefficients. The green line in the panel shows the estimated kernel densities. We then compare whether the actual coefficient estimate of the cumulative disaster experience variable (column 1 in Table 3) falls on the generated distributions. The red vertical line indicates the actual estimate. This suggests that our actual regression coefficient estimate falls on the extreme left tail of the distribution, which is consistent with the p-values of the actual estimate being 0.000. Thus these results imply that the documented effect of disaster experiences on portfolio choice cannot be obtained by random chance alone.

We also conduct a matched sample analysis as an alternative to the regression analyses. Households experiencing a large number of disasters and those experiencing a small number of disasters are matched to have a more balanced and overlapping distribution of household characteristics. Details of the matching procedure and results using the matched sample are discussed in Supplementary Material Section G. The results show that a greater number of disaster experiences is associated with portfolio choices that are more conservative, which confirms our main findings in Table 3.

TABLE 6

The Effects of Income and Wealth Shocks

Panel A of Table 6 examines to what extent an income or wealth channel explains the main results by adding proxies for income and wealth shocks in a seriatim fashion. We use income, liquid financial assets, home-ownership, and local economic conditions (using county-by-year fixed effects) as proxies for income and wealth shocks that accompany financial disasters. Column 1 excludes all these proxies and column 5 includes all of them. Columns 1–5 present the results from linear probability models of risky asset market participation in households' disaster experiences whereas columns 6–10 show OLS regressions of the fraction of liquid assets invested in risky assets in households' disaster experiences. In Panel B of Table 6, columns 1–2 repeat column 2 in Table 6, and columns 3–4 repeat column 5 in Table 6, using two different measures of health status as proxies for income shock. HEALTH_LIMIT_AMOUNT (HEALTH_LIMIT_KIND) indicator variable denotes whether household heads think that the amount (kind) of work they can do is limited by their health. Panel C examines the effects of disaster experiences on risk-taking behavior only for households that do not own their houses (columns 1 and 3) and with housing variables as proxies for wealth shock (columns 2 and 4). CUMNUM_OF_DISASTERS is a household's total number of disaster experiences up to the current survey year. MVRP is the market value of residential property, MDRP is the mortgage and debt value of residential property, and NET_WEALTH is the sum of RISKY_ASSETS, SAFE_ASSETS, and net value of residential property (MVRP-MDRP). Definitions of all other variables are described in Table 1. 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. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Adding Proxies for Income and Wealth Shocks in a Seriatim Fashion

	PARTICIPATION				
	All Excluded	Col 1 + Income	Col 2 + Liquid Assets	Col 3 + Home Ownership	Col 4 + Local Economic Cond.
	1	2	3	4	5
ln(1 + CUMNUM_OF_DISASTERS)	−.040***	−.041***	−.0315***	−.0321***	−.030***
	(.011)	(.011)	(.009)	(.009)	(.010)
Economic significance	[−9.95%]	[−10.21%]	[−7.89%]	[−8.03%]	[−7.44%]
No. of obs.	109,145	108,369	107,776	107,776	107,776
Adj. R^2	0.536	0.542	0.629	0.629	0.635
	RISKY_SHARE				
	All Excluded	Col6 + Income	Col7 + Liquid Assets	Col8 + Home Ownership	Col9 + Local Economic Cond.
	6	7	8	9	10
ln(1 + CUMNUM_OF_DISASTERS)	−.025***	−.026***	−.023***	−.024***	−.022**
	(.009)	(.009)	(.008)	(.008)	(.009)
Economic significance	[−7.17%]	[−7.49%]	[−6.76%]	[−6.93%]	[−6.39%]
Income	No	Yes	Yes	Yes	Yes
Liquid assets	No	No	Yes	Yes	Yes
Homeownership	No	No	No	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes	Yes
Household and age FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	No
County-by-year FE	No	No	No	No	Yes
No. of obs.	82,130	81,566	81,566	81,566	81,566
Adj. R^2	0.590	0.591	0.642	0.643	0.649

	PARTICIPATION		RISKY_SHARE	
	1	2	3	4
Panel B. Income Channel via Health Status				
ln(1 + CUMNUM_OF_DISASTERS)	−.030***	−.029***	−.020**	−.021**
	(.010)	(.010)	(.009)	(.009)
HEALTH_LIMIT_AMOUNT	−.002		−.005	
	(.008)		(.007)	
HEALTH_LIMIT_KIND		−.003		−.004
		(.006)		(.006)
Income/Liquid assets	Yes	Yes	Yes	Yes
Household characteristics/FE	Yes	Yes	Yes	Yes
Age and county-by-year FE	Yes	Yes	Yes	Yes
No. of obs.	104,722	107,686	80,195	81,511
Adj. R^2	0.633	0.635	0.649	0.648
Panel C. Does Housing Market Drive These Findings?				
ln(1 + CUMNUM_OF_DISASTERS)	−.034***	−.020**	−.023*	−.019**
	(.012)	(.010)	(.013)	(.009)
MVRP/NET_WEALTH		.025***		−.008
		(.008)		(.007)
MDRP/NET_WEALTH		−.019**		.012
		(.009)		(.008)
Income/Liquid assets	Yes	Yes	Yes	Yes
Household characteristics/F.E.	Yes	Yes	Yes	Yes
Age and county-by-year FE	Yes	Yes	Yes	Yes
No. of obs.	64,175	84,304	43,302	61,747
Adj. R^2	0.640	0.625	0.692	0.602

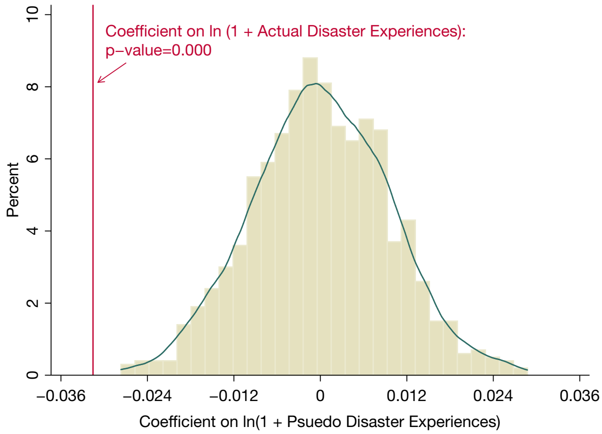
FIGURE 3

Risky Asset Market Participation and Disaster Experiences: Placebo Test

Figure 3 is based on the following empirical model:

$$\text{PARTICIPATION}_{it} = \beta^{\text{Pseudo}} \ln\left(1 + \text{CUMNUM\_OF\_DISASTERS}_{it}^{\text{Pseudo}}\right) + \gamma' X_{it} + h_i + a_{it} + \tau_t + \varepsilon_{it},$$

where $\text{PARTICIPATION}_{it}$ is an indicator for risky asset market participation of household i in year t, $\mathbf{X}_{it}$ is a vector of control variables, and h_i, a_{it}, and τ_t indicate household, age, and year fixed effects, respectively. $\ln(1+\text{CUMNUM\_OF\_DISASTERS}_{it}^{\text{Pseudo}})$ is a log of household i's cumulative number of *hypothetical* disaster experiences up to time t. We randomly assign the history of disaster experiences during the entire sample period to each household. We run the above regression 1,000 times and save the $\widehat{\beta}^{\text{Pseudo}}$ value. The figure plots a density of $\widehat{\beta}^{\text{Pseudo}}$. The vertical red line indicates the actual $\widehat{\beta}$ obtained from the regressions based on the $\ln(1+\text{CUMNUM\_OF\_DISASTERS}_{it})$ (in column 1 of Table 6). The green lines show the kernel densities. Observations are weighted by the NLSY79 sample weights. The sample period runs from 1988 to 2012. Standard errors are clustered by county.



Finally, to ensure that personal, direct disaster experiences drive conservative investment decisions of households, we construct a measure of "indirect" disaster experiences: for each household, we count the number of disasters that occurred in the adjacent counties of the current residence of the household only when these disasters did not affect the county of the current residence. We then include both the indirect and direct (our original measure) disaster experience variables in the main regression models. The results reported in Supplementary Material Section H suggest that personal, direct experiences matter in portfolio choice; however, observing disasters in the adjacent counties has statistically weak impact on the individuals' risk-taking behavior.

IV. The Effects of Income and Wealth Shocks

Having established the robustness of the main findings, we now investigate the mechanisms by which disaster experiences affect households' risk-taking behavior. Motivated by the existing literature, we explore four different channels, including income shocks, wealth shocks, the role of homeownership, and the socioeconomic status of households. The main conclusion of these analyses is that a relatively small part of our main results can be explained by these channels.

A. Adding Proxies for Income and Wealth Shocks in a Seriatim Fashion

We examine to what extent an income or wealth channel explains our main results. Specifically, we start from the model without any proxies for income or wealth shocks and add, in a seriatim fashion, proxies for income and wealth shocks that accompany financial disasters. If income or wealth shocks were very important in explaining the portfolio choices of households, we expect the disaster experience variable to become less important in every successive regression with more controls for income and wealth shocks and perhaps even turn insignificant.

In column 1, Panel A of Table 6, we exclude all proxies for income and wealth shocks in estimating the effect of disaster experiences on risky asset market participation. These proxies include income, income squared, liquid financial assets, liquid financial assets squared, homeownership, and local economic conditions (using county-by-year fixed effects).[13] We include all other control variables used in column 2 of Table 3. In column 2, we add income and income squared to the specification in column 1. In column 3, we add liquid assets and liquid assets squared to the specification in column 2. In column 4, we additionally include a homeownership dummy variable that is set to one if households own their home, and zero otherwise, to the specification in column 3. Finally, in column 5, we include county-by-year fixed effects as a proxy for time-varying shocks to the local economy at the county level to the specification in column 4.

Panel A of Table 6 shows that income and wealth shocks reduce the impact of disaster experiences on risky asset market participation by about 25%: the magnitude of economic significance drops from 9.95% in column 1 to 7.44% in column 5 when all proxies for income, and wealth shocks in our database are included in the regressions. Similarly, for risky asset share, income and wealth shocks reduce the impact of disasters by about 11%: the magnitude of economic significance drops from 7.17% in column 1 to 6.39% in column 5 when all proxies for income and wealth shocks in our database are included in the regressions. These results indicate that a relatively small part of the main results can be explained by income, wealth, or local economic shocks due to disaster events. We conclude that there is a significant remaining effect that is captured by the disaster experience variable even after accounting for the income and wealth shocks. In the next section, we extend the analyses in Panel A of Table 6 by examining how income shocks due to diminished health status and housing shocks affect portfolio choice.

B. Income Channel Induced by Health Status

Strategic asset allocation models with nontradable human wealth (stochastic labor income) yield an optimal portfolio rule that also depends on the mean wealth-income ratio and covariance between risky asset returns and labor income

[13]A set of county-by-year fixed effects captures a lowered-income effect resulting from time-varying damage to the local economy (at county level) due to disasters. We additionally include a different set of geographic location fixed effects (state and state-by-year fixed effects) and explicitly control for macroeconomic conditions at the state level such as GDP growth, population, population density, and unemployment rates in our specifications. These robustness checks are reported in Supplementary Material Section I.

(Campbell and Viceira (2002), Cocco, Gomes, and Maenhout (2005), and Bonaparte, Korniotis, and Kumar (2014)). Hence changes in future income streams due to disaster shocks might also affect asset allocation decisions. Unfortunately, we cannot observe future income streams in the NLSY79 data. However, if disaster shocks damage an individual's health and as a result adversely affect the household's future income stream, including health status variables as controls in our regression specifications would capture this effect.

In Panel B of Table 6, we include HEALTH_LIMIT_AMOUNT (HEALTH_LIMIT_KIND) indicator variables that capture whether household heads think they are limited in the amount (kind) of work they can do because of their health. These variables assume a value of 1 if the household believes that they are limited in the amount (kind) of future jobs, and 0 otherwise. Controlling for the (diminished) health status of individuals does not alter our core results in all four specifications in columns 1–4: the coefficient on the disaster experiences variable remains statistically and economically significant. In all specifications, a household head's poor health status is negatively associated with portfolio choice decisions, which is qualitatively similar to the findings by Rosen and Wu (2004). However, these coefficients are not statistically significant in our estimations. Thus it appears that disaster experiences seem to matter for portfolio choice even after accounting for income shocks through diminished health status.

C. Housing Channel

Homeownership has long been central to the ability of U.S. households to amass wealth. Homeowners' median net worth is 10 times that of renters, and the difference is driven mainly by the value of home equity (Freeman and Quercia (2014)). Moreover, the literature about portfolio decisions documents that investment in housing plays an important role in households' financial decision-making (e.g., Cocco (2005), Yao and Zhang (2005), and Chetty, Sándor, and Szeidl (2017)). We, therefore, examine whether natural disasters destroy wealth by damaging the primary residence of homeowners and thus result in a mechanical reallocation of their portfolios away from risky assets.

To do so, we first estimate the effect of disaster experiences on portfolio choice decisions for households with no homeownership. These households make up about 60% of our sample. If we still find a strong negative effect for this subsample, it would indicate that the wealth loss due to home damage in natural disasters mechanically leading to a conservative portfolio cannot drive our results. Columns 1 and 3 in Panel C of Table 6, we examine the portfolio choice decisions of no-home households and find strong effects of disaster experiences for this subsample. The coefficients (−0.034 and −0.023) on the disaster experience variables are, in fact, larger in columns 1 and 3 compared to those (−0.029 and −0.021) in columns 2 and 5 in Table 3. This suggests that the effect of disasters is more pronounced for households that do not own their home. Homeownership might mitigate the effect of disaster experiences on households' portfolio choices, because having hazard insurance on the residential property provides some financial security to the homeowners. In columns 2 and 4, we use our entire sample but explicitly control for the market value of a residential property and its mortgage debt, relative to net wealth of the household. We define NET_WEALTH as the sum of risky assets, safe assets, and net value of residential property. In this alternative test, we continue to find strong effects of disaster experiences on portfolio choices. The estimated coefficients on housing variables are broadly consistent with the findings in the literature (e.g., Cocco (2005)). We also obtain almost identical and robust results (not reported) when we use home equity (which is defined as the market value of a residential property minus its outstanding mortgage debt, as in Chetty et al. (2017)) as a control for housing wealth. Based on these results, we conclude that there is a significant remaining effect that is captured by the disaster experience variable even after accounting for potential changes in housing values due to natural disasters.

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

Das, Kuhnen, and Nagel (2020) show that individuals with higher socioeconomic status (SES) are more optimistic about future macroeconomic developments, including business conditions, the national unemployment rate, and stock market returns. Motivated by this finding, 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. The details of the estimation procedures and results are presented and discussed in Supplementary Material Section J. These results indicate that socioeconomic status is an important conditioning variable explaining how households' portfolio choices react to their disaster experiences.

V. How Disaster Experiences Affect Risk Preferences and Expectations

In Section IV, we find evidence that income or wealth shocks may explain a relatively small part of the variation in households' risk-taking behavior after disaster events, and thus cannot subsume the impact of disaster experiences on portfolio choice. In this section, we investigate how past disaster experiences alter future expectations and preferences of households.

A. Relocation Tests: Movers Versus Stayers

To disentangle the risk preferences and expectations channels from each other, each of which has a direct influence on asset allocation through risk aversion and expectations (as predicted by theory), we exploit the confidential residence location data in the NLSY79 survey. We begin by classifying each county as either a high-disaster-prone (HD) or a low-disaster-prone (LD) county. All counties that experienced above the median number of natural disasters over the sample period of 1964 to 2013 are classified as HD. The remaining counties are classified as LD.[14] Since we are able to keep track of the entire history of households' residential locations, we compare the risk-taking behavior of stayers (48.9% of 101,300

[14]If we use disaster data only up to 2000, 80% of the counties have the same classification obtained from the entire sample. This result suggests that the high-low classification is highly persistent over time.

household year observations) and each of the following types of movers separately: i) households that relocated from a HD to a LD county during the sample period, denoted as $\text{MOVER}_{\text{HD}\mapsto\text{LD}}$ (8.5% of the sample); ii) households that relocated from a HD to another HD county, denoted as $\text{MOVER}_{\text{HD}\mapsto\text{HD}}$ (29.0% of the sample); iii) households that relocated from a LD to a HD county, denoted as $\text{MOVER}_{\text{LD}\mapsto\text{HD}}$ (5.5% of the sample); and iv) households that relocated from a LD to another LD county, denoted as $\text{MOVER}_{\text{LD}\mapsto\text{LD}}$ (8.1% of the sample).[15] In all specifications, we include households' disaster experiences since households' expectations depend not only on their current residence but also on their disaster experiences. We note that stayer households are included, but the household fixed effects absorb such households while estimating the effect of the move. In every specification, $\text{MOVER}_{\text{Before Move}}$ is the omitted variable: the coefficients on $\text{MOVER}_{\text{After Move}}$ denote incremental effects relative to the time before move, $\text{MOVER}_{\text{Before Move}}$ for that subgroup. We design the tests in this way to facilitate easy comparison across the different types of movers.

First, we note that persons moving from a HD to a LD area are expected to update their beliefs to anticipate fewer disasters. This would lead them to choose a more risky portfolio, if the expectations channel were the only channel at work. On the contrary, columns 1 and 5 in Panel A of Table 7 show that these movers become more conservative in their portfolio choices after their move. This result unambiguously establishes the role of risk preferences in portfolio choice, which were presumably affected by their disaster experiences, and the preferences channel overriding the expectations channel. To address the concern whether this result is simply due to the act of moving or is informative about the movers' preferences, we repeat the same analysis for households that relocated from a HD area to *another* HD: that is, they changed their residence, but the new location is also in a HD county. Thus nothing has changed in the calculus of these households, save the act of moving. Columns 2 and 4 with insignificant coefficients in the regressions, therefore, suggest that the act of moving per se has no effect on portfolio choice. This is remarkable even though these movers make up 29.0% of the sample. In contrast, using $\text{MOVER}_{\text{HD}\mapsto\text{LD}}$ who make up just 8.5% of the sample, we were able to detect strong economic effects. We interpret this finding as a permanent shock to preferences based on the past experience history.

In columns 3 and 7, we examine the movers from an LD area to an HD area. Such households would rationally expect more disasters and hence be expected to choose a more conservative portfolio. Preferences could also play a role, though in the same direction of expectations. Thus, although we cannot separate the role of these two channels in this type of move, the results of these specifications provide consistent evidence of a more conservative portfolio after this type of move: the coefficients in columns 3 and 7 are strongly statistically significant. We note that we

[15]We exclude households with multiple status changes (e.g., $\text{MOVER}_{\text{HD}\mapsto\text{LD}\mapsto\text{HD}}$) because these households cannot be exclusively classified. For example, $\text{MOVER}_{\text{HD}\mapsto\text{LD}\mapsto\text{HD}}$ would be classified as both $\text{MOVER}_{\text{HD}\mapsto\text{LD}}$ and $\text{MOVER}_{\text{LD}\mapsto\text{HD}}$ in our regression specifications. Such households make up only about 5% of our sample (5,938 household-year observations). We obtain qualitatively similar results by including those households with multiple status changes in our sample. We also exclude households whose current residence information cannot be matched with FIPS code recorded in FEMA data (538 household-year observations).

TABLE 7

How Disaster Experiences Affect Risk Preferences and Expectations: Relocation Tests

Panel A of Table 7 compares the risk-taking behavior of stayers and each of the following types of movers: i) households that relocated from a high-disaster-prone county (HD) to a low-disaster-prone county (LD) during the entire sample period, denoted as $\text{MOVER}_{\text{HD}\mapsto\text{LD}}$, ii) households that relocated from an HD to another HD, denoted as $\text{MOVER}_{\text{HD}\mapsto\text{HD}}$, iii) households that relocated from an LD to an HD, denoted as $\text{MOVER}_{\text{LD}\mapsto\text{HD}}$, and iv) households that relocated from an LD to another LD, denoted as $\text{MOVER}_{\text{LD}\mapsto\text{LD}}$. In all regression specifications, households with no moves are included. However, the household fixed effects absorb such households in the estimation. $\text{MOVER}_{\text{Before Move}}$ is the omitted group in all regressions. The coefficients on $\text{MOVER}_{\text{After Move}}$ denote incremental effects relative to the before move, $\text{MOVER}_{\text{Before Move}}$ for that subgroup. For example, −0.051 in column 1 indicates the effect of moving to an LD from an HD; that is, it compares $\text{MOVER}_{\text{After Move}}$, relative to before the move, $\text{MOVER}_{\text{Before Move}}$. Each county is categorized as either a high-disaster-prone county (HD) or a low-disaster-prone county (LD); a county is an HD if the total number of disasters (over the period of 1964 to 2013) that occurred in that county exceeds the median value of the distribution of the number of disasters, and LD otherwise. 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. Panel B of Table 7 repeats columns 1, 3, 5, and 7 in Panel A by further dividing the group, $\text{MOVER}_{\text{After Move}}$, into two subgroups: $\text{MOVER}_{\text{After Move}}$[ST] and $\text{MOVER}_{\text{After Move}}$[LT]. ST stands for short-term and LT stands for long-term. $\text{MOVER}_{\text{After Move}}$[ST] indicates that the time passed since the move is less than the median value of duration of stay (6 years in our sample). Similarly, we define $\text{MOVER}_{\text{After Move}}$[LT] if the time passed since the move is greater than the median. The coefficients on $\text{MOVER}_{\text{After Move}}$[STorLT] denote incremental effects relative to the before move, $\text{MOVER}_{\text{Before Move}}$. For example, −0.054 in column 1 of Panel B indicates the effect of moving to an LD after staying more than the median value of duration of stay, relative to before the move, $\text{MOVER}_{\text{Before Move}}$. 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 A. Movers Versus Stayers

	MOVER (Before↦After)			
	HD↦LD	HD↦HD	LD↦HD	LD↦LD
[PARTICIPATION]	1	2	3	4
$\text{MOVER}_{\text{Before Move}}$	(Omitted)	(Omitted)	(Omitted)	(Omitted)
$\text{MOVER}_{\text{After Move}}$	−.047***	−.008	−.057***	−.001
	(.015)	(.010)	(.020)	(.017)
ln(1 + CUMNUM_OF_DISASTERS)	−.031*	−.042***	−.020	−.026
	(.018)	(.013)	(.018)	(.018)
No. of obs.	58,132	78,872	55,139	57,720
Adj. R^2	0.639	0.637	0.645	0.645
[RISKY_SHARE]	5	6	7	8
$\text{MOVER}_{\text{Before Move}}$	(Omitted)	(Omitted)	(Omitted)	(Omitted)
$\text{MOVER}_{\text{After Move}}$	−.033***	−.007	−.035**	−.012
	(.013)	(.008)	(.017)	(.014)
ln(1 + CUMNUM_OF_DISASTERS)	−.021	−.026**	−.016	−.026*
	(.016)	(.011)	(.016)	(.014)
Income, liquid assets, household characteristics	Yes	Yes	Yes	Yes
Age and household FE	Yes	Yes	Yes	Yes
County-by-year FE	Yes	Yes	Yes	Yes
No. of obs.	43,454	59,143	41,225	42,939
Adj. R^2	0.652	0.653	0.658	0.655

Panel B. Really Long-Lived Impact?

	$\text{MOVER}_{\text{HD}\mapsto\text{LD}}$		$\text{MOVER}_{\text{LD}\mapsto\text{HD}}$	
	PARTICIPATION	RISKY_SHARE	PARTICIPATION	RISKY_SHARE
	1	2	3	4
$\text{MOVER}_{\text{Before Move}}$	(Omitted)	(Omitted)	(Omitted)	(Omitted)
$\text{MOVER}_{\text{After Move}}$[ST]	−.043***	−.031**	−.048**	−.033*
	(.015)	(.013)	(.019)	(.018)
$\text{MOVER}_{\text{After Move}}$[LT]	−.056**	−.039**	−.084***	−.040*
	(.022)	(.019)	(.026)	(.022)
ln(1 + CUMNUM_OF_DISASTERS)	−.032*	−.022	−.018	−.016
	(.018)	(.016)	(.018)	(.016)
Income and liquid assets	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Age and household FE	Yes	Yes	Yes	Yes
County-by-year FE	Yes	Yes	Yes	Yes
No. of obs.	58,132	43,454	55,139	41,225
Adj. R^2	0.639	0.652	0.645	0.658

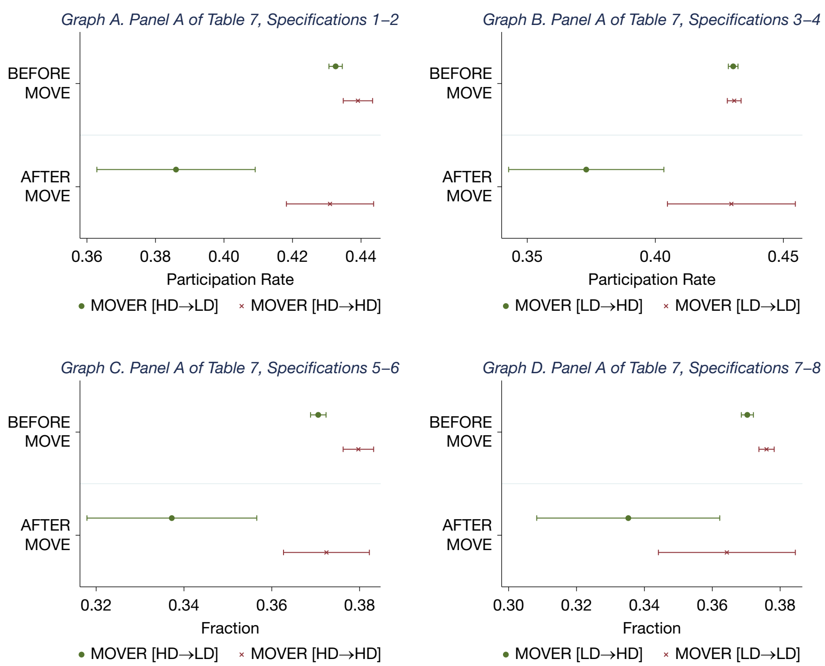
find significant coefficients even though only 5% of our sample observations make this type of move. We, therefore, conclude from these results that both expectations and preferences could affect household portfolio choice. Similar to the earlier tests (in columns 2 and 6), we examine the portfolio behavior of households that relocated from a LD area to another LD area in columns 4 and 8. Columns 4 and 8 with insignificant coefficients in the regressions indicate that the act of moving per se has no effect on portfolio choice.

Figure 4 presents the results of Table 7 pictorially. The average fitted risky asset market participation rates (using columns 1–4 in Panel A of Table 7) before and after the moves by the four types of movers are depicted in Graphs A and B of Figure 4. Graphs C and D show the average fitted risky asset share (using columns 5–8 in Panel A of Table 7). The figure presents the point estimates along with their 90% confidence intervals, derived from the regression specifications in Table 7. Graph A shows that those in $\mathrm{MOVER}_{\mathrm{HD}\mapsto\mathrm{LD}}$ become more conservative in their portfolio choice decisions after their moves. As an example, these households could

FIGURE 4

Risk-Taking Behavior of Movers: Relocation Tests

Graph A of Figure 4 shows the fitted risky asset market participation rates for households that relocated from a high-disaster-prone county (HD) to a low-disaster-prone county (LD) during the entire sample period, denoted as $\mathrm{MOVER}_{\mathrm{HD}\mapsto\mathrm{LD}}$, and those for households that relocated from an HD to another HD, denoted as $\mathrm{MOVER}_{\mathrm{HD}\mapsto\mathrm{HD}}$. The fitted values are based on columns 1–2 in Panel A of Table 7. All counties that experienced above the median number of natural disasters over the sample period are classified as HD. The remaining counties are classified as LD. Graph C presents the fitted risky asset share for the same households, $\mathrm{MOVER}_{\mathrm{HD}\mapsto\mathrm{LD}}$ and $\mathrm{MOVER}_{\mathrm{HD}\mapsto\mathrm{HD}}$. These fitted values are based on columns 5–6 in Panel A of Table 7. Graphs B and D are the counterparts of Graphs A and C for households that relocated from an LD to an HD, denoted as $\mathrm{MOVER}_{\mathrm{LD}\mapsto\mathrm{HD}}$, and households that relocated from an LD to another LD, denoted as $\mathrm{MOVER}_{\mathrm{LD}\mapsto\mathrm{LD}}$. These fitted values are based on columns 3–4 and 7–8 in Panel A of Table 7. Dots are the point estimates. Dashed lines indicate 90% confidence intervals. Observations are weighted by the NLSY79 sample weights.



have moved from Florida (a HD area due to the threat of hurricanes) to Arizona (a LD area in our sample). Our results suggest that natural disasters affect individuals' preferences and tastes for assets because these households may update their beliefs after moving to a LD area and think that they are less likely to be hit by natural shocks, a belief that leads to more risk-taking. However, they become more conservative in their asset allocation decisions even after controlling for other changes in their observables that predict their portfolio choices. We note that those in $\text{MOVER}_{\text{HD}\mapsto\text{HD}}$ do not hold such conservative portfolios after their move, which implies that the act of moving does not drive the results.

Finally, to examine whether the effect of a relocation to a LD (or HD) area is long-lived, we further divide $\text{MOVER}_{\text{After Move}}$ of two types of movers, $\text{MOVER}_{\text{HD}\mapsto\text{LD}}$ and $\text{MOVER}_{\text{LD}\mapsto\text{HD}}$ into two subgroups – $\text{MOVER}_{\text{After Move}}[\text{ST}]$ and $\text{MOVER}_{\text{After Move}}[\text{LT}]$ – based on the duration of stay at the new location. Here, ST stands for short-term and LT for long-term. $\text{MOVER}_{\text{After Move}}$ $[\text{ST}]$ indicates that the time passed since the move is less than the median value of the duration of stay (6 years in our sample). Similarly, we define $\text{MOVER}_{\text{After Move}}[\text{LT}]$ if the time passed since the move is greater than the median. The coefficients on $\text{MOVER}_{\text{After Move}}$ $[\text{ST or LT}]$ denote incremental effects relative to the before move, $\text{MOVER}_{\text{Before Move}}$. In Panel B of Table 7, we then show that the disaster experience effects are long-lived: the coefficients on $\text{MOVER}_{\text{After Move}}[\text{LT}]$, -0.056 and -0.039 in columns 1–2, remain strong even after a long time period, thus supporting the risk preferences channel. The results imply that our disaster effect shows up, on average, more than 6 years after the move and is visible for up to 24 years after the move. This result is consistent with Knüpfer et al. (2017), where adverse labor market experiences during the Finnish depression had a long-lived impact on households' risk-taking behavior.

These results, which establish the role of preferences and expectations in portfolio choice, motivate the following analyses in which we provide direct quantitative evidence on the relative importance of both channels. We do so by investigating changes in the risk-aversion measures of households and changes in their expectations about the stock market. Finally, we attempt to decompose the observed portfolio choice effects into risk preferences and expectations channels based on a simple portfolio choice model.

B. Risk Preferences Channel

To examine how disaster experiences change households' risk preferences, we run the following first-difference logit regressions:

$$(3)\quad \Pr\left(1_{\{\Delta(\text{RISK\_AVERSION}_{it})>0\}}|\Delta X_{it},\ \Delta\ln(1+\text{CUMNUM\_OF\_DISASTERS}_{it})|\}\right) = F(\beta\Delta\ln(1+\text{CUMNUM\_OF\_DISASTERS}_{it})+\gamma'\Delta X_{it}),$$

where Δ indicates the first-difference operator and $\boldsymbol{X}_{it}$ is a vector of controls including ln(1 + INCOME) and [ln(1 + INCOME)]^2. We obtain a sequence of three survey questions in the NLSY79 about an individual's job-related risk aversion and use the responses to construct a risk-aversion measure, which ranges from 1 (least risk averse) to 4 (most risk averse), using the method (Barsky, Juster, Kimball, and Shapiro (1997)) described in Supplementary Material Section L.1.

Taking first differences is crucial for our risk-aversion tests because unobservable and time-invariant risk appetite might influence both our risk-aversion measure and households' choice of residence, which determines their disaster experiences. By taking first differences, we cancel out the potential effect of this unobservable, time-invariant risk appetite. This approach also cleanly controls for a host of demographic variables and circumstances specific to the individuals that might affect their risk aversion. Effectively, we examine the effect of changes in disaster experiences on changes in risk aversion.

Panel A of Table 8 presents the results from the logit regressions, where the dependent variable is an indicator that is set to one if a household's risk-aversion measure increases, and zero otherwise. The results show that changes in disaster experiences are significantly positively related to changes in risk aversion at the 1% level after controlling for changes in income (both linear and quadratic terms). The effects are economically large. Moving from no change to 1-standard-deviation increase in the cumulative number of disasters increases the likelihood of being more risk averse by 20.5 percentage points (column 2). Since our risk-aversion measure has four distinct categories ranging from 1 (least risk averse) to 4 (most risk averse), changes in the risk-aversion measure have seven distinct values, from -3 (greatest decrease) to 3 (greatest increase). Hence, to account for the magnitude of the change in risk aversion, we run ordered logit regressions of the changes in the risk-aversion measure on the same set of covariates in equation (3) (Panel B of Table 8). The results are qualitatively unchanged. Moving from no change to 1-standard-deviation increase in the cumulative number of disasters increases the likelihood of being among the most risk-averse (4) from the least risk-averse (1) households by 4.9 percentage points (column 2). The results in these tables strongly suggest that an individual's risk aversion increases after experiencing a disaster, which feeds into future portfolio choice decisions.

C. Expectations Channel

Using Dutch survey data, Hurd, Van Rooij, and Winter (2011) find that individuals with more optimistic beliefs about future stock returns are more likely to participate in the stock market. We thus examine the effect of disaster experiences of individuals on their expectations about the future stock market return and volatility. We use the UBS/Gallup survey data. Details on the survey questions that we use for our analysis are discussed in Supplementary Material Section L.2. We run the following OLS regressions of expected stock market return over the next 12 months on households' disaster experiences and a set of control variables:

$$(4)\quad \mathbb{E}_{it}[\text{RETURN}] = \beta \ln(1 + \text{CUMNUM\_OF\_DISASTERS}_{it}) + \gamma' X_{it} + a_{it} + \tau_{it} + \varepsilon_{it},$$

where $\mathbb{E}_{it}[\text{RETURN}]$ indicates household i's expectations about the stock market return at time t and X_{it} includes income, indicators for completed high school and college education, race, and gender. a_{it} indicates age-fixed effects, and τ_{it} refers to year-month fixed effects. We note that since the set of respondents changes in every survey round, we are not able to include household fixed effects in this analysis. We use two types of disaster experience variables: $\text{DUM\_DISASTERS}_{it}$ is an indicator variable that is set to 1 if a household has experienced at least one disaster during the

TABLE 8

Risk-Aversion Measure and Disaster Experiences

Table 8 presents the effect of *changes* in disaster experiences on *changes* in risk-aversion measures. Risk-aversion measures, which range from 1 (least risk averse) to 4 (most risk averse), are obtained from the following sequence of three survey questions on the NLSY79 (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 a third, ii) in half, and iii) by 20%. Would you take the new job?*" Panel A shows first-difference logit regressions of the risk-aversion indicator variable on disaster experiences. The risk-aversion indicator variable is set to 1 if the household's job-related risk-aversion measure increases between surveys, and 0 otherwise. Panel B provides first-difference ordered logit regressions of the risk-aversion measure (which preserves the magnitude and sign of the risk-aversion change between surveys) on disaster experiences. Both panels use log of income and the square of log of income as controls. Observations are weighted by the NLSY79 sample weights. The average fitted values are calculated keeping all the other predictor variables at their sample mean. Numbers in parentheses are standard errors that are clustered by county. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Risk Aversion Difference Dummies (Logit)

	$1_{\{\Delta(\text{RISK\_AVERSION}_{it})>0\}}$	
	1	2
Δln(1 + CUMNUM_OF_DISASTERS)	.497***	.461***
	(.073)	(.078)
Δln(1 + INCOME)		−.523***
		(.141)
Δ([ln(1 + INCOME)]^2)		.031***
		(.007)
Avg. fitted prob. at		
ΔCUMNUM_OF_DISASTERS = 0	0.267	0.269
ΔCUMNUM_OF_DISASTERS = 1σ (=6)	0.489	0.474
Diff. between two fitted prob.	0.222***	0.205***
No. of obs.	20,392	18,505
Pseudo-R^2	0.003	0.005

Panel B. Risk Aversion Differences (Ordered Logit)

	$\Delta(\text{RISK\_AVERSION}_{it})$	
	1	2
Δln(1 + CUMNUM_OF_DISASTERS)	.344***	.319***
	(.066)	(.068)
Δln(1 + INCOME)		−.390***
		(.116)
Δ([ln(1 + INCOME)]^2)		.024***
		(.006)
ΔRISK_AVERSION = −3 (most decrease)		
Avg. fitted prob. at		
ΔCUMNUM_OF_DISASTERS = 0	0.059	0.056
ΔCUMNUM_OF_DISASTERS = 1σ (=6)	0.031	0.031
Diff. between two fitted prob.	−0.028***	−0.025***
ΔRISK_AVERSION = 3 (most increase)		
Avg. fitted prob. at		
ΔCUMNUM_OF_DISASTERS = 0	0.066	0.064
ΔCUMNUM_OF_DISASTERS = 1σ (=6)	0.121	0.113
Diff. between two fitted prob.	0.055***	0.049***
No. of obs.	20,392	18,505
Pseudo-R^2	0.0007	0.001

month before the interview date, and 0 otherwise, or a log of cumulative number of disasters (experienced over the month before the interview date). Based on our earlier results, we predict β to be negative.

Panel A of Table 9 presents the results. In all columns, disaster experiences are strongly negatively related to expected future stock market returns, even after controlling for age and time effects and demographic variables. The presence of disaster experiences decreases the respondents' estimate of the next year's expected

TABLE 9

Expected Stock Market Return and Volatility and Disaster Experiences

Table 9 presents the effect of disaster experiences on households' expectations about the stock market. Panel A shows OLS regression of expected stock market return over the next 12 months on households' disaster experiences. Expected stock market data are reported by individual respondents in the UBS/Gallup survey. Two types of disaster experiences are used: DUM_DISASTERS and ln(1 + CUMNUM_OF_DISASTERS), where DUM_DISASTERS is an indicator variable that is set to 1 if a household has experienced at least one disaster during the month before the interview date, and 0 otherwise; ln(1 + CUMNUM_OF_DISASTERS) is a log of cumulative number of disasters experienced over the month before the interview date. Panel B reports the results from the logit regressions of indicators denoting an increase in the expected stock market volatility on disaster experiences. The expected stock market volatility indicator is set to 1 if respondents expect an increase in volatility over the next 12 months, and 0 otherwise. Both panels use demographics and income of households as controls. Observations are weighted by the UBS/Gallup survey sample weights. Numbers in parentheses are standard errors that are clustered by state. ***, **, and * indicate significance at the 1%, 5%, and 10% levels, respectively.

Panel A. Expected Stock Market Return Over the Next 12 Months

	Expected Stock Market Return Over Next 12 Months			
	1	2	3	4
DUM_DISASTERS	−.006**	−.005**		
	(.002)	(.002)		
ln(1 + CUMNUM_OF_DISASTERS)			−.007**	−.006**
			(.003)	(.003)
ln(1 + INCOME)		−.021		−.021
		(.020)		(.021)
HIGH_SCHOOL		−.007		−.007
		(.005)		(.005)
COLLEGE		−.017***		−.017***
		(.002)		(.002)
HISPANIC		.019		.019
		(.017)		(.017)
BLACK		.048***		.048***
		(.006)		(.006)
FEMALE		.019***		.019***
		(.002)		(.002)
Age and year-month FE	Yes	Yes	Yes	Yes
No. of obs.	27,896	26,365	27,896	26,365
Sample period	2000–2002	2000–2002	2000–2002	2000–2002
Adj. R^2	0.072	0.095	0.072	0.095

Panel B. Expected Stock Market Volatility Over the Next 12 Months (Logit)

	Dummy Indicating Increase in Expected Volatility			
	1	2	3	4
DUM_DISASTERS	−.049	−.058		
	(.053)	(.053)		
ln(1 + CUMNUM_OF_DISASTERS)			−.034	−.046
			(.063)	(.064)
Income	Yes	Yes	Yes	Yes
Household characteristics	Yes	Yes	Yes	Yes
Age and year-month FE	Yes	Yes	Yes	Yes
No. of obs.	20,310	19,040	20,310	19,040
Sample period	1998–2000	1998–2000	1998–2000	1998–2000
Pseudo-R^2	0.018	0.021	0.018	0.021

stock market returns by approximately 50 basis points (column 2). Thus disaster experiences seem to affect expectations of households, which can be a viable channel affecting their portfolio choice decisions.

The UBS/Gallup survey also asks respondents about the expected stock market volatility over the next 12 months. Since the response to this question has three distinct categories, we run the following logit regression to examine whether disaster experiences affect households' expectations about the stock market volatility:

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$$(5)\quad \Pr\left(1_{\{\mathbb{E}_{it}[\text{VOLATILITY}]=\text{Increase}\}}|X_{it},\ \ln(1+\text{CUMNUM\_OF\_DISASTERS}_{it})|\}\right) = F(\beta\ln(1+\text{CUMNUM\_OF\_DISASTERS}_{it})+\gamma' X_{it}+a_{it}+\tau_{it}),$$

where $1_{\{\mathbb{E}_{it}[\text{VOLATILITY}]=\text{Increase}\}}$ is an indicator variable set to one if respondent i expects an increase in volatility over the next 12 months at time t, and zero otherwise; $\boldsymbol{X}_{it}$ is the same vector of control variables as in equation (4); and a_{it} and τ_{it} indicate age and year-month fixed effects, respectively. Panel B of Table 9 shows the results: in all specifications, disaster experiences are unrelated to households' expectations about future stock return volatility.

D. Decomposition

In Table 10, we attempt to decompose the relative importance of risk aversion and expectations on portfolio choice decisions in our sample when an individual is exposed to the risk of natural disasters that might affect both channels. We adopt the classic portfolio choice model in which an investor with constant relative risk-aversion (CRRA) preferences maximizes her expected utility by optimally

TABLE 10

How Disaster Experiences Affect Risk Preferences and Expectations: Quantitative Decomposition

Table 10 presents the relative contribution of expectations and risk preferences to changes in the risky asset share based on the estimates of various parameters from Tables 3 to 9. We adopt the classic portfolio choice model in which an investor with constant relative risk-aversion (CRRA) preferences maximizes her expected utility by optimally allocating her wealth to risky and risk-free assets over one period (Merton (1969), Samuelson (1969)). The model implies that the optimal fraction (α) of wealth invested in risky assets is proportional to the risk premium (RP) and inversely proportional to the product of the volatility (σ^2) and relative risk-aversion coefficient (γ): $\alpha = \text{RP}/(\gamma\sigma^2)$. We decompose the changes in α into three parts as follows:

$$\Delta\alpha \approx \frac{1}{\sigma^2}\left[\frac{\Delta(\text{RP})}{\gamma} + (\text{RP})\Delta\left(\frac{1}{\gamma}\right) + \Delta(\text{RP})\Delta\left(\frac{1}{\gamma}\right)\right].$$

We use the excess returns on market from Kenneth French's website to calculate risk premium and volatility. Scenario I uses all available return series until 2012, whereas Scenario II uses return data from 1988 to 2012. We assume that the expected volatility is not affected by disaster experiences and therefore is fixed based on the results from Panel B of Table 9). Adjusted percentage contributions indicate normalized contributions that neglect the second-order term, $\Delta(\text{RP})\Delta\left(\frac{1}{\gamma}\right)$.

Parameter/ Contribution	Scenario I [1926–2012]	Scenario II [1988–2012]	Note
RP	7.54%	6.97%	Average excess return on market (NYSE, AMEX, and NASDAQ)
σ	18.88%	15.13%	Standard deviation of market return
Δ(RP)	−0.12%	−0.12%	Estimated from column 4 in Panel A of Table 9
$\alpha_{\mu-\sigma}$	39.99%	39.99%	Estimated from column 4 in Table 3 (at $\mu-\sigma$ Of DE)
α_{μ}	37.43%	37.43%	Estimated from column 4 in Table 3 (at μ Of DE)
$\gamma_{\mu-\sigma}$	5.29	7.62	Model implied relative risk aversion coefficient (at $\mu-\sigma$ Of DE)
γ_{μ}	5.56	8.00	Model implied relative risk aversion coefficient (at μ Of DE)
$\Delta\gamma$	0.27	0.38	$\gamma_{\mu}-\gamma_{\mu-\sigma}$
$\Delta\ \alpha$ due to			
Δ(RP)	−0.64%	−0.69%	$\frac{\Delta(\text{RP})}{\gamma\sigma^2}$
$\Delta\gamma$	−1.95%	−1.90%	$\frac{\text{RP}}{\sigma^2}\Delta\left(\frac{1}{\gamma}\right)$
Δ(RP) and $\Delta\gamma$	0.03%	0.03%	$\frac{\Delta(\text{RP})}{\sigma^2}\Delta\left(\frac{1}{\gamma}\right)$
Contribution(%) to $\Delta\alpha$			
Δ(RP)	25%	27%	
$\Delta\gamma$	76%	74%	
Δ(RP) and $\Delta\gamma$	−1%	−1%	
Adjusted contribution (%) to $\Delta\alpha$			
Δ(RP)	25%	27%	
$\Delta\gamma$	75%	73%	

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allocating her wealth to risky and risk-free assets over one period (Merton (1969), Samuelson (1969)). The model implies that the optimal fraction (α) of wealth invested in risky assets is proportional to the risk premium (RP) and inversely proportional to the product of volatility (σ^2) and relative risk-aversion coefficient (γ): $\alpha = \text{RP}/(\sigma^2 \times \gamma)$. Keeping σ^2 constant (justified by the results in Panel B of Table 9, which shows that personal disaster experiences do not affect future expectations about stock market volatility), we decompose the changes in α into three parts as follows:

$$(6) \qquad \Delta\alpha \approx \frac{1}{\sigma^2}\left[\frac{\Delta(\text{RP})}{\gamma} + (\text{RP})\Delta\left(\frac{1}{\gamma}\right) + \Delta(\text{RP})\Delta\left(\frac{1}{\gamma}\right)\right].$$

We consider two different households with different disaster experiences: one household is at 1-standard-deviation below the average ($\mu - \sigma$), and the other is at the average of the disaster experiences distribution (μ). We consider two scenarios: Scenario I uses all available return series until 2012, whereas Scenario II uses return data from 1988 to 2012, the same period as in our sample, when calculating the parameter values of the risk premium and volatility as inputs to the portfolio choice decomposition.

$\Delta(\text{RP})$ is obtained from our expectations test for a 1-standard-deviation change in the disaster experience variable (column 4 in Panel A of Table 9). $\alpha_{\mu-\sigma}$ and α_μ are the fitted fractions of the risky asset share at the $\mu - \sigma$ and μ of the disaster experiences distribution, respectively (from the specification in column 4, Table 3). We can now calculate the model-implied relative risk-aversion coefficient for two different households in the disaster experiences distribution, $\gamma_{\mu-\sigma}$ and γ_μ. Consistent with our risk-aversion measure tests (Table 8), disaster experiences make households more risk averse: changes in model-implied relative risk-aversion coefficients are 0.27 and 0.38, for Scenario I and Scenario II, respectively. These changes are economically meaningful as they represent risk-aversion increases around 5%.

Our final calculation reveals that the contribution of the expectations channel in explaining portfolio choices is 25%, and the balance of 75% is explained by changes in risk aversion under Scenario I (under Adjusted Contribution(%) to $\Delta\alpha$). We adjust contributions by neglecting a higher-order term, a change due to both $\Delta(\text{RP})$ and $\Delta\gamma$, of which the contribution is about 1%. Similar results are obtained in Scenario II. We conclude that the expectations channel accounts for one quarter and the risk preferences channel accounts for three-quarters of the changes in portfolio choices that we observe in the sample due to changes in the natural disaster experiences of the households in our sample.

VI. Conclusion

We investigate whether households' experiences of natural disasters affect their portfolio choices. Using micro-panel data from the NLSY 1979 Cohort and disaster declarations data from the FEMA, we show that past disaster experiences have an economically significant effect of decreasing a household's future risky asset market participation and share of risky assets in their portfolio. Disaster experiences have predictive strength that is comparable to that of commonly analyzed variables such as income and liquid assets for portfolio choice. The analysis controls for age, household, county-by-year fixed effects, household demographics, and state-level macroeconomic variables. It is important to note that our findings based on the NLSY79 sample are applicable to the entire population of the United States. Therefore, our study potentially has important implications for explaining the limited stock market participation puzzle. We also note that our findings are not driven by a few households that experienced an extreme number of disasters or households that were exposed to a few catastrophic disasters because we exploit within-household variation in estimating these effects. Our results suggest that the cumulative disaster experiences that include both severe and super-severe disasters have a substantial influence on the risk-taking behavior of households. Super-severe disaster experiences seem to be economically more important than severe ones in a household's risky asset market participation, whereas both types of disasters seem to equally matter in determining the risky asset share of the household's portfolio. We find evidence that income or wealth shocks may explain only a small part of the variation in portfolio choice decisions after disaster events and cannot subsume the impact of disaster experiences on households' risk-taking behavior. Such experience effects persist even after an individual relocates to a new geographic area that is not vulnerable to disasters. This finding establishes a role of changes in preferences of households that are driven by life experiences. Indeed, we find that individuals become more risk averse and expect lower future returns (but do not expect changes in volatility of returns) after disaster experiences. A quantitative decomposition of the disaster effect using the Merton (1969) portfolio choice model shows that 25% of the effect is due to changes in expectations, and 75% of the effect is due to changes in risk aversion. Our results are consistent with the view that even transient but important personal experiences can affect an individual's preferences and tastes in a dynamically meaningful manner. These results call upon future heterogeneous agent life-cycle models of portfolio choice to incorporate shocks to deep parameters that can be altered by salient life experiences.

Supplementary Material

To view supplementary material for this article, please visit http://doi.org/10.1017/S0022109022000680.

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