Journal of Economic Behavior and Organization 231 (2025) 106931

Contents lists available at ScienceDirect

Journal of Economic Behavior and Organization

journal homepage: www.elsevier.com/locate/jebo

Research paper

From pandemics to portfolios: Long-term impacts of the 2009 H1N1 outbreak on household investment choices

Naijia Guo [a], Charles Ka Yui Leung [b], Shumeng Zhang [c],*

[a] Faculty of Business and Economics, the University of Hong Kong, Hong Kong Special Administrative Region
[b] Department of Economics and Finance, City University of Hong Kong, Hong Kong Special Administrative Region
[c] School of Economics, Nankai University, Tianjin, China

ARTICLE INFO

JEL classification:
D10
G11
I10

Keywords:
Pandemic
Portfolio choice
Risky share
Risk attitude

ABSTRACT

This study examines how experiencing a pandemic affects household investment behaviors. By leveraging cross-state variations in the H1N1 mortality rate in 2009, our difference-in-differences analysis reveals interesting findings. Although the pandemic does not significantly affect stock market participation, it depresses the proportion of liquid assets invested in risky assets among households who participate in the stock market. This effect persists for up to eight years after the pandemic and is particularly pronounced among households characterized by higher risk aversion and greater income volatility. Analysis conducted using different datasets consistently suggests that the pandemic primarily influences portfolio choices through a shift in risk attitudes.

1. Introduction

Over the last century, viruses have taken more lives than all armed conflicts (Adda, 2016).[1] Beyond their health impact, viral outbreaks also incur substantial economic damage.[2] Furthermore, viral public health crises expose people to great risks and prompt behavioral responses (Rasul, 2020). Specifically, pandemics can limit inter-person interactions (Ghent et al., 2024), change health behaviors (Agüero and Beleche, 2017), alter household consumption (Chetty et al., 2020) and savings (Hurwitz et al., 2021), affect time allocation decisions (Restrepo and Zeballos, 2020), shape people's expectations (Hanspal et al., 2021; Chen et al., 2023), and even have a long-term impact on economic status (Almond, 2006).

Households, as the ultimate owners of property, financial, and business assets, directly manage one-third of these assets in the US (Koijen and Yogo, 2019). While extensive research exists on pandemics, their impact on financial behavior, particularly household portfolio choices, is less understood. This study aims to fill this gap by examining how a pandemic influences these decisions.

A pandemic can affect household finance through several channels. First, pandemics can alter subjective perceptions, such as risk preference, subjective life expectancy, and degree of impatience, temporarily or permanently, thus affecting portfolio decisions. As noted by Shachat et al. (2021), the emergence of a public health crisis can unpredictably change individuals' economic preferences, which are crucial in economic decision-making. Second, a pandemic can increase background risks,[3] which subsequently alter

* Corresponding author.
E-mail addresses: njguo@hku.hk (N. Guo), kycleung@cityu.edu.hk (C.K.Y. Leung), smzhang@nankai.edu.cn (S. Zhang).

[1] For instance, the 1918 Spanish flu, the most severe influenza pandemic in recent history, resulted in about 50 million deaths (Taubenberger and Morens, 2006). The 2019 COVID-19 outbreak, as of August 2024, has infected 775 million people and caused over 7 million deaths (Source: the World Health Organization website, https://covid19.who.int/, accessed August 5, 2024).

[2] The annual global cost of moderately severe to severe pandemics accounts for 0.7% of global income (Source: World Report in 2017, accessed August 17, 2024).

[3] The background risk is referred to as the exogenous risks that are not under the agent's control (Eeckhoudt et al., 1996).

https://doi.org/10.1016/j.jebo.2025.106931
Received 17 August 2024; Received in revised form 4 February 2025; Accepted 5 February 2025
Available online 19 February 2025

portfolio decisions (Guiso et al., 1996; Heaton and Lucas, 2000). This can manifest through changes in household finance due to shocks in health status (Almond and Mazumder, 2005; Fan and Zhao, 2009), wealth (Pool et al., 2019), and labor market outcomes, such as employment, occupational choice, and earnings (Albanesi and Kim, 2021; Larrimore et al., 2022).

This study delves into the immediate and enduring effects of the 2009 H1N1 pandemic on US household portfolio choices and examines the risk preference and the background risk channels. The H1N1 pandemic, occurring from April 2009 to August 2010, was caused by a novel human-to-human transmissible H1N1 virus, also known as the "swine flu".[4] The swine flu garnered significant public attention in the US, which experienced the highest number of H1N1 cases and fatalities globally during the pandemic.[5] The unforeseen H1N1 pandemic provides a natural experiment on how households adjust their portfolios in response to an exogenous shock.[6]

We utilize data from four sources. First, we manually collect state-level H1N1 death data during the pandemic from an online forum *FluTracker*, calculating the death rate for each state as a measure of H1N1 intensity. Second, we gather data from nine waves of the Panel Study of Income Dynamics (PSID) (2001–2017, in odd-numbered years). We construct two portfolio choice measures: stock market participation and risky share (the share of risky assets in liquid assets, conditional on stock market participation). Third, we collect state-level macroeconomic indicators and medical resources to account for potential confounding factors related to H1N1 intensity. Last, we draw data from four waves (2009, 2012, 2015, and 2018) of the National Financial Capability Study State-by-state Survey (NFCS), which elicits respondents' risk attitudes, to explore the risk attitude mechanism.

We use a difference-in-differences (DID) approach to study the impact of the H1N1 pandemic on household portfolio choices. This method utilizes state-level variations in H1N1 intensity and differences between pre- and post-pandemic periods *within households*. The panel data enables us to account for time-invariant unobserved heterogeneity at the household level and examine the long-term effect of the pandemic.

We find that the 2009 pandemic does not influence stock market participation (the extensive margin of stock holdings). However, the risky share (the intensive margin) decreases with the H1N1 intensity in the post-pandemic period. Specifically, a 1 percent increase in the 2009 H1N1 death rate leads to an average decrease in the risky share by 0.3 percent. Additionally, our decomposition analysis suggests that this decline in the risky share is primarily due to a decrease in net investment in stocks, rather than an increase in liquid assets.

Our findings are robust to several concerns. To address the possibility of omitted variable bias, we include interactions between year dummies and a set of pre-pandemic state-level covariates that may correlate with H1N1 intensity, stock holdings, or both. We also address the potential confounding effect of the 2007–2008 financial crisis by controlling for economic indicators that reflect the impact of the financial crisis. In addition, our results are robust to different sample selection criteria, alternative measures of the H1N1 intensity, and discretizing the H1N1 death rate. Furthermore, we perform placebo tests by examining the effect of exposure to seasonal influenza on risky shares and using the hypothetical pandemic year.

An event study framework reveals the *time-varying* impact of the H1N1 pandemic on portfolio choices. In line with the parallel trend assumption, the difference in risky share is small and statistically insignificant prior to 2009. The initial exposure impact in 2009 is negligible. However, from 2011 onwards, the exposure effect on the risky share becomes significantly negative and remains stable, suggesting a delayed but prolonged impact of the H1N1 pandemic.

To explain these findings, we use NFCS data to explore the risk preference mechanism. We find that an increase in H1N1 intensity significantly decreases an individual's risk tolerance during the *post-pandemic* period. This suggests that living in a state with a higher H1N1 death rate amplifies risk aversion, which results in more conservative financial behavior, such as reducing exposure to risky assets in favor of safe alternatives. Moreover, the effect is particularly pronounced for women and unmarried individuals. Additionally, analysis using PSID data indicates that alternative factors such as health, demographics, labor market outcomes, and family wealth, which might affect household portfolio choices through the background-risk channel, are unlikely to account for our results.

Our heterogeneity analysis shows that the impact of H1N1 is stronger for households headed by women or single individuals. This supports the risk attitude channel, showing that households with higher risk aversion and greater income volatility are more susceptible to the pandemic's adverse effects. The impact is also stronger for those with heads experiencing unstable income, including those not working for a government and those not represented by a union. Using a risk tolerance measure from the 1996 PSID, we find that risk-averse individuals reduce their risky share more than risk-loving individuals in response to the H1N1 pandemic. Additionally, the exposure effect follows a hump-shaped pattern over the life cycle, with young individuals (who have lower accumulated wealth) and older workers (nearing retirement with declining labor income) reducing their risky asset holdings most significantly.

[4] The name "swine flu" comes from the fact that its gene components are similar to viruses known to infect pigs, even though it cannot be transmitted through the consumption of pork products. In the epidemiological literature, the 2009 H1N1 influenza virus is formally referred to as the "(H1N1)pdm09 virus" or the "novel H1N1 virus".

[5] Data from 2009 Google searches originated in the US reveals that the term "Swine influenza" even exceeded the popularity of "Barack Obama", the first African-American US president who took office in 2009, and "unemployment", which had hit a peak rate of 10% in 2009 for the first time since 1983 (Appendix Figure A1). See Section 2.1 for an overview of the H1N1 pandemic.

[6] Compared to COVID-19, studying the impact of the H1N1 pandemic on portfolio choices offers several advantages. Firstly, the H1N1 pandemic occurred over a decade ago, providing a longer time frame to assess its long-term effects. Secondly, the government response to COVID-19, including quarantines, business closures, cash distributions, and the widespread adoption of remote work, introduces additional variables that can confound the analysis of portfolio decisions. In contrast, the simpler response to the H1N1 pandemic allows for a clearer and more straightforward examination of its impact on household financial behavior.

This study makes three key contributions to the literature. Firstly, it enriches the body of work examining the socioeconomic consequences of pandemics, particularly their impact on household portfolio choices. While existing studies predominantly focus on the *short-term* correlation between the COVID-19 pandemic and the *intensive margin* of asset holding,[7] this study shifts the focus to the 2009 H1N1 pandemic. Using long panel data from 2001 to 2017, we identify both the immediate and *long-term* effects of the H1N1 pandemic on stock market participation and risky shares.[8] In addition, our DID framework provides more rigorous causal evidence of the pandemic impact.

Secondly, this study contributes to the literature on the determinants of household portfolio choices. Previous factors identified include cognitive ability (Christelis et al., 2010; Agarwal and Mazumder, 2013; Breunig et al., 2021), financial literacy (Gaudecker, 2015), housing (Cocco, 2005; Chetty et al., 2017), income risk (Angerer and Lam, 2009; Betermier et al., 2012; Catherine et al., 2024), social interaction (Hong et al., 2004; Liang and Guo, 2015), physical health (Fan and Zhao, 2009), mental health (Bogan and Fertig, 2013), and emotional status (Kuhnen and Knutson, 2011). We add to this literature by highlighting the role of pandemics in household asset allocations and emphasizing the shift in risk preference as a potential mechanism.

Thirdly, this study contributes to the broader literature on how aggregate shocks influence preferences and beliefs. There is substantial evidence that aggregate shocks can shift risk preferences and alter risk-taking behavior.[9] For instance, Malmendier and Nagel (2011) find that economic fluctuations reduce stock market participation and risky asset allocations, while Cameron and Shah (2015) show that non-economic shocks, such as earthquakes or floods, decrease risk-taking behavior. As comprehensively reviewed by Giuliano and Spilimbergo (2024), the existing literature on pandemics primarily focuses on trust and political preferences. Limited research studies how the pandemic, a key type of non-economic shock, affects preferences. This study fills the gap by providing novel evidence that pandemics can reshape risk preferences and influence investment decisions.

This paper is organized as follows. Section 2 introduces the background of the 2009 H1N1 pandemic and describes the data. Section 3 discusses our DID empirical strategy and Section 4 reports the findings on how exposure to the H1N1 pandemic affects household stock holdings. Section 5 discusses the possible channels through which the H1N1 pandemic affects household portfolio choices, followed by the heterogeneity analysis in Section 6. The last section concludes.

2. Background and data

2.1. The US and the 2009 H1N1 influenza pandemic

The 2009 outbreak of the novel A (H1N1) influenza (informally called "swine flu") was declared by WHO as the first global pandemic since the 1968 flu pandemic. During the pandemic, more than 214 countries and overseas territories or communities reported cases of 2009 H1N1 infection, which accounts for 11 to 21 percent of the world population at the time (Kelly et al., 2011). In the US, the novel H1N1 virus was first detected in California on April 15, 2009, and spread quickly across the US. From April 12, 2009, to April 10, 2010, the CDC estimated 43.3–89.3 million cases of the novel H1N1 virus, including 195,086–402,719 hospitalizations and 8868–18,306 deaths in the US (Shrestha et al., 2011). Fig. 1 illustrates the weekly trends of lab-confirmed cases and deaths associated with the 2009 H1N1 influenza in the United States. The data reveal that H1N1 cases peaked in June and October, while related deaths followed slightly later, reaching their highest levels in August and November of 2009.

The (H1N1)pdm09 virus is typically transmitted from person to person through droplets. It shares symptoms similar to other flu: fever, cough, headache, etc. However, the disease patterns in severe cases are remarkably different from seasonal flu. The health of infected people can deteriorate within three to five days after they have symptoms and progress to respiratory failure. This virus differs significantly from other known H1N1 viruses during the pandemic because it contains a unique segment of flu genes not identified by humans previously. As a result, children and young and middle-aged adults have almost no existing antibodies against it, while almost a third of people over 60 years old had immunity probably due to their exposure to an older H1N1 at some time in their earlier lives. It is estimated that 80% of 2009 H1N1 deaths were people less than 65 years old. By contrast, about 80%–90% of typical seasonal influenza were those aged 65 or older (Dawood et al., 2012). Given the uniqueness of the novel H1N1 virus, seasonal flu vaccines offered little protection against it.

The US government took a series of measures in response to this pandemic. In the initial stage, schools were closed if cases were confirmed.[10] In addition, H1N1-related news was published on cdc.gov and the official websites of state health departments every week. Public health advice was also provided on these websites, including washing hands properly and using hand sanitizers, encouraging the public to keep social distancing, and encouraging sick people with mild symptoms to stay home from work or school until their symptoms subsided. On the other hand, CDC started working on the 2009 H1N1 flu vaccine in April. However, given the slow production process, the vaccine was unavailable until October 2009.

In early August 2010, scientists found that the pandemic flu activity had returned to normal levels that were considered typical for seasonal flu, which provided strong evidence that the (H1N1)pdm09 virus was transitioning to a seasonal influenza virus. As a result, the World Health Organization (WHO) declared an end to the H1N1 pandemic on August 11, 2010. From then on, the novel H1N1 virus spreads as a seasonal flu virus.

[7] For instance, Coibion et al. (2025) show that COVID-19 exposure is negatively associated with household investments.

[8] This is in sharp contrast to the earlier studies which focus on how pandemic may affect people's perception in the short run, such as Ibuka et al. (2010).

[9] The direction of these effects is debated. See Chuang and Schechter (2015) and Schildberg-Hörisch (2018) for detailed reviews on how economic shocks, natural disasters, and conflicts affect risk preferences.

[10] By May 5, 2009, 980 schools across the countries were closed, involving 607,778 students.

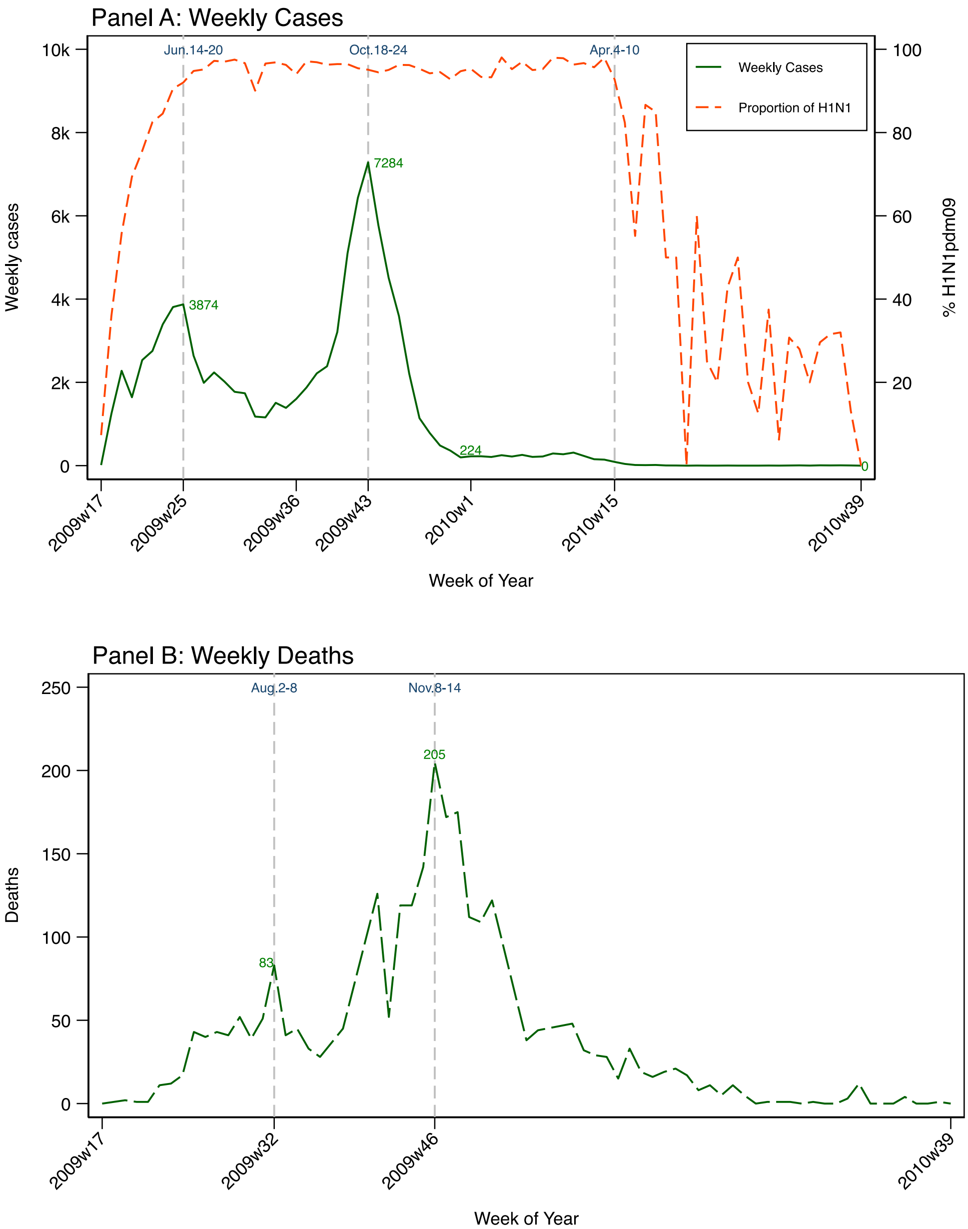

Fig. 1. Weekly cases and cumulative deaths of H1N1pdm09 in the US.
Notes: In Panel A, the solid line represents the number of weekly lab-confirmed H1N1 cases, while the dashed line denotes the proportion of weekly lab-confirmed H1N1 cases in total lab-confirmed influenza cases. In Panel B, the long-dashed line denotes the number of weekly lab-confirmed H1N1 deaths.
Data source: WHO Collaborating Laboratories.

2.2. Data

2.2.1. H1N1 data

We first describe the data on the severity of the H1N1 pandemic. We use data collected from *FluTrackers* (FluTrackers.com), an online forum and early warning system that has gathered information on infectious diseases since 2006.[11] *FluTrackers* collected in real time the state-level H1N1 surveillance information from each state health department and posted the content as well as the

[11] The CDC publishes influenza surveillance information in a weekly report called *FluView*. When the novel H1N1 virus emerged, *FluView* reported state-level individual case counts. However, the CDC discontinued reporting confirmed cases on July 24, 2009, and only reported the country-level hospitalizations and deaths due to the 2009 H1N1 virus. Unfortunately, we cannot obtain useful information from the CDC reports since the country-level figures mask the regional variations in the H1N1 outbreak.

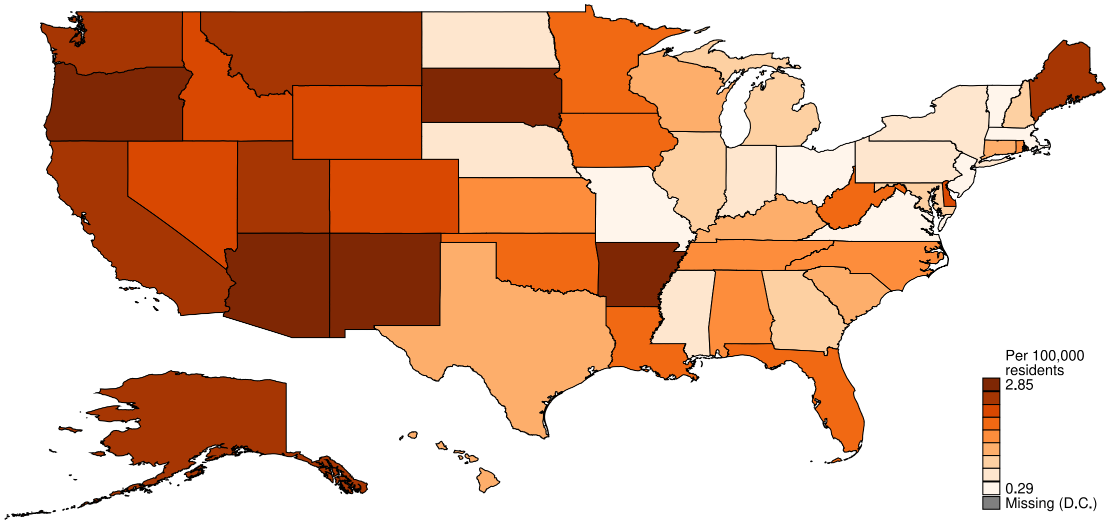

Fig. 2. Geographic distribution of H1N1 death rates in the 2009 pandemic.
Notes: The state-level H1N1 death rate is defined as the state-level lab-confirmed death toll per 100,000 residents during the 2009 Pandemic (April 2009–August 2010). The state-level death toll is aggregated from FluTrackers.com. The number of state residents is the intercensal estimate for 2009, drawn from the Population Division of the US Census Bureau. See Appendix Table B1 for specific death rates of each state. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

source link. We extract the lab-confirmed H1N1 death information of each state from *FluTrackers* during the pandemic (i.e., April 2009–August 2010). Our main measurement for the state-level H1N1 intensity is the number of lab-confirmed death tolls per 100,000 residents. We use death rates rather than death numbers because death numbers can be positively correlated with the local population. To our knowledge, this state-level data is the most granular geographical unit that can be publicly accessible. Fig. 2 shows the geographic distribution of H1N1 death rates for 50 states (excluding the District of Columbia), where a darker color implies a higher death rate, ranging from 0.29 (Missouri) to 2.85 (South Dakota).

Given the impact of H1N1 on casualties, it is natural to inquire whether the stock market mirrors the prevalence of the pandemic and drives the adjustments in household portfolios. We conduct several analyses and reserve the details in Figures A2–A4 and Table B2 in Appendix. Overall, the H1N1 pandemic has a negligible impact on the stock market, which is in distinct contrast with the case of the COVID-19 pandemic.[12]

In addition to the state-level H1N1 death rate, we develop a measure of the H1N1 case rate. However, the state-level number of H1N1 cases is unavailable during the pandemic. To address this limitation, we compile data on H1N1 cases from a broader geographic level. Specifically, approximately 80 WHO Collaborating Laboratories across the United States report weekly data on the total number of respiratory specimens tested and the number testing positive for influenza types A and B, including (H1N1)pdm09. These data are aggregated at the Health and Human Services (HHS) region level, which encompasses multiple states.[13] We utilized an R package named "cdcfluview" to gather this weekly HHS-level data on H1N1 case numbers. We aggregate the number of confirmed H1N1 cases for *each HHS region* during the pandemic to calculate the H1N1 case rate, i.e., the number of H1N1 case counts per 100,000 residents.

2.2.2. PSID

Our primary data source for household information is the PSID. It is a longitudinal and nationally representative survey that tracks over 82,000 individuals from more than 9500 families for over 50 years. The PSID data were released annually until 1997 and became a biannual survey. At the time of our research, due to the lack of some state-by-year-level control variables in 1999 and 2019, we used nine waves of data in the PSID (i.e., 2001, 2003, 2005, 2007, 2009, 2011, 2013, 2015, and 2017).[14]

Variable Definitions. — Wealth data are collected at the household level and refer to the value at the time of the survey. Following Brunnermeier and Nagel (2008), *risky assets* is defined as the sum of stock in publicly held corporations, stock mutual funds, and investment trusts, including stocks in the Individual Retirement Account (IRA). *Risky-free assets* are the sum of checkings, savings, bonds, trusts, and IRAs invested in interest earnings. We further denote the sum of risky and riskless assets as liquid assets. *Stock*

[12] he COVID-19 pandemic and the measures taken to control it caused the S&P 500 to experience a historic decline of one-third of its value in February and March 2020 (Hanspal et al., 2021) and have a substantially adverse impact on stock returns (Al-Awadhi et al., 2020).

[13] The US is divided into 10 HHS regions. For more details about HHS regions, see the website of the US Department of Health and Human Services.

[14] During the course of our research, most state-by-year control variables were missing in or before 1999, and some were missing in 2019. As we will introduce in Section 4.3, controlling these variables in causal analysis is essential since they are closely related to the outcome variables and the H1N1 intensity. Therefore, we use PSID data from 2001 to 2017 for a balanced panel of state-by-year characteristics.

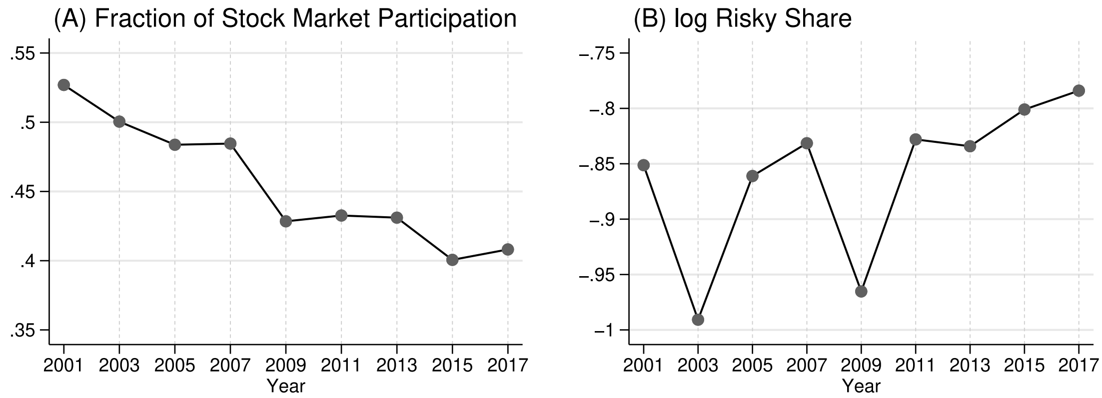

Fig. 3. Aggregate changes in household portfolio: 2001–2017.
Notes: Stock market participation is defined as owning positive risky assets. Risky share is the value of risky assets divided by liquid assets, conditional on stock market participation.
Data source: PSID, waves 2001–2017, in odd-numbered years.

market participation equals one if the household has positive risky assets and zero otherwise. The *risky share* is the proportion of the liquid assets held in risky assets, conditional on stock market participation.

Fig. 3 plots the average changes in the household portfolios over time. Since 2001, the fraction of households owning risky assets displays an overall declining trend (Panel A).[15] In particular, it dropped substantially during the financial crisis period (2007–2009). On the other hand, the risky share displayed high volatility between 2001 and 2017 (Panel B).

We proxy the household head (i.e., the "reference person" defined by PSID since 2017) as the main decision-maker in a family. Therefore, the head's characteristics (e.g., age, gender, education, race, marital status) are used as a priority. All monetary values, such as wealth and income, are defined in the 2017 dollar adjusted by the CPI-U index.

Sample restrictions. — Appendix Figure A5 provides a step-by-step illustration of the sample size and the selection criteria applied for both the stock market participation and the risky asset share. Our original sample is restricted to those who have non-missing basic individual and household characteristics (introduced in detail in Section 4.1). We then exclude households with asset changes due to a family member moving into or out of the family. We also drop observations if the household head is either a student or retired.[16] Households with liquid assets less than $1000 are also excluded because the asset allocation decision is irrelevant for households with little wealth (Gomes and Smirnova, 2021). To facilitate panel-data analysis, we further remove households with only one observation. After applying these criteria, the final sample for stock market participation includes 25,947 observations from 5049 family units. For the analysis of risky asset allocation, we focus on households owning risky assets, resulting in a sample of 9790 observations from 2239 family units.

Summary Statistics. — Table 1 presents pooled cross-sectional statistics for all households in our analysis sample. During our sample period, the average age of household head is 43. The head's gender is skewed to males (83%). More than 70% of the heads are white. The average number of years of education is 14.5 years. The fraction of households participating in the stock market is 0.41, and the (conditional) risky share is 0.53.

2.2.3. Macroeconomic indicators and medical resources

We collect a comprehensive set of state-level variables in 2008 on macroeconomic indicators and medical resources from different sources (see Appendix Table B4 for the data sources). To compare various states fairly, we use the per capita (or per 100,000 population) basis for GDP, personal income, bankruptcy cases, assets, deposits, hospital beds, active physicians, physicians in patient care, and registered nurses.

Appendix Table B5 reports balanced test results for these state characteristics in 2008, the year preceeding the H1N1 pandemic. A state is classified as high death-rate (HDR) if its H1N1 death rate during the pandemic exceeded the median level observed across all 50 states. As shown in the table, H1N1 intensity correlates with state macroeconomic indicators, such as housing price, asset per capita, population density, and medical resources, including physicians and nurses. To account for potential confounding factors that might influence both a state's response to the H1N1 pandemic and its H1N1 intensity, our robustness checks include interactions between all observed pre-pandemic macroeconomic and medical covariates and year indicators.[17]

[15] This downward trend aligns with the pattern presented with the Survey of Consumer Finances data in the same period, as described by Gomes and Smirnova (2021). Gomes and Smirnova (2021) notes that the stock market participation rate increased during the "dotcom bubble" in the late 1990s, and then gradually reverted to its prior level in the early 2000s.

[16] Household investment decisions usually change during the transition into retirement (Addoum, 2017; Fagereng et al., 2017). As a result, we exclude retired households following Brunnermeier and Nagel (2008).

[17] Since the results of the balanced test could be influenced by states near the "cut-off" between the low and high death-rate groups, we perform a robustness check by dropping states with mid-level death rates (Appendix Table B6). Another potential issue arises from states with extreme H1N1 death rates, which could skew the balance test. So we execute another test by excluding states with the lowest and highest death rates (Appendix Table B7). The findings from both robustness checks align with those presented in Table B5.

Table 1
Summary statistic for the PSID households.
Data source: PSID, waves 2001–2017, in odd-numbered years.

Variable	Mean	Median	Std.
Age	43.35	43.00	11.46
Male	0.83	1.00	0.37
White	0.74	1.00	0.44
Years of schooling	14.52	15.00	2.13
Stock market participation	0.41	0.00	0.49
Risky share	0.53	0.50	0.29
Number of family	5049		
Observations	25,947		

Notes: The table reports summary statistics for all households that satisfy our sample selection rule. Head information is used in priority as individual characteristics, and if head information is missing, spouse information is used instead. *Stock market participation* equals unity if the household has positive risky assets and zero otherwise. *Risky share* is the proportion of the liquid assets held in risky assets, conditional on stock market participation.

2.2.4. Risk attitude

In Section 5.1, we investigate whether changes in risk attitude can explain why the H1N1 pandemic affects stock holdings. Since PSID only surveyed risk attitude in the 1996 wave, we employ the NFCS data, which contains risk attitude data across waves. Initiated in 2009 and conducted triennially, the NFCS surveys a sample of over 25,000 adults in each wave, with approximately 500 respondents per state.

We pool four waves of NFCS (2009, 2012, 2015, and 2018) and obtain a sample of over 93,000 observations. The weighted summary statistics of this NFCS sample are presented in Appendix Table B8. The survey question on risk tolerance in the NFCS inquires: “When thinking of your financial investments, how willing are you to take risks? Please use a 10-point scale, where 1 means ‘Not At All Willing’ and 10 means ‘Very Willing’.” A lower score corresponds to greater risk aversion. As shown in Table B8, the average risk score of the NFCS sample is 4.77.

3. Empirical strategy

Our primary goal is to estimate the causal impact of the 2009 H1N1 pandemic on the extensive and intensive margins of stock holdings. As introduced in Section 2.2.1, we use state-level H1N1 death rates during the pandemic (i.e., April 2009–August 2010) to measure H1N1 intensity. We adopt a DID identification strategy and exploit the state-level differences in the H1N1 intensity and the differences between the pre-pandemic and post-pandemic periods. Specifically, the following equation is estimated:

$$Y_{ijt} = \beta_1 During_t \times H1N1_s + \beta_2 After_t \times H1N1_s + \alpha_t + \alpha_i + \alpha_j + \delta t\alpha_j + \gamma \boldsymbol{X}_{it} + \epsilon_{ijt}, \tag{1}$$

where i indexes family, t indexes year, and j indexes the state where the family resides. The outcome of interest, Y_{ijt}, denotes either an indicator variable for stock market participation or the natural log of risky share. $H1N1_s$ is the natural log of the lab-confirmed H1N1 death rate in the state s during the 2009 H1N1 pandemic. The state s is where the household resided in 2009.[18] $During_t$ is an indicator equal to 1 if $t = 2009$. $After_t$ is an indicator equal to 1 if $t > 2009$. α_t, α_i, and α_j are year, family, and state fixed effects, respectively. Note that we can still identify state fixed effects (α_j) even when we include family fixed effects (α_i) since there are families who move across states.[19] The model also includes state-specific time trends, $t\alpha_j$, to mitigate the concern that the effect of the pandemic can be driven by preexisting state-specific trends. Including these trends in a DID framework is a more conservative specification, as it relaxes the standard assumption that all unit-specific trends are zero (Downey, 2024).[20] $\boldsymbol{X}_{it}$ includes household characteristics, which are described in Section 4.1. Controlling these variables helps reduce the variance of the error term and hence improves the precision of the estimate of β_1 and β_2. Standard errors are clustered at the state level, aligning with the level at which the treatment is defined (Abadie et al., 2023).

We interact $H1N1_s$ with $During_t$ and $After_t$ dummy separately rather than construct the classic interaction between $H1N1_s$ and a $Post_t$ dummy ($Post_t$ is equal to 1 if $t \geq 2009$). This is because more than 60% of the families in 2009 took the PSID survey before June (i.e., the peak of the first wave in the US), as shown in Appendix Figure A6. Therefore, to be conservative, we define $During_t$ and $After_t$ dummies following Kolstad and Kowalski (2012).

Our DID setting aligns with the case without untreated units, as all states in our sample were exposed to the H1N1 pandemic and reported H1N1-related deaths. Therefore, the continuous variation in the H1N1 intensity can be used to recover the average causal response (ACR) (Callaway et al., 2024), i.e., the average treatment effect with respect to an increase in the H1N1 death rate.[21]

18 Our baseline analysis does not require the sample to stay in the same state. Later, we will focus on families that did not move in the robustness analysis.

19 Given that we include α_i, which controls time-invariant family characteristics, α_j captures the effect of time-invariant state-level characteristics for movers (i.e., families that changed their residency state during our study period.).

20 It is crucial to include state-specific time trends in our regressions. Omitting these trends could violate the parallel trends assumption, particularly for the intensive margin of stock investment.

21 ACR, as defined by Callaway et al. (2024), is the derivative of the average treatment effect with respect to the treatment does, expressed as $ACR(d) = \frac{\partial ATE(d)}{\partial d}$.

With a continuous treatment variable in our DID framework, the traditional parallel trends assumption, which consider untreated potential outcomes, is not strong enough to identify the average treatment effect.[22] This limitation arises because selection bias may occur if different states experience different treatment effects of the same H1N1 intensity. As emphasized by Callaway et al. (2024), identifying ACR with a continuous treatment requires a stronger parallel trends assumption, which involves potential outcomes under different treatment intensities. The stronger assumption stipulates that changes in household stock holdings for states with lower death rates should provide a good counterfactual for the changes in household stock holdings that would have been observed for states with higher death rates.[23] In other words, the stronger assumption assumes there is no heterogeneous treatment effect. As a robustness check, we discretize the H1N1 death rate to generate a discrete treatment variable, which allows identification under the standard parallel trends assumption. We then assess whether the exposure effect remains significant (see Appendix C for details).

Downey (2024) highlights that the introduction of unit-specific trends in Eq. (1) can lead to inconsistency in the pooled post-treatment coefficient, $\hat{\beta}_2$. This bias arises from two main issues. First, short-term treatment effects are given disproportionately larger weights compared to long-term effects. Second, some weights may be negative. In the worst-case scenario, these issues combined could result in a pooled post-treatment effect with a sign opposite to that of all individual dynamic treatment effects. To address this problem, Downey recommends estimating Eq. (2) and calculating a consistent estimate of the average treatment effect, $\bar{\beta}_{post}$, which is our preferred estimate. This estimate is derived as the average of the dynamic treatment effects (i.e., separate effects for each post-treatment time period), $\hat{\beta}_{2011}$, $\hat{\beta}_{2013}$, $\hat{\beta}_{2015}$, and $\hat{\beta}_{2017}$. In our analysis, we will report both $\hat{\beta}_2$ and $\bar{\beta}_{post}$ for comparison.

$$\begin{aligned} Y_{ijt} &= \beta_1 During_t \times H1N1_s + \beta_{2011} H1N1_s + \beta_{2013} H1N1_s + \beta_{2015} H1N1_s + \beta_{2017} H1N1_s \\ &\quad + \alpha_t + \alpha_i + \alpha_j + \delta t\alpha_j + \gamma \boldsymbol{X_i} + \epsilon_{ijt}, \end{aligned} \tag{2}$$

We next use an event study framework to estimate year-specific treatment effects relative to the base year 2007. Formally, the model is written as:

$$Y_{ijt} = \beta_t H1N1_s + \alpha_t + \alpha_i + \alpha_j + \delta t\alpha_j + \gamma \boldsymbol{X_{it}} + \epsilon_{ijt}. \tag{3}$$

The estimates β_ts measure the dynamic treatment effects over time and help to examine whether pre-trends exist.[24]

In our heterogeneity analysis, we modify Eq. (1) to allow for varying treatment effects across different groups. The modified model is expressed as:

$$Y_{ijt} = \beta_{1,g} During \times H1N1_s + \beta_{2,g} After \times H1N1_s + \alpha_t + \alpha_i + \alpha_j + \delta t\alpha_j + \gamma \boldsymbol{X_{it}} + \epsilon_{ijt}, \tag{4'}$$

where $\beta_{1,g}$ and $\beta_{2,g}$ denote the treatment effect of the group g during and after the pandemic, respectively. As discussed in the baseline specification, the inclusion of $t\alpha_j$ in Eq. (4′) may lead to an inconsistent estimate of the pooled post-treatment effect $\hat{\beta}_{2,g}$. To address this, we estimate dynamic treatment effects for 2011, 2013, 2015, and 2017 using Eq. (4), and compute the consistent estimator $\bar{\beta}_{post,g}$, which is the average of $\hat{\beta}_{2011,g}$, $\hat{\beta}_{2013,g}$, $\hat{\beta}_{2015,g}$, and $\hat{\beta}_{2017,g}$). We then test the equality of $\hat{\beta}_{1,g}$ and $\hat{\beta}_{1,g'}$ (or $\bar{\beta}_{post,g}$ and $\bar{\beta}_{post,g'}$) to assess whether the treatment effects differ significantly between groups g and g'.

$$\begin{aligned} Y_{ijt} &= \beta_{1,g} During_t \times H1N1_s + \beta_{2011,g} H1N1_s + \beta_{2013,g} H1N1_s + \beta_{2015,g} H1N1_s \\ &\quad + \beta_{2017,g} H1N1_s + \alpha_t + \alpha_i + \alpha_j + \delta t\alpha_j + \gamma \boldsymbol{X_{it}} + \epsilon_{ijt} \end{aligned} \tag{4}$$

4. Exposure effect of the H1N1 pandemic on stock holdings

This section presents our main empirical findings. We begin by presenting baseline results that illustrate the impact of the H1N1 pandemic on both the extensive and intensive margins of stock holdings. Following this, we perform an event study analysis to estimate the year-specific effects of exposure to the pandemic. To ensure the robustness of our findings regarding the risky share, we conduct a comprehensive set of tests. Lastly, we undertake a decomposition analysis for the risky share.

4.1. Main results

Table 2 shows the results of the effect of the 2009 H1N1 pandemic on the extensive margin and the intensive margin of stock holdings. Columns (1) and (5) control family fixed-effects on family, year, and state, and state-specific time trends. In columns (2) and (6), we add a wide set of household characteristics, motivated by past studies on household portfolio decisions, including the age and squared age of the household head, along with their interactions with the head's race, educational level, and gender. We

[22] The traditional parallel trends assumption is expressed as $\mathbb{E}\left[Y_{post}(0) - Y_{pre}(0) \mid D = d\right] = \mathbb{E}\left[Y_{post}(0) - Y_{pre}(0) \mid D = 0\right]$, which says that the average counterfactual outcome evolution, for units with any dose d in the absence of treatment, is identical to the outcome evolution observed for units in the untreated group.

[23] The strong parallel trends assumption is expressed as: $\mathbb{E}\left[Y_{post}(d) - Y_{pre}(0)\right] = \mathbb{E}\left[Y_{post}(d) - Y_{pre}(0) \mid D = d\right]$, which says that the average outcome evolution for the entire population, if all units received dose d, is equal to the actual outcome path experienced by the units in dose group d.

[24] Although we include the state-specific time trends in Eq. (3), the consistency of $\hat{\beta}_t$ is not a concern. This is because the event study approach estimates year-specific treatment effects rather than a pooled average treatment effect (Downey, 2024).

Table 2
Impact on the 2009 H1N1 pandemic on household portfolio.
Data source: PSID, waves 2001–2017, in odd-numbered years.

	Stock market participation			Log risky share		
	(1)	(2)	(3)	(4)	(5)	(6)
During × log(H1N1 death rate)	0.008	0.008	−0.002	−0.030	−0.030	−0.068
	(0.017)	(0.017)	(0.018)	(0.051)	(0.051)	(0.054)
After × log(H1N1 death rate)	−0.013	−0.013		−0.209***	−0.209***	
	(0.026)	(0.026)		(0.058)	(0.058)	
$\hat{\beta}_{2011}$			−0.023			−0.186***
			(0.028)			(0.068)
$\hat{\beta}_{2013}$			−0.014			−0.321***
			(0.029)			(0.064)
$\hat{\beta}_{2015}$			−0.041			−0.332***
			(0.030)			(0.081)
$\hat{\beta}_{2017}$			−0.058			−0.365***
			(0.037)			(0.100)
$\bar{\beta}_{post}$			−0.034			−0.301***
			(0.029)			(0.069)
Average of the outcome variable	0.410	0.410	0.410	−0.886	−0.886	−0.886
Family FE, State FE, Year FE	✓	✓	✓	✓	✓	✓
State-specific time trends	✓	✓	✓	✓	✓	✓
Family controls		✓	✓		✓	✓
Observations	25,947	25,947	25,947	9790	9790	9790
Adj.R^2	0.475	0.475	0.475	0.285	0.285	0.285

Notes: All specifications include fixed effects of family, state, and year. Columns (1), (2), (4), and (5) estimate Eq. (1). Columns (3) and (6) estimate Eq. (2). $\bar{\beta}_{post}$ is the average of estimated dynamic treatment effects in 2011, 2013, 2015, and 2017 (i.e., $\hat{\beta}_{2011}$, $\hat{\beta}_{2013}$, $\hat{\beta}_{2015}$, and $\hat{\beta}_{2017}$ in Eq. (2)). See the description in Section 4.1 for the list of family controls. Standard errors are clustered at the state level and shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

also include interactions between the age and cohort indicators of heads.[25] Our results barely change with these household-level controls. Stock market participation does not change with H1N1 intensity, possibly suggesting the fixed participation costs associated with the H1N1 pandemic remain unchanged.[26] For risky share, we find little changes related to the H1N1 death rate in the year of the pandemic outbreak.[27] In contrast, we show a significant decrease after the pandemic. The estimated coefficients in columns (4) and (5), are around −0.21, indicating that a 1 percent rise in the H1N1 mortality rate corresponds to a decrease of 0.21 percent in the risky share.

As discussed in Section 3, the pooled post-treatment effect ($\hat{\beta}_2$) in Eq. (1) may be inconsistent if we introduce the unit-specific time trends in the DID specification. Following the approach in Downey (2024), we calculate the weights on underlying dynamic treatment effects for each post-pandemic year. Appendix Figure A7 shows that these weights are downward sloping, indicating that the pooled post-treatment effect disproportionately emphasizes short-term treatment effects. In addition, the weight for 2017 is slightly negative. To address this potential bias, we estimate dynamic treatment effects for 2011, 2013, 2015, and 2017 using Eq. (2), with results reported in columns (3) and (6) in Table 2. In the second panel of the table, we compute the sample mean of the year-specific estimates, $\bar{\beta}_{post} = (\hat{\beta}_{2011} + \hat{\beta}_{2013} + \hat{\beta}_{2015} + \hat{\beta}_{2017})/4$, which serves as the consistent estimator of the pooled post-treatment effect. For stock market participation, $\bar{\beta}_{post}$ is close to zero and statistically insignificant. For risky share, $\bar{\beta}_{post}$ is statistically significant at the 1% level. A 1 percent increase in the H1N1 intensity leads to a decrease of 0.3 percent in the risky share. Alternatively, a standard deviation above the mean of the H1N1 death rate leads to a decrease of 14.48% of the mean of the risky share.[28]

Although the estimated coefficient of $After \times log(H1N1\ death\ rate)$ ($\hat{\beta}_2$) in column (5) is smaller in magnitude than the sample mean of the year-specific estimates ($\bar{\beta}_{post}$) in column (6), the difference between these estimates is not statistically significant.[29] Downey (2024) suggests that the bias of the pooled post-treatment effect might be negligible in cases where treatment effects are roughly constant across time, which could explain our results. As shown in column (6) of Table 2, the year-specific treatment effects

[25] Fagereng et al. (2017) show cohort-specific patterns in stock market participation and risky share. Heads who were born before 1928, between 1928 and 1944, between 1945 and 1964, between 1965 and 1984, and after 1984, were defined as the "greatest generation", "silent generation", "baby boom", "baby bust", and "echo boom", respectively. Note that we do not include marital status, health status, income, and financial situation (e.g., whether they own mortgages, debts, or business), since these variables can be affected by the pandemic.

[26] As noted by Fagereng et al. (2016), the fixed entry cost can affect stock market entry and exit but not affect the conditional risky share.

[27] The absence of a significant change in the risky share in 2009 may be partially explained by the timing of the data collection. As shown in Appendix Figure A6, more than 60% of the data in 2009 were collected before June, i.e., the peak of the first wave in the US. Therefore, the limited exposure to the H1N1 pandemic within our 2009 sample likely accounts for the null effect on the risky share that year. To address this, we conduct a robustness check by excluding the 2009 sample and re-estimating the exposure effect for the post-pandemic periods only. The details of this analysis are provided in Appendix C.

[28] The mean and standard deviation of state death rate during the H1N1 pandemic are 1.16 and 0.56, respectively. So the percentage change in the H1N1 death rate due to a 1 standard deviation above the mean is 48.28% (0.56/1.16*100%). Applying this percentage change to the elasticity coefficient $\bar{\beta}_{post}$, we get the percentage decrease in risky share, equal to 14.48% (0.3*48.28%).

[29] Assuming these two estimates are independent, the t statistic is computed as $t = \frac{\hat{\beta}_{pool} - \bar{\beta}_{post}}{\sqrt{SE(\hat{\beta}_{pool})^2 + SE(\bar{\beta}_{post})^2}} = \frac{-0.206-(-0.299)}{\sqrt{0.058^2+0.070^2}} \approx 1.023$, which is well below the threshold of 2.

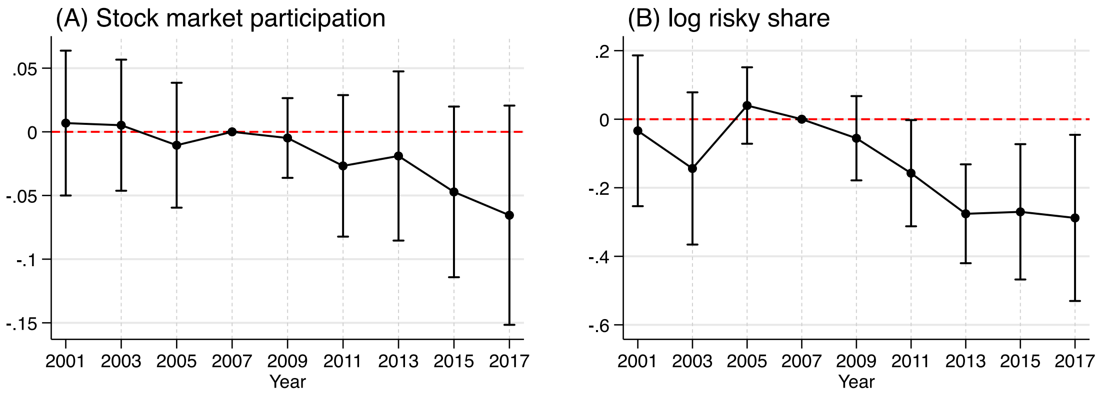

Fig. 4. Dynamic effects of the 2009 H1N1 pandemic on household portfolio.
Notes: The figure plots the estimates of β_t in Eq. (3). Appendix Table B9 reports the estimates of β_t. The regression controls fixed-effects of family, state, year, state-specific year trend, and household features. The capped spikes indicate 95 percent confidence intervals, with robust standard errors clustered at the state level.
Data source: PSID, waves 2001–2017, in odd-numbered years.

increases between 2011 and 2013, but remain relatively constant from 2013 to 2017. In our subsequent analysis, we mainly focus on the risky share, adopt the specification that includes household-level characteristics, and report both the pooled post-treatment effect $\hat{\beta}_2$ and the mean of year-specific estimates $\bar{\beta}_{post}$.

Our definition of risky assets includes both IRA and non-IRA stocks. Although IRA stocks are often managed by institutions, individuals can adjust the portfolio annually. Individuals can also hold multiple IRA accounts, provided that the total annual contribution to all IRAs does not exceed the contribution limits.[30] To address the concern that the observed decrease in risky asset share may reflect institutional trading rather than household decisions, we exclude IRA and private annuity investments and construct alternative measures.[31] The results are reported in Appendix Table B10. The reduction in the risky asset share remains robust compared to our main results. Unlike the null results with IRA stocks included, stock market participation is now significantly reduced. This is because adjusting investments in non-IRA stocks is more flexible than in IRA stocks, which are subject to withdrawal restrictions.[32] Nonetheless, we retain our original definition of risky assets, including IRA stocks, consistent with common practice in household finance literature (e.g., Guvenen (2007), Brunnermeier and Nagel (2008), Malmendier and Nagel (2011), Atella et al. (2012) and Palia et al. (2014)).

4.2. Event study

We now estimate a more flexible functional form specified in Eq. (3). In Fig. 4, we plot the estimated coefficients of β_t with 2007 as the base year. For both stock market participation and risky share, $\hat{\beta}_t$ is close to zero and statistically insignificant prior to 2007. These results justify the parallel trend assumption imposed in the DID analysis.[33] In addition, no immediate change associated with the H1N1 death rate is observed in the extensive margin or the intensive margin of stock holdings in 2009. However, after the pandemic, the risk share in states with higher mortality rates decreases more than in those with lower mortality rates. The estimated coefficient of β_t is about −0.16 in 2011, then reduces to −0.28 in 2013 and stabilizes at around −0.29 by 2017, suggesting that the exposure effect exhibits a two-year lag and persists at least until 2017.

Our finding that the year-specific treatment effect is non-decreasing over time may initially seem at odds with rational expectation theory, but it may align more closely with rational inattention theory (RIT) (Sims, 2003; Miao et al., 2022; Maćkowiak et al., 2023). According to RIT, economic agents have limited attention and must rationally ignore lower-priority items. At the beginning of the pandemic, agents naturally shifted their focus to preventing illness and caring for friends and family who were infected, temporarily sidelining financial portfolios. As vaccines and treatments became available over time, these agents would begin to reallocate their attention from medical concerns to financial considerations. Consequently, economic agents would actively adjust their portfolios, resulting in a year-specific treatment effect that appears non-decreasing over time.

[30] There are different kinds of IRAs, each with different contribution limits (https://www.iraresources.com/ira-contribution-limits).

[31] The PSID does not distinguish between personally traded IRA stocks and institutionally traded ones, nor does it differentiate between IRAs and private annuities.

[32] Withdrawing from an IRA before age 59.5 will incur an early withdrawal penalty in addition to taxes on the withdrawal (https://www.investopedia.com/articles/personal-finance/121115/how-ira-works-after-retirement.asp).

[33] One important implication from Callaway et al. (2024) is that we cannot distinguish between the standard and strong parallel trends assumption using the conventional tests of pre-trends. This limitation arises because only untreated potential outcomes are observed in pre-treatment periods, making it impossible to test the additional component of the strong parallel trends assumption, which involves treated potential outcomes. Consequently, as they suggest, the results of pre-trend tests should be interpreted with caution.

4.3. Robustness checks

We now examine the robustness of our findings on risky share in Appendix Table B11.

Add pre-pandemic state-level controls. — In column (1), we control for the interactions of observed pre-pandemic macroeconomic characteristics and year dummies. These indicators include GDP per capita, personal income per capita, unemployment rate, homeownership rate, the Federal Housing Finance Agency (FHFA) housing price index (HPI), bankruptcy cases per 100,000 people, assets per capita, deposits per capita in Federal Deposit Insurance Corporation (FDIC) insured financial institutions, and population density. These macroeconomic indicators capture factors that may affect individuals' financial risk-taking and influence household portfolio choices (Malmendier and Nagel, 2011). Moreover, as discussed in Section 2.2.3, the H1N1 death rates can be correlated with the macroeconomic situation and business cycles.[34] We find that the exposure effect of the H1N1 pandemic barely changes.

Although the outbreak of the novel H1N1 virus was unforeseen, states with fewer medical resources generally experienced higher death rates during the pandemic (Section 2.2.3). In column (2), we further add the interactions of pre-pandemic medical controls (hospital beds, active physicians, physicians in patient care, and registered nurses per 100,000 people) and year dummies. The result confirms that the effect of H1N1 intensity on risky share remains robust.

Examine whether the financial crisis confounds the results. — One may be concerned that the exposure effect of the 2009 H1N1 pandemic on household stock holdings is confounded by the 2007–2008 financial crisis, which profoundly impacted the US economy.[35] If the H1N1 intensity in a region is correlated with the local severity of the financial crisis, our estimation would be biased.

To assess the local impact of the 2007–2008 financial crisis, we compute the state-level change rate of GDP per capita, unemployment rate, and housing price index in 2008 compared with 2006 following this equation:

$$\%\Delta \mathrm{x}_j = (\mathrm{x}_{j,2008} - \mathrm{x}_{j,2006})/\mathrm{x}_{j,2006} \times 100,$$

where $x_{j,t}$ represents GDP per capita, unemployment rate, or housing price index of state j in year t. The geographic distribution of the percentage changes in these indicators is shown in Appendix Figure A8. Most states in the West (excluding Alaska) and Southeast experienced larger decreases in GDP per capita and housing prices, as well as larger increases in the unemployment rate in 2008 compared with 2006. As presented in Appendix Table B13, the correlation between H1N1 intensity and changes in these economic indicators is negligible and statistically insignificant.

We further incorporate the local impact of the 2007–2008 financial crisis in our main regression to examine whether the exposure effect of the H1N1 pandemic on the risky share persists. The impact of the financial crisis is captured by the interactions between year dummies and the percentage changes in GDP per capita, unemployment rate, and housing prices during the financial crisis. Column (3) of Appendix Table B11 presents the results. The elasticity of the risky share concerning the H1N1 death rate barely changes and remains statistically significant after we control for these interactions.

Other robustness checks. — More robustness checks are presented in Appendix C. Specifically, we examine whether migration decisions are correlated with the H1N1 death rate and exclude migrants from the sample, focusing only on households that have always lived in the same state. We also drop outlier states with unusually high H1N1 death rates, South Dakota and New Mexico. To address potential timing issues, we exclude households that participated in the 2009 PSID before the first wave of the H1N1 pandemic (i.e., between January and June 2009) and re-estimate the key coefficients. We use an alternative measure of H1N1 intensity based on HHS-level case rates and conduct placebo tests using seasonal flu case rates from 2009 to 2010 to assess the specificity of the results to the H1N1 pandemic. Furthermore, we discretize the H1N1 death rates into two or three groups based on death rate distributions. A falsification test using pre-2009 data helps verify the parallel trends assumption. Lastly, we examine the exposure effect of the H1N1 pandemic on illiquid assets, including business ownership and non-business assets like housing and cars. The findings from these robustness checks confirm that households in states with higher H1N1 death rates significantly reduce their risky asset holdings and the ownership of incorporated businesses in the post-pandemic periods, consistent with our main results.

4.4. Decomposition of the risky share

As our measure of the risky share is the proportion of liquid assets invested in risky assets, a decrease in the risky share can stem from a reduction in risky assets, an augmentation in liquid assets, or a combination of both factors. Therefore, we now explore what factors drive the decline in risky shares in Table 3.

We first examine whether the value of risky assets changes with H1N1 intensity. As shown in column (1), we observe a decrease in the value of risky assets by approximately $103 to $148 per 1 percent increase in the H1N1 death rate. While not statistically significant, this reduction accounts for 15% to 22% of the average value of risky assets ($680). Contrastingly, column (2) reveals that risk-free assets exhibit a growth trend with rising H1N1 intensity in the post-pandemic period. Specifically, we note a significant increase of $258 in the value of risk-free assets for every 1 percent increase in the H1N1 death rate, representing over a third of

[34] Adda (2016) shows that epidemics spread faster during economic booms because people are more likely to travel, which increases interpersonal connection and hence accelerates the spread of infectious diseases.

[35] Among others, see Agarwal and Varshneya (2022) and Piskorski and Seru (2021) for more discussion of the financial crisis.

Table 3
Structural changes in household portfolio choices.
Data source: PSID, waves 2001–2017, in odd-numbered years.

Dependent variable (in US$ 1000)	Risky assets (1)	Risk-free assets (2)	Liquid assets (3)	Net amount put in stocks in the past 2 years (4)
During × log(H1N1 death rate)	−10.255	14.826*	4.571	3.400
	(7.151)	(8.348)	(11.044)	(2.961)
After × log(H1N1 death rate)	−10.229	11.313	1.084	−2.494***
	(9.560)	(7.596)	(9.547)	(0.723)
$\bar{\beta}_{post}$	−14.769	25.841***	11.072	−2.517***
	(12.679)	(7.540)	(12.236)	(0.686)
Average of the outcome variable	68.016	67.151	67.151	2.781
Family, state, year FEs	✓	✓	✓	✓
State-specific time trend	✓	✓	✓	✓
Family controls	✓	✓	✓	✓
State-level controls 2008 × year dummies	✓	✓	✓	✓
Observations	25,940	25,940	25,940	22,690
Adj. R^2	0.462	0.306	0.511	0.043

Notes: Liquid assets are the sum of risky and risk-free assets. See the description in Section 4.1 for the list of family controls and state-level controls. The coefficients of $During \times log(H1N1\ death\ rate)$ and $After \times log(H1N1\ death\ rate)$ in the first panel are estimated from Eq. (1), while $\bar{\beta}_{post}$ in the second panel is the average of estimated dynamic treatment effects in 2011, 2013, 2015, and 2017 (i.e., $\hat{\beta}_{2011}$, $\hat{\beta}_{2013}$, $\hat{\beta}_{2015}$, and $\hat{\beta}_{2017}$ in Eq. (2)). Standard errors are clustered at the state level and shown in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

the mean value of these assets ($671). We further test the exposure effect of H1N1 on liquid assets in column (3) but do not find any significant correlation between the H1N1 death rate and changes in liquid assets.[36]

Since observed changes in risky shares in response to H1N1 intensity could be attributable to fluctuations in asset returns rather than household portfolio rebalancing decisions, we directly examine the net purchases of risky assets, where a positive (negative) value indicates net buying (selling) of stocks. Column (4) shows that when the H1N1 death rate increases by 1 percent, the net amount put in household stocks decreases significantly by about 25.17 dollars.

Only about 20% of our PSID sample report non-zero net purchases or sales of stocks. This result may be due to inertia (Brunnermeier and Nagel, 2008) since people do not rebalance portfolios frequently.[37] In Appendix D, we adopt the approach of Calvet et al. (2009) and decompose the change in the risky share into a passive change caused by the returns of assets and an active change arising from household decisions to rebalance their portfolios. The decomposition analysis collaborates with our previous finding that the decline in the risky share can be attributed mainly to an active rebalancing strategy.

5. Why did the H1N1 pandemic lead people to hold a lower risky share?

5.1. Increased risk aversion

From April 2009 to November 2009, the percentage of Americans worrying about getting H1N1 rose steadily from about 30% to as high as 80% (Appendix Figure A13). This suggests that with the outbreak of the swine flu, public concern about infection increased progressively. Since the H1N1 pandemic is caused by a new and unexpected virus, posing significant health risks, such concerns could potentially alter individuals' risk attitudes, which could explain why people reallocate their portfolios towards less risky assets in response to the pandemic.[38]

The PSID provides risk-tolerance information only in its 1996 wave, making it unsuitable for examining changes in risk attitudes in response to the 2009 H1N1 pandemic. Instead, we utilize data from the 2009–2018 NFCS, which includes risk attitude scores across multiple waves, to analyze this mechanism. Our regression model is specified as follows:

$$Y_{ijt} = \theta After_t \times H1N1_j + \alpha_t + \alpha_j + \gamma \boldsymbol{X}_{ijt} + \epsilon_{ijt}. \tag{5}$$

Here, Y_{ijt} denotes the risk attitudes towards financial investments of individual i who lives in state j in year t. Specifically, Y_{ijt} equals 1 if the individual is unwilling to take any financial risks (risk-averse) and 0 otherwise. $After_t$ is a binary indicator, assigned a value of 0 for 2009 and 1 for subsequent years. $H1N1_j$ is log of H1N1 death rate in state j during the pandemic. Since the NFCS data set

[36] In 2009, an increase of 1 percent in H1N1 intensity corresponded to an increase of $46 in the value of liquid assets. This decline is statistically insignificant and represents only 6.8% of the mean value of liquid assets. Similarly, from 2011 onward, a 1 percent rise in H1N1 intensity results in an increase of $110 in the value of liquid assets. However, this reduction is not statistically significant, accounting for a mere 16% of the mean liquid assets.

[37] Another explanation can be measurement errors, as people may not be able to remember their stock trading behavior precisely over the past few years. In addition, people tend to incorporate information consistent with their previous portfolio choices, resulting in sticky portfolios across time (Kuhnen et al., 2017).

[38] Previous literature has highlighted a systematic relationship between risk attitude and portfolio choice. Specifically, financial risk attitude is a significant positive predictor of the willingness to invest in stocks (Keller and Siegrist, 2006) and plays a crucial role in determining portfolio choices (Frijns et al., 2008).

Table 4
The effect of H1N1 pandemic on risk attitude.
Data source: NFCS, waves 2009, 2012, 2015 and 2018.

Outcome variable	Risk aversion (binary variable)			
	(1)	(2)	(3)	(4)
After × log(H1N1 death rate)	0.017*			
	(0.008)			
$\hat{\theta}_{2012}$		0.010		
		(0.008)		
$\hat{\theta}_{2015}$		0.022**		
		(0.010)		
$\hat{\theta}_{2018}$		0.017*		
		(0.009)		
Male × After × log(H1N1 death rate)			0.007	
			(0.014)	
Female × After × log(H1N1 death rate)			0.026*	
			(0.016)	
Unmarried × After × log(H1N1 death rate)				0.029*
				(0.015)
Married × After × log(H1N1 death rate)				0.006
				(0.011)
Average of the outcome variable	0.170	0.170	0.170	0.170
Family FE, State FE, Year FE	✓	✓	✓	✓
Family and state-level controls	✓	✓	✓	✓
Observations	90,276	90,276	90,276	90,276
Adj. R^2	0.067	0.067	0.053	0.068

Notes: Risk aversion is a binary variable set to 1 when the risk score is 1, indicating the respondent's unwillingness to take any risks in financial investments. The covariates include family-level controls and interactions between pre-pandemic state-level controls and year dummies. Column (1) shows the result based on Eq. (5). Column (2) shows the results using Eq. (6). Columns (3) and (4) report the results using Eq. (7). Standard errors are clustered at the state level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

is pooled cross-sectional, we only control for the fixed effects for years (α_t) and states (α_j), not individual fixed effects. Additionally, because the NFCS was first collected in 2009, coinciding with the H1N1 outbreak, we cannot control for the state-specific pre-trends ($t\alpha_j$) in the regression. The vector $\boldsymbol{X}_{ijt}$ encompasses household controls and the interactions between the pre-pandemic state-level characteristics and year dummies, aligning with the covariates in column (2) of Appendix Table B11. Standard errors ϵ_{ijt} are clustered at the state level.

We also perform an event study analysis, allowing the exposure effect of the pandemic (θ) to vary over time, with 2009 as the base year. We estimate the following equation for this analysis:

$$Y_{ijt} = \theta_t H1N1_j + \alpha_t + \alpha_j + \gamma \boldsymbol{X}_{ijt} + \epsilon_{ijt}. \tag{6}$$

In addition, we examine whether there exist heterogeneous effects of the pandemic on risk aversion by estimating the following equation:

$$Y_{ijt} = \theta_g After \times H1N1_s + \alpha_t + \alpha_j + \gamma \boldsymbol{X}_{ijt} + \epsilon_{ijt}, \tag{7}$$

where θ_g is the exposure effect of the H1N1 intensity on risk attitude for group g.

The related results are presented in Table 4. Column (1) shows the baseline results based on Eq. (5) while column (2) presents the event study results estimated through Eq. (6). Using both specifications, we find a significant increase in the probability of being risk-averse in response to the H1N1 pandemic in the post-pandemic periods.

We now move to investigate whether the H1N1 pandemic differentially affects people's risk attitude by running the regression (7). As presented in columns (3) and (4) in Table 4, we find that women and unmarried individuals become more risk-averse after the H1N1 pandemic. By contrast, the willingness of men and married individuals to take financial risks remains largely unaffected. In addition, our heterogeneity analysis in Section 6 offers further evidence, showing that households with female or unmarried heads significantly reduce their risky asset share after the pandemic, compared to households with male or married heads. This implies that women and unmarried individuals may be more susceptible to the pandemic's impact, leading to a shift in their risk preferences and subsequent adjustment in their risky asset allocation.

Additionally, we provide evidence related to health consumption to corroborate the risk-averse mechanism. In each wave, the PSID collects the consumption information of the preceding year.[39] As shown in Appendix Figure A14, while the overall household consumption remains unaffected by the H1N1 death rate, both health consumption and the proportion of total consumption allocated

[39] For instance, the 2017 wave documents the consumption of 2016.

to health increase, especially in 2014 and 2016.[40] Delving deeper, we examine different components of health consumption in Appendix Figure A15. Interestingly, although the share of expenditures on doctors, hospitals/nursing homes, and prescriptions do not show significant changes, the share of health insurance premiums paid by families notably *rises* with the intensity of H1N1, in line with our earlier findings from the NFCS data that people become more risk-averse in response to the pandemic.

5.2. Background risks

Factors such as health, demographics, and labor market outcomes may affect household portfolio decisions are often refereed as background risks. In Appendix Table B16, we examine whether the H1N1 pandemic affects people's health, marital status, having young children or not, earnings, and employment status. We observe no significant changes in most outcomes in response to the H1N1 intensity. However, we do find an increase in the probability of having children under 18 and the probability of being unemployed after the pandemic. In particular, the finding on the unemployment rate coincides with our earlier finding in Appendix C that the H1N1 pandemic *reduces* the likelihood of being an incorporation business owner, suggesting that the H1N1 pandemic discourage risk-taking.

We then formally control for these possible channels in Eq. (1). If any of these channels are essential, we would observe a significant change in the exposure effect of the H1N1 pandemic. However, Appendix Table B17 shows that the estimate of the exposure effects ($\bar{\beta}_{post}$) remains −0.3 significantly in columns (2)–(6), as in the baseline case (column (1)). Therefore, we conclude that these channels do not provide a satisfactory explanation for why the H1N1 pandemic affects the intensive margin of stock holdings.

Several studies suggest that households increase their risky assets when their overall family wealth increases (e.g., Calvet et al. (2009)). It is plausible that the H1N1 pandemic might diminish family wealth, resulting in a corresponding decrease in the risky share. Our findings indicate an insignificant change in family wealth associated with the H1N1 death rate (the first two columns of Appendix Table B18). Even after we add the control of total wealth in Eq. (1), we do not observe a notable shift in the exposure effect (the last two columns in Appendix Table B18). Hence, changes in family wealth are unlikely to be a driver of the result that risky shares decline with H1N1 intensity.[41]

5.3. Discussions on other possible mechanisms

In addition to a shift in risk preference, other changes in subjective perceptions may also explain the impact of a pandemic on household portfolio choices. For example, investors with longer subjective life horizons tend to increase their equity investments on both the extensive and intensive margin (Spaenjers and Spira, 2015).[42] Due to data limitations in the PSID, we cannot directly test whether the H1N1 pandemic reduced subjective life expectancy. However, we believe that subjective life expectancy is less likely to change, as self-reported health status does not significantly vary with H1N1 intensity (see column (1) in Appendix Table B16).

Another possible mechanism is a change in the degree of impatience. When people have a large discount rate, they tend to put more weights on current consumption rather than future utility. Those exposed to higher H1N1 intensity might become less patient, placing more importance on the present and devaluing the future. This could lead to a reduction in risky investments and an increase in immediate consumption. However, our data shows no significant changes in total household consumption in response to the H1N1 pandemic (Panel A of Appendix Figure A14), suggesting little change in the degree of impatience among individuals.

6. Heterogeneity analysis

This section presents evidence of how the H1N1 pandemic affects the risky share of various groups differently. Our findings show that such heterogeneous effects are related to risk attitude and income volatility. In particular, household heads who are more risk-averse tend to reduce their risky shares, which supporting our proposed risk attitude mechanism in Section 5.1. In addition, household heads experiencing greater income volatility or job instability are more likely to decreases their share of stock holdings after the pandemic.

We estimate Eq. (4) and visualize the estimates of β_{1g} and β_{2g} in Figs. 5 and 6. Since the estimates of β_{1g}s, which capture the *contemporary* change (i.e., the change in 2009) in the risky share for group g, are close to zero and statistically insignificant, we focus on the estimates of β_{2g}s, i.e., the *post-pandemic* change of the risky share.

6.1. Heterogeneous effects by groups

We first illustrate the heterogeneous patterns regarding demographic features, job features, and risk attitude in Fig. 5.

[40] The PSID documents a range of domains of consumption. However, several domains were not recorded prior to 2005. To approximate the total consumption of a household, we use the method proposed by Attanasio and Pistaferri (2014). The details about this approach can be found in Appendix F.

[41] Similarly, Brunnermeier and Nagel (2008) and Chiappori and Paiella (2011) suggest a lack of significant correlation between risky investment shares and family wealth.

[42] This could be because stocks are considered safer in the long run (Campbell and Viceira, 2002) or because the optimal stock exposure can rise with a longer investment horizon (Benartzi and Thaler, 1995).

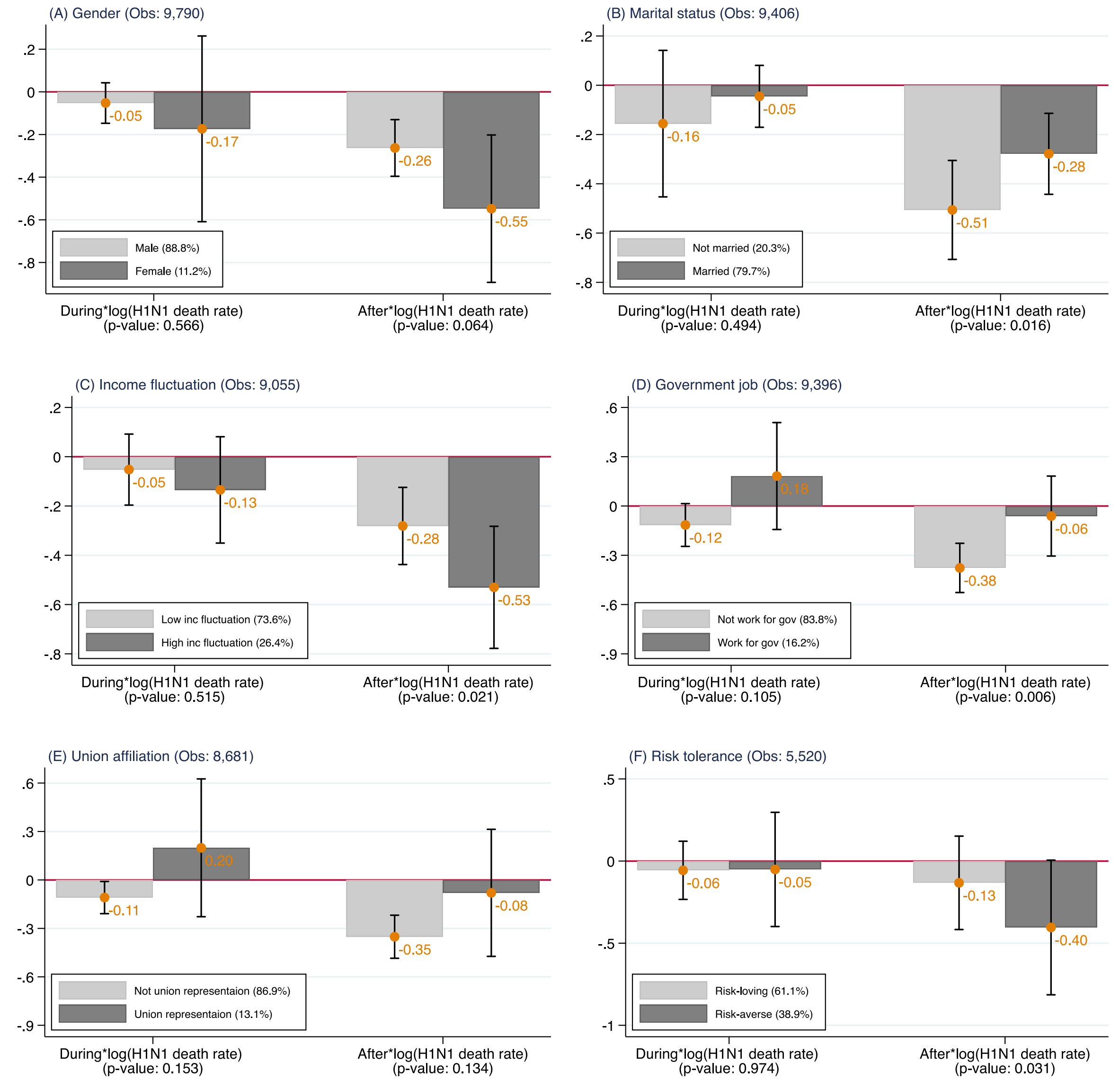

Fig. 5. Heterogeneous effects of the 2009 H1N1 pandemic.
Notes: This figure plots the estimates of Eq. (4). In each panel, the first group of bars (i.e, the estimated coefficient of $During \times log(H1N1\ death\ rate)$) represent $\hat{\beta}_{1,g}$ in Eq. (4). The second group of bars (i.e, the estimated coefficient of $After \times log(H1N1\ death\ rate)$) represent $\bar{\beta}_{post,g}$, i.e., the average of $\hat{\beta}_{2011,g}$, $\hat{\beta}_{2013,g}$, $\hat{\beta}_{2015,g}$, and $\hat{\beta}_{2017,g}$ in Eq. (4). Capped spikes represent the 95 percent confidence interval for each coefficient. In each panel of the figures, the p-value in the parentheses below the x-axis comes from a Wald test, where the null hypothesis is that the estimates of the H1N1 impact for groups g and g' are equal.
Data source: PSID, waves 2001–2017, in odd-numbered years.

Gender. — On average, when the H1N1 death rate increases by 1 percent, families with male heads reduce the risky share by 0.26%, while families with female heads reduce the risky share by 0.55% (Panel A of Fig. 5). The portfolio choice of households with female heads is more affected by the pandemic, and the difference is statistically significant, with a p-value of 0.064.[43]

Marital status. — We divide our sample into single and married subsamples according to the marital status of the household head in 2007. Panel B shows that the exposure effect of the pandemic for households whose heads are single is nearly twice that for

[43] This result may have several possible explanations. First, women are more risk-averse than men (Borghans et al., 2009) and are less willing to take financial risks (Charness and Gneezy, 2012). Second, female heads are more likely to be single than male heads. In our sample, nearly all female heads are single (approximately 99.4%), whereas only one-fifth of the male heads share this marital status (marital status is elicited from the PSID 2007 wave to address endogeneity concerns). It is difficult for single to share risks with others, so they tend to reduce their risky shares more than married couples. Third, females earn more volatile income than males. Based on our calculation, the standard deviation of female income is 0.31, significantly larger than that of male income, 0.27.

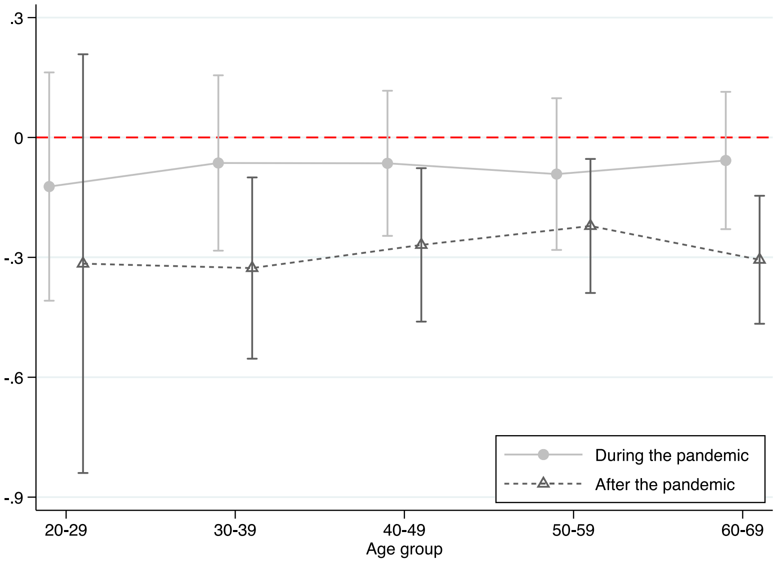

Fig. 6. Life-cycle impact of the 2009 H1N1 pandemic on risky share.
Notes: This figure plots the age profiles of the H1N1-intensity elasticities for the risky share by estimating Eq. (4). The gray circle (i.e, the estimated coefficient of $During \times log(H1N1\ death\ rate)$) represents $\hat{\beta}_{1,g}$ in Eq. (4). The hollow triangle (i.e, the estimated coefficient of $After \times log(H1N1\ death\ rate)$) represents $\bar{\beta}_{post,g}$, i.e., the average of $\hat{\beta}_{2011,g}$, $\hat{\beta}_{2013,g}$, $\hat{\beta}_{2015,g}$, and $\hat{\beta}_{2017,g}$ in Eq. (4). Capped spikes represent the 95 percent confidence interval for each coefficient. The estimated coefficients for each age group are reported in Appendix Table B15.
Data source: PSID, waves 2001–2017, in odd-numbered years.

those whose heads are married. Moreover, the difference is statistically significant, with a p-value of 0.016. Intuitively, a married individual carries less risk than a single one because a couple can share risk. As a result, households with married heads are less sensitive to the pandemic.

Income fluctuation. — To address the endogeneity concern that income stability can be affected by H1N1 intensity, we use the fluctuation of income before the pandemic as a proxy.[44] Panel C illustrates that families with more volatile income exhibit a risky share elasticity of −0.53, nearly double that of families with less volatile income, which stands at −0.28. The difference between these two groups is statistically significant at the 5% level.

Government job and Union contract. — Government jobs in PSID are defined as working for the federal, state, or local government or in a public school system. Compared to private sector jobs, government jobs are more stable (Kopelman and Rosen, 2016). Panel D shows that government workers' risky shares barely changed after the pandemic. By contrast, the risky shares of private sector workers decrease by 0.38% if the H1N1 mortality rate increases by 1%.

Panel E shows that union workers, who are more protected from layoffs and with better health benefits, behave like government workers. When the H1N1 death rate increases by 1 percent, workers represented by a union do not change risky shares. By contrast, people not represented by a union decrease their risky shares by 0.35%.

Risk aversion. — The 1996 PSID elicits respondents' risk preferences using income-related gambling questions. When answering these questions, respondents have to choose between a job with a certain income and one with a risky income. In the latter option, they face equal chances of either doubling their income or having it decreased by a specific percentage. The detailed gamble questions are described in Appendix E. Based on their responses, respondents are sorted into six risk-tolerance categories, with Category 1 (Category 6) being the most (least) risk-averse.[45] 20.8 percent of the respondents decline all the risky job options, while 7.7 percent accept all of them (Appendix Table B14).

To exploit the risk tolerance measure, we limit our subsequent heterogeneity analysis to household heads who answered the gamble questions in 1996, leaving us a subsample comprising 56.4 percent of our initial sample. Respondents in Category 1 or 2 are categorized as *risk-averse* and the others are *risk-loving*. We estimate Eq. (4) and plot the estimated effect of the pandemic in Panel F of Fig. 5. When the H1N1 death rate increases by 1 percent, the risk-averse respondents decrease their risky shares by 0.33%, while the risk-loving respondents show no significant adjustment in their risky shares. Such a difference in the exposure effect is

[44] Specifically, we run a Mincer regression of income on age, age square, educational attainment, race, gender, and state fixed effects, using data prior to the pandemic (i.e., data in 2001, 2003, 2005, and 2007) and obtain residuals. Then, we calculate the standard deviation of the residual for each household before the pandemic. A family has *high-income (low-income)* fluctuation if the standard deviation of the Mincer residual is *above (below)* the average level.

[45] It is important to acknowledge that risk preferences may shift due to events like natural catastrophes (Hanaoka et al., 2018) and macroeconomic shocks (Dohmen et al., 2016). Ideally, an index reflecting risk tolerance just prior to the H1N1 pandemic outbreak would be used. However, the PSID contains risk attitudes only in the 1996 wave. Hence, we assume the risk-tolerance categories recorded in the 1996 PSID remain constant until the 2009 pandemic.

statistically significant, with a *p*-value of 0.046. This result implies that risk-averse respondents exhibit greater sensitivity to the H1N1 pandemic exposure.

In Appendix G, we present additional analyses examining heterogeneity by race, education, and entrepreneurship. However, these analyses do not reveal significant heterogeneous effects across these groups.

6.2. Life-cycle effects of the H1N1 pandemic

We now examine how the H1N1 pandemic might differentially impact the risky share among various age groups. We estimate Eq. (4) to measure the age-specific elasticity during and after the pandemic. The index g in Eq. (4) now represents an age group (i.e., 20–29, 30–39, 40–49, 50–59, or 60–69).

As shown in Fig. 6, while the life-cycle profile of H1N1 effect *during* the pandemic is insignificant and relatively flat, the *post*-pandemic counterpart delivers a hump-shaped pattern. However, the differences across age groups lack statistical significance. Numerically, the risky share elasticity decreases from around 0.45 at ages 20–29, to 0.2 at ages 50–59, and then slightly increases to 0.3 at ages 60–69. Intuitively, young investors (aged 20–29) who have just entered the labor market have very little buffer savings and hence may bear higher background risk (Guiso and Sodini, 2013). The pandemic outbreak may lead to large uncertainty in their jobs and income. As a result, they would reduce their risky investment more than others. For older investors (aged 60–69), they are approaching retirement and should compensate for the anticipated decline in labor income by reducing risky share (Fagereng et al., 2017). Consequently, these people tend to decrease more risky share when they are exposed to higher H1N1 intensity.

Overall, our findings indicate that the impact of H1N1 on risky investments is more significant for households with female, single, and young heads. Additionally, this effect is more pronounced for household heads experiencing greater income fluctuations, those who are non-government employees and not represented by unions, and heads who exhibit higher levels of risk aversion. These results suggest that households facing greater income volatility and displaying higher risk aversion are more susceptible to the adverse effects of the pandemic.

7. Conclusion

To examine the potential impact of pandemic exposure on household portfolio allocation in subsequent years, we utilize the exogenous nature of the 2009 H1N1 pandemic and the corresponding variation in death rates across states. By analyzing nine waves of the PSID from 2001 to 2017 through a DID framework, we have made several significant observations.

Firstly, we find that exposure to the pandemic influences the composition of household portfolios in terms of risky assets (intensive margin), but it does not affect stock market participation (extensive margin). Specifically, we do not observe any changes in stock holdings during the 2009 pandemic. However, conditioning on the stock market participation, we observe that a 1 percent increase in the H1N1 death rate leads to a subsequent 0.3 percent reduction in the proportion of risky assets held in portfolios. This exposure effect remains stable over time and *persists* until the end of our sample period. Our decomposition analysis further reveals that this exposure effect primarily arises from *adjustments in risky assets* rather than liquid assets.

To further validate these findings, we utilize multiple waves of the NFCS, which confirm that higher H1N1 intensity decreases people's willingness to take financial investment risks following the pandemic. Notably, neither family wealth nor a range of factors related to health, demographic characteristics, and labor market outcomes can fully account for the relationship between the H1N1 pandemic and portfolio choices.

Additionally, we find that the impact of the pandemic varies across different household characteristics. Specifically, the effect of the pandemic is more pronounced for households with female or single heads, those experiencing greater income volatility, individuals not employed in government positions, those not represented by a union, and those with a risk-averse attitude. Furthermore, the age profiles of the exposure effect exhibit a hump-shaped pattern, indicating that middle-aged individuals exhibit a relatively smaller adjustment in their portfolios compared to other age groups. These findings collectively suggest that individuals who are more risk-averse and face greater income volatility and job instability are more susceptible to the effects of the pandemic.

Our study indicates that experiencing significant events, like a pandemic, can profoundly influence our risk attitudes. This finding holds crucial implications for the finance industry. Financial institutions may need to reassess their risk management strategies, recognizing that exposure to a pandemic can lead to changes in investment behavior and market dynamics. Additionally, understanding these behavioral shifts can aid policymakers and regulators in designing interventions that stabilize markets during periods of heightened uncertainty. Furthermore, although our analysis centers on household portfolio decisions, future research should investigate whether these shifts in risk attitudes could also surface in other domains.

Declaration of competing interest

The authors declare the following financial interests/personal relationships which may be considered as potential competing interests: Guo is serving as Associate Editor for Journal of Economic Behavior & Organization. If there are other authors, they declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Acknowledgment

This paper was previously circulated under the title "Does a Pandemic Affect Household Portfolio Choices? Evidence from the 2009 H1N1 Pandemic". All authors have contributed equally. We are grateful for the comments from the Editor, two anonymous referees, Shin-Yi Chou, Mitch Downey, Erqi Ge, Jia He, James Heckman, Xiaoyang Li, Xiangjun Ma, Shiko Maruyama, Martin Meltzer, Petra Todd, Ming-ang Zhang, and seminar participants at the AASLE, AMES, CCER Summer Institute, Chinese Finance Annual Meeting, EASM, HCEO-IESR Summer School on Socioeconomic Inequality, CEANA Meeting, Chinese University of Hong Kong, City University of Hong Kong, Hang Seng University of Hong Kong, Keio University, Osaka University, and Toyo University. We also extend our sincere thanks to Yifei Hao, Peiyu Liu, and Xinyue Luo for their exceptional research assistance. All errors are our own.

Funding

Zhang was supported by Nankai University 63242105.

Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.jebo.2025.106931.

Data availability

The paper relies on publicly available data, which can be accessed through purchase or subscription. Upon request, the authors can provide guidance on obtaining the data.

References

Abadie, Alberto, Athey, Susan, Imbens, Guido W, Wooldridge, Jeffrey M, 2023. When should you adjust standard errors for clustering? Q. J. Econ. 138 (1), 1–35.
Adda, Jérôme, 2016. Economic activity and the spread of viral diseases: Evidence from high frequency data. Q. J. Econ. 131 (2), 891–941.
Addoum, Jawad M., 2017. Household portfolio choice and retirement. Rev. Econ. Stat. 99 (5), 870–883.
Agarwal, Sumit, Mazumder, Bhashkar, 2013. Cognitive abilities and household financial decision making. Am. Econ. J.: Appl. Econ. 5 (1), 193–207.
Agarwal, Sumit, Varshneya, Sandeep, 2022. Financial crisis and the U.S. mortgage markets – a review. In: Leung, C.K.Y. (Ed.), Handbook of Real Estate and Macroeconomics. Northampton, M.A., USA, (Chapter 9).
Agüero, Jorge M., Beleche, Trinidad, 2017. Health shocks and their long-lasting impact on health behaviors: Evidence from the 2009 H1N1 pandemic in Mexico. J. Heal. Econ. 54, 40–55.
Al-Awadhi, Abdullah M, Alsaifi, Khaled, Al-Awadhi, Ahmad, Alhammadi, Salah, 2020. Death and contagious infectious diseases: Impact of the COVID-19 virus on stock market returns. J. Behav. Exp. Financ. 27, 100326.
Albanesi, Stefania, Kim, Jiyeon, 2021. Effects of the COVID-19 recession on the US labor market: Occupation, family, and gender. J. Econ. Perspect. 35 (3), 3–24.
Almond, Douglas, 2006. Is the 1918 influenza pandemic over? Long-term effects of in utero influenza exposure in the post-1940 US population. J. Polit. Econ. 114 (4), 672–712.
Almond, Douglas, Mazumder, Bhashkar, 2005. The 1918 influenza pandemic and subsequent health outcomes: An analysis of SIPP data. Am. Econ. Rev. 95 (2), 258–262.
Angerer, Xiaohong, Lam, Pok-Sang, 2009. Income risk and portfolio choice: An empirical study. J. Financ. 64 (2), 1037–1055.
Atella, Vincenzo, Brunetti, Marianna, Maestas, Nicole, 2012. Household portfolio choices, health status and health care systems: A cross-country analysis based on SHARE. J. Bank. Financ. 36 (5), 1320–1335.
Attanasio, Orazio, Pistaferri, Luigi, 2014. Consumption inequality over the last half century: Some evidence using the new PSID consumption measure. Am. Econ. Rev. 104 (5), 122–126.
Benartzi, Shlomo, Thaler, Richard H., 1995. Myopic loss aversion and the equity premium puzzle. Q. J. Econ. 110 (1), 73–92.
Betermier, Sebastien, Jansson, Thomas, Parlour, Christine, Walden, Johan, 2012. Hedging labor income risk. J. Financ. Econ. 105 (3), 622–639.
Bogan, Vicki L., Fertig, Angela R., 2013. Portfolio choice and mental health. Rev. Financ. 17 (3), 955–992.
Borghans, Lex, Heckman, James J, Golsteyn, Bart HH, Meijers, Huub, 2009. Gender differences in risk aversion and ambiguity aversion. J. Eur. Econ. Assoc. 7 (2–3), 649–658.
Breunig, Christoph, Huck, Steffen, Schmidt, Tobias, Weizsäcker, Georg, 2021. The standard portfolio choice problem in Germany. Econ. J. 131 (638), 2413–2446.
Brunnermeier, Markus K., Nagel, Stefan, 2008. Do wealth fluctuations generate time-varying risk aversion? Micro-evidence on individuals. Am. Econ. Rev. 98 (3), 713–736.
Callaway, Brantly, Goodman-Bacon, Andrew, Sant'Anna, Pedro H.C., 2024. Difference-in-Differences with a Continuous Treatment. Technical Report, National Bureau of Economic Research.
Calvet, Laurent E., Campbell, John Y., Sodini, Paolo, 2009. Fight or flight? Portfolio rebalancing by individual investors. Q. J. Econ. 124 (1), 301–348.
Cameron, Lisa, Shah, Manisha, 2015. Risk-taking behavior in the wake of natural disasters. J. Hum. Resour. 50 (2), 484–515.
Campbell, John Y., Viceira, Luis M., 2002. Strategic Asset Allocation: Portfolio Choice for Long-Term Investors. Clarendon Lectures in Economic.
Catherine, Sylvain, Sodini, Paolo, Zhang, Yapei, 2024. Countercyclical income risk and portfolio choices: Evidence from sweden. J. Financ. 79 (3), 1755–1788.
Charness, Gary, Gneezy, Uri, 2012. Strong evidence for gender differences in risk taking. J. Econ. Behav. Organ. 83 (1), 50–58.
Chen, Yuting, Cortes, Patricia, Kosar, Gizem, Pan, Jessica, Zafar, Basit, 2023. The impact of COVID-19 on workers' expectations and preferences for remote work. AEA Pap. Proc. 113, 556–561.
Chetty, Raj, Friedman, John N, Hendren, Nathaniel, Stepner, Michael, Team, The Opportunity Insights, 2020. How did COVID-19 and Stabilization Policies Affect Spending and Employment? A New Real-Time Economic Tracker Based on Private Sector Data, vol. 27431, National Bureau of Economic Research Cambridge, MA.

Chetty, Raj, Sándor, László, Szeidl, Adam, 2017. The effect of housing on portfolio choice. J. Financ. 72 (3), 1171–1212.
Chiappori, Pierre-André, Paiella, Monica, 2011. Relative risk aversion is constant: Evidence from panel data. J. Eur. Econ. Assoc. 9 (6), 1021–1052.
Christelis, Dimitris, Jappelli, Tullio, Padula, Mario, 2010. Cognitive abilities and portfolio choice. Eur. Econ. Rev. 54 (1), 18–38.
Chuang, Yating, Schechter, Laura, 2015. Stability of experimental and survey measures of risk, time, and social preferences: A review and some new results. J. Dev. Econ. 117, 151–170.
Cocco, Joao F., 2005. Portfolio choice in the presence of housing. Rev. Financ. Stud. 18 (2), 535–567.
Coibion, Olivier, Gorodnichenko, Yuriy, Weber, Michael, 2025. The cost of the covid-19 crisis: lockdowns, macroeconomic expectations, and consumer spending. J. Econ. Behav. Organ. 229, 106846.
Dawood, Fatimah S, Iuliano, A Danielle, Reed, Carrie, Meltzer, Martin I, Shay, David K, Cheng, Po-Yung, Bandaranayake, Don, Breiman, Robert F, Brooks, W Abdullah, Buchy, Philippe, et al., 2012. Estimated global mortality associated with the first 12 months of 2009 pandemic influenza A H1N1 virus circulation: A modelling study. Lancet Infect. Dis. 12 (9), 687–695.
Dohmen, Thomas, Lehmann, Hartmut, Pignatti, Norberto, 2016. Time-varying individual risk attitudes over the great recession: A comparison of Germany and Ukraine. J. Comp. Econ. 44 (1), 182–200.
Downey, Mitch, 2024. Unit-specific trends lead to biased estimates of average treatment effects in difference-in-differences settings. Unpublished paper.
Eeckhoudt, Louis, Gollier, Christian, Schlesinger, Harris, 1996. Changes in background risk and risk taking behavior. Econometrica 683–689.
Fagereng, Andreas, Gottlieb, Charles, Guiso, Luigi, 2017. Asset market participation and portfolio phoice over the life-cycle. J. Financ. 72 (2), 705–750.
Fagereng, Andreas, Guiso, Luigi, Pistaferri, Luigi, 2016. Back to Background Risk?. CEPR Discussion Paper No. DP11051, Available at SSRN: https://ssrn.com/abstract=2717601.
Fan, Elliott, Zhao, Ruoyun, 2009. Health status and portfolio choice: Causality or heterogeneity? J. Bank. Financ. 33 (6), 1079–1088.
Frijns, Bart, Koellen, Esther, Lehnert, Thorsten, 2008. On the determinants of portfolio choice. J. Econ. Behav. Organ. 66 (2), 373–386.
Gaudecker, Hans-Martin Von, 2015. How does household portfolio diversification vary with financial literacy and financial advice? J. Financ. 70 (2), 489–507.
Ghent, Andra C., Rowberry, Paige, Spiegel, Matthew I., 2024. The impact of COVID restrictions on business dynamics. Available at SSRN: https://ssrn.com/abstract=4834348.
Giuliano, Paola, Spilimbergo, Antonio, 2024. Aggregate Shocks and the Formation of Preferences and Beliefs. National Bureau of Economic Research No. w32669.
Gomes, Francisco, Smirnova, Oksana, 2021. Stock market participation and portfolio shares over the life-cycle. Available at SSRN: https://ssrn.com/abstract=3808350.
Guiso, Luigi, Jappelli, Tullio, Terlizzese, Daniele, 1996. Income risk, borrowing constraints, and portfolio choice. Am. Econ. Rev. 158–172.
Guiso, Luigi, Sodini, Paolo, 2013. Household finance: An emerging field. In: Handbook of the Economics of Finance. Vol. 2, Elsevier, pp. 1397–1532.
Guvenen, Fatih, 2007. Do stockholders share risk more effectively than nonstockholders? Rev. Econ. Stat. 89 (2), 275–288.
Hanaoka, Chie, Shigeoka, Hitoshi, Watanabe, Yasutora, 2018. Do risk preferences change? Evidence from the Great East Japan Earthquake. Am. Econ. J.: Appl. Econ. 10 (2), 298–330.
Hanspal, Tobin, Weber, Annika, Wohlfart, Johannes, 2021. Exposure to the COVID-19 stock market crash and its effect on household expectations. Rev. Econ. Stat. 103 (5), 994–1010.
Heaton, John, Lucas, Deborah, 2000. Portfolio choice in the presence of background risk. Econ. J. 110 (460), 1–26.
Hong, Harrison, Kubik, Jeffrey D., Stein, Jeremy C., 2004. Social interaction and stock-market participation. J. Financ. 59 (1), 137–163.
Hurwitz, Abigail, Mitchell, Olivia S., Sade, Orly, 2021. Longevity perceptions and saving decisions during the COVID-19 outbreak: An experimental investigation. AEA Pap. Proc. 111, 297–301.
Ibuka, Yoko, Chapman, Gretchen, Meyers, Lauren, Li, Meng, Galvani, Alison, 2010. The dynamics of risk perceptions and precautionary behavior in response to 2009 (H1N1) pandemic influenza. BMC Infect. Dis. 10, 296–307.
Keller, Carmen, Siegrist, Michael, 2006. Investing in stocks: The influence of financial risk attitude and values-related money and stock market attitudes. J. Econ. Psychol. 27 (2), 285–303.
Kelly, Heath, Peck, Heidi A, Laurie, Karen L, Wu, Peng, Nishiura, Hiroshi, Cowling, Benjamin J, 2011. The age-specific cumulative incidence of infection with pandemic influenza H1N1 2009 was similar in various countries prior to vaccination. PLoS One 6 (8), e21828.
Koijen, Ralph S.J., Yogo, Motohiro, 2019. A demand system approach to asset pricing. J. Polit. Econ. 127 (4), 1475–1515.
Kolstad, Jonathan T., Kowalski, Amanda E., 2012. The impact of health care reform on hospital and preventive care: Evidence from Massachusetts. J. Public Econ. 96 (11–12), 909–929.
Kopelman, Jason, Rosen, Harvery, 2016. Are public sector jobs recession-proof? Were they ever? Public Financ. Rev. 44, 370–396.
Kuhnen, Camelia M., Knutson, Brian, 2011. The influence of affect on beliefs, preferences, and financial decisions. J. Financ. Quant. Anal. 46 (3), 605–626.
Kuhnen, Camelia M., Rudorf, Sarah, Weber, Bernd, 2017. The Effect of Prior Choices on Expectations and Subsequent Portfolio Decisions. National Bureau of Economic Research No. w23438.
Larrimore, Jeff, Mortenson, Jacob, Splinter, David, 2022. Earnings shocks and stabilization during COVID-19. J. Public Econ. 104597.
Liang, Pinghan, Guo, Shiqi, 2015. Social interaction, internet access and stock market participation–An empirical study in China. J. Comp. Econ. 43 (4), 883–901.
Maćkowiak, Bartosz, Matějka, Filip, Wiederholt, Mirko, 2023. Rational inattention: A review. J. Econ. Lit. 61, 226–273.
Malmendier, Ulrike, Nagel, Stefan, 2011. Depression babies: Do macroeconomic experiences affect risk taking? Q. J. Econ. 126 (1), 373–416.
Miao, Jianjun, Wu, Jieran, Young, Eric, 2022. Multivariate rational inattention. Econometrica 90, 907–945.
Palia, Darius, Qi, Yaxuan, Wu, Yangru, 2014. Heterogeneous background risks and portfolio choice: Evidence from micro-level data. J. Money Credit. Bank. 46 (8), 1687–1720.
Piskorski, Tomasz, Seru, Amit, 2021. Debt relief and slow recovery: A decade after Lehman. J. Financ. Econ. 141, 1036–1059.
Pool, Veronika K, Stoffman, Noah, Yonker, Scott E, Zhang, Hanjiang, 2019. Do shocks to personal wealth affect risk-taking in delegated portfolios? Rev. Financ. Stud. 32 (4), 1457–1493.
Rasul, Imran, 2020. The economics of viral outbreaks. AEA Pap. Proc. 110, 265–268.
Restrepo, Brandon J., Zeballos, Eliana, 2020. The effect of working from home on major time allocations with a focus on food-related activities. Rev. Econ. Househ. 18 (4), 1165–1187.
Schildberg-Hörisch, Hannah, 2018. Are risk preferences stable? J. Econ. Perspect. 32 (2), 135–154.
Shachat, Jason, Walker, Matthew J., Wei, Lijia, 2021. How the onset of the Covid-19 pandemic impacted pro-social behaviour and individual preferences: Experimental evidence from China. J. Econ. Behav. Organ. 190, 480–494.
Shrestha, Sundar S, Swerdlow, David L, Borse, Rebekah H, Prabhu, Vimalanand S, Finelli, Lyn, Atkins, Charisma Y, Owusu-Edusei, Kwame, Bell, Beth, Mead, Paul S, Biggerstaff, Matthew, et al., 2011. Estimating the burden of 2009 pandemic influenza A (H1N1) in the United States (April 2009–April 2010). Clin. Infect. Dis. 52 (suppl_1), S75–S82.
Sims, Christopher, 2003. Implications of rational inattention. J. Monet. Econ. 50, 665–690.
Spaenjers, Christophe, Spira, Sven Michael, 2015. Subjective life horizon and portfolio choice. J. Econ. Behav. Organ. 116, 94–106.
Taubenberger, Jeffery K., Morens, David M., 2006. 1918 influenza: The mother of all pandemics. Rev. Biomed. 17 (1), 69–79.