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RESEARCH

How do risk perceptions change with new information? Evidence from a survey of flood-prone counties

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Abstract

Prior research has documented a tendency for insurance demand to rise abruptly in the aftermath of natural disasters. This may be attributable in part to the information shock that a disaster represents. Understanding how beliefs about risk evolve is of first-order importance for projecting climate adaptation in coming decades. Using a three-wave survey of 815 residents of flood-prone coastal counties in the southeastern United States, including 436 for whom we have longitudinal data, we study how individuals' beliefs evolve after two very different types of informational shocks about future local flooding risk: viewing flood maps, and experiencing a local flooding disaster. We find that, on average, exposure to maps causes respondents to update their beliefs of their homes' ten-year flood risk downward substantially (38%, 95% CI 13%-56%) and reduces their reported level of worry on a ten-point scale. We then leverage the natural experiment created by differential flood exposure during the hurricane seasons of 2017 and 2018, the latter of which fell between the initial wave of our survey and two follow-up waves. We find that flood disasters systematically increase flood concern in affected counties, as expected. More surprisingly, we find that those who experienced a flood before Wave 1 saw greater increases in concern between waves than those who did not – a pattern inconsistent with standard Bayesian updating and more consistent with the conservatism hypothesis, whereby experiential information shocks are incorporated into beliefs only gradually. This delayed updating process has implications for projections of climate adaptation among flood-prone populations.

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1 Introduction

The tendency for insurance demand to spike after disasters has been observed broadly across different risk domains and countries (Browne and Hoyt, 2000; Gallagher, 2014; Kousky and Michel-Kerjan, 2017; Palm, 1995). New disasters presumably convey some information about future disaster risk, and swings in demand could in principle be attributed to consumers updating their beliefs about future risk within a rational-expectations framework. However, the size of the demand spikes observed in the wake of flooding and hurricanes, for example, are hard to reconcile with the prevailing scientific view that one extreme flooding event in a given location is not predictive of similar events in the same location in the near future (Gallagher, 2014). Additionally, evidence from Gallagher (2014) and Kousky and Michel-Kerjan (2017) indicates that insurance takeup falls back to baseline levels quite quickly in the years after a disaster. These patterns are hard to reconcile with a rational expectations model where consumers are always considering historical data and make small adjustments to risk perceptions using Bayesian updating.

The literature has offered at least two competing explanations. First, lab evidence shows that people can switch between mental states of not worrying at all and worrying "too much" when it comes to insurance purchases (Mcclelland et al., 1993). Under this hypothesis, coastal residents are underinsured most of the time, but dramatically overinsured in the years after an event. The second alternative explanation is that consumers are usually inattentive to unusual risks like floods, but are galvanized by major flooding events to consider their risks anew and make a considered insurance decision. This would suggest that flood-prone households are usually underinsured, and only become adequately insured after a disaster focuses their attention on the threat.

Both explanations indicate that policy interventions that help people develop beliefs consistent with actual risk may be welfare-improving. However, less is known about how beliefs change over time, both in the immediate aftermath of a disaster and as the disaster recedes. In this paper, we use cross-sectional and longitudinal survey data to understand how beliefs about risk evolve after two different informational treatments: exposure to FEMA flood maps, and exposure to a nearby flooding event. These two treatments align with dual-process theories of risk perception which distinguish between analytical and experiential information processing (Slovic et al., 2004).

To measure the responses to these information stimuli, and in the first case to administer the stimulus itself, we conduct a survey of coastal U.S. residents using Amazon's Mechanical Turk platform. We elicit information on respondents' beliefs about the likelihood of home flooding, and their level of worry about home flooding on a 1-10 scale. We also include questions about willingness to pay for hypothetical flood insurance policies and actual flood insurance takeup. The first treatment is an experiment in the survey wherein some participants were randomly selected to view online FEMA flood insurance risk maps (FIRMs) covering their neighborhoods and are given brief instructions in how to read them. This experimental treatment allows us to estimate the effect of neutral information disclosure on flood beliefs and willingness to pay for insurance.

We define two additional treatment variables based on exposure to local flood events during the hurricane seasons of 2017 (before Wave 1) and 2018 (after Wave 1, before subsequent waves). We assign a treatment value of 1 to respondents if their original residence as reported in Wave 1 was in a county that received a Presidential Disaster Declaration during each period. Surely not all residents of declared counties directly experienced flooding, so we interpret this treatment as an information shock loosely analogous to flood map exposure. This treatment captures the secondary, informational effects of floods near to one's home, like increased attention to flooding on the local news and greater salience of local flood risk. It is not intended as a proxy for personal experience with home flooding (among longitudinal respondents, only 4% report their home flooding between waves, far below the 41% of the sample that is "treated" under this definition.)

In our cross-sectional analysis, we find that exposure to the FIRM maps causes statistically and economically significant reductions in perceptions of flood likelihood and flood worry on average, but no detectable effect on stated willingness to pay for insurance. We show that this reduction comes despite the fact that estimates of flood risk made by those who did not view the maps tend to *underestimate* the risk portrayed by the maps. Qualitative responses suggest heterogeneity in how respondents react to the maps, with roughly twice as many reporting reduced concern as increased concern after viewing them. Regression models with interaction terms do not yield consistent evidence of differential updating based on observable risk characteristics; we discuss these results in Appendix C.

In our longitudinal analysis, we find that reported flood likelihood perceptions increase for those exposed to flood events between waves, more so than for those who did not experience a flood, indicating that flood exposure can stimulate updating in a way that resembles learning from the flood maps. However, the effect on self-reported worry is marginally significant, and is absent for willingness to pay for insurance. Most surprisingly, we find that respondents who experienced a local flood in the year before Wave 1 reported significantly increased risk perceptions between Waves 1 and 2 relative to those who did not, even those who did not experience a subsequent flood event after Wave 1. This suggests a delayed or drawn-out updating process that complicates the narrative surrounding belief updating after disasters.

This paper contributes to the literature on insurance demand and learning about risk in several ways. Past studies of insurance demand response to floods, notably Gallagher (2014) and Kousky and Michel-Kerjan (2017), are based on aggregate data and do not measure individual decisions or underlying beliefs. By contrast, we analyze beliefs and insurance demand at the individual level using directly elicited beliefs. A small number of other studies have used direct elicitation (Bakkensen and Barrage, 2022; Royal and Walls, 2019), but unlike in those studies we elicit beliefs from the same respondents both before and after a flood event. This novel approach allows us to a) experimentally manipulate exposure to FIRM maps, b) observe heterogeneity in beliefs based on both demographic and hydrological characteristics, and heterogeneity in the effect of FIRM exposure, and c) test theories of behavioral belief updating implied by the empirical work on insurance takeup.

The remainder of this paper is organized as follows. Section 2 provides a literature review and background on the question of insurance demand response to disas-

ters. Section 3 describes the survey design in detail and presents descriptive statistics about the sample of responses collected. Section 4 details the empirical approach and Section 5 presents the results. Section 6 concludes with a discussion of the results and policy implications.

2 Background

Empirical research has found that rare events like earthquakes, flooding, and cancer clusters can induce behavioral changes that are suggestive of dramatic shifts in underlying beliefs. Several studies have analyzed behavior after such events, ranging from insurance purchase decisions (Gallagher, 2014; Palm, 1995; Said et al., 2015) to real estate transactions (Bin and Landry, 2013; Davis, 2004; McCoy and Walsh, 2018; McCoy and Zhao, 2018) to resource allocation by corporations and governments (Dessaint and Matray, 2017; Wibbenmeyer et al., 2016). The general tendency is for people to behave in ways consistent with either increased perceived risk, more risk-averse preferences, or both, such as buying earthquake or flood insurance after an earthquake or flood occurs nearby. Most of these papers do not have disaggregated microdata on individual decisions, except for Dessaint and Matray (2017) and the real estate transaction studies.

The clearest evidence for the insurance demand response to flooding events comes from Gallagher (2014), who assembles a panel dataset of U.S. flooding events and insurance takeup running from 1980 to 2007 and conducts an event study. He finds that a flood disaster declaration in a given county increases participation in the National Flood Insurance Program (NFIP) in that county by an average of 9%, but that the effect diminishes and eventually disappears over the ensuing decade. In a similar study, Kousky and Michel-Kerjan (2017) finds a slightly smaller effect on flood insurance takeup due to hurricanes, and finds that it fades out even more quickly.

Flood insurance is an ideal context to study demand responses because flood insurance prices are set by the government-run National Flood Insurance Program (NFIP) and do not generally change systematically in response to flood events (Gallagher, 2014). However, in both studies the researchers rely on aggregate insurance data, so they can not analyze mechanisms on an individual level nor do they use any data on perceptions of flood likelihood or costs, or individual risk preferences.

Another strand of the literature has investigated changes in risk preferences after catastrophic events. Several papers measure risk preferences among populations recently exposed to disasters like Hurricane Katrina or the 2004 Indian Ocean tsunami. These have found varied results (Bchir and Willinger, 2013; Cameron and Shah, 2015; Cassar et al., 2017; Eckel et al., 2009). To our knowledge, only Hanaoka et al. (2018) include longitudinal data from before and after a major disaster, and they find increased tastes for risk among those more exposed to the 2011 Fukushima earthquake.

Other researchers have used real estate transactions data to infer differences in belief, either cross-sectionally across different types of market participant (Bernstein et al., 2019, 2022; Keys and Mulder, 2020), or over time after changes to FIRM

maps (Hino and Burke, 2021), high-profile floods like Hurricane Katrina (Hopkins and Muller, 2019), or a personal flooding experience (Hyde, 2023). Most studies in this vein have taken a revealed-preference approach, although a few studies in this domain have used explicit belief data elicited from at-risk homeowners or residents, as we do. Bakkensen and Barrage (2022) conduct a door-to-door survey of coastal residents in Rhode Island and find a negative correlation between perceived flood risk and actual risk as estimated by U.S. government authorities, providing partial inspiration for the present work. Royal and Walls (2019) undertake a similar survey in a Maryland county on the Chesapeake Bay and find cross-sectional evidence that past flood experience predicts risk perceptions. Others have used beliefs about climate change – although not beliefs about the likelihood of specific disasters – as reported on surveys (Deryugina, 2013; Marlon et al., 2018; Osberghaus and Fugger, 2022; Shum, 2012), or data on beliefs about rare events in a non-learning context (Cameron, 2005; Champ et al., 2010; Reynaud et al., 2013).

A related literature on belief updating biases offers a theoretical lens for interpreting heterogeneity in how individuals respond to informational shocks. Tversky and Kahneman (1974) identify the representativeness heuristic – the tendency to overweight new data relative to prior beliefs – as a systematic departure from Bayesian updating. The opposite bias, conservatism, leads individuals to underweight new information and update too slowly toward the data (Benjamin, 2019; Edwards, 1968). Experimental evidence suggests considerable heterogeneity across individuals in which bias predominates: some update in a broadly Bayesian fashion, while others systematically overreact or underreact to new information (Benjamin, 2019).

In the context of flood risk, these biases have distinct predictions. A representativeness bias or information-recency effect would predict sharp, potentially excessive belief updating immediately following a disaster, while conservatism would predict sluggish or delayed updating that persists across periods. Our finding that respondents who experienced a local flood before Wave 1 continued to show increasing risk perceptions between waves – rather than stable or declining beliefs as a simple Bayesian model would predict – is most consistent with the conservatism hypothesis, suggesting that experiential flood shocks are incorporated into beliefs only gradually.

Finally, a literature on risk aversion in the presence of ambiguity finds that risks with an ill-specified probability of occurring (like having precisely 13 rainy days in a given city next month) are sometimes treated differently than ones that are numerically specified, as they are, for example, in the context of government flood maps (Di Mauro and Maffioletti, 2001). Additionally, Schade et al. (2012) find that self-reported "worry" is more important than subjective probability in predicting insurance demand, motivating us to include a worry elicitation question in our survey. Prior work has also found a disconnect between numerical risk assessments and willingness to pay for insurance protection (Robinson and Wouter Botzen, 2019), so we analyze beliefs and willingness to pay in parallel. The persistent disconnect we document between updated flood risk perceptions and willingness to pay for insurance across both informational treatments suggests that preference updating – learning about one's tolerance for loss and demand for protection – may follow a slower or qualitatively different process than belief updating about flood likelihood, a distinction with direct implications for the design of information interventions.

3 Survey and data sources

3.1 Survey construction

Our survey was fielded in three waves on Amazon's Mechanical Turk platform. We conducted the initial wave between June 4 and July 31, 2018. Responses were solicited from 124 coastal counties in 13 states spanning the Gulf coast and most of the Eastern Seaboard (see Fig. 1. We had two goals in mind when determining the set of counties included in the study. First, we wanted to focus on people living near flood zones. These individuals are making the most high-stakes decisions about flood insurance coverage and are presumably most sensitive and attuned to the threat of flooding. Second, we want to focus on regions with particular vulnerability to hurricanes, as quasi-experimental exposure to hurricanes is the basis of our longitudinal identification strategy. While narrowly targeting the survey to coastal residents potentially weakens the external validity of our findings, we argue that the coastal sample is drawn from the true population of interest.

Records from the past 150 years suggest that coastal areas on the Gulf of Mexico, the Atlantic coast below New York City, and Long Island are the areas of the mainland United States most frequently targeted by powerful hurricanes (Blake et al.,

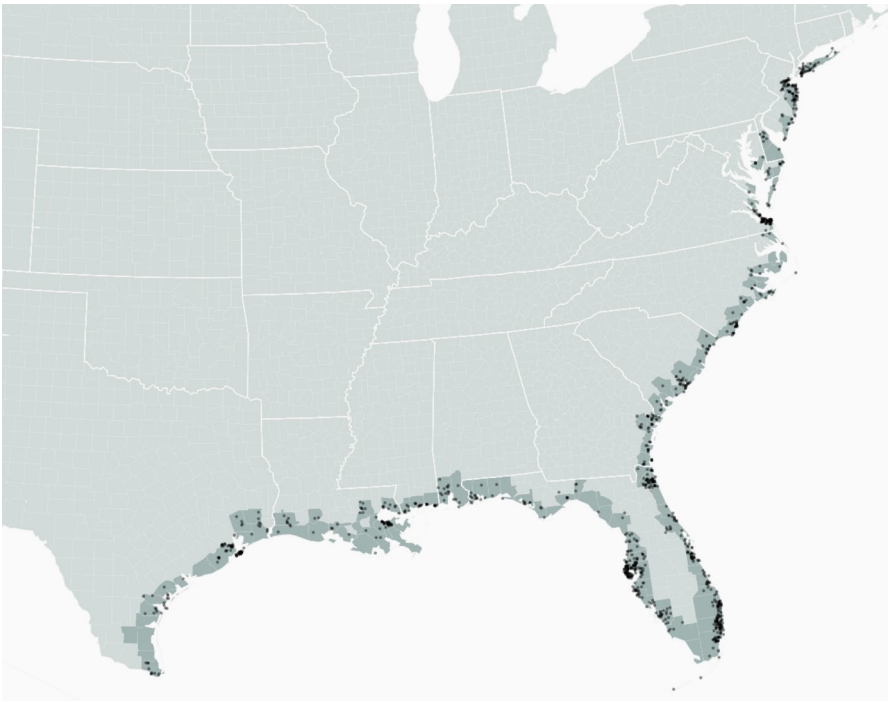

Fig. 1 Location of 815 survey respondents. The figure above shows the approximate geographic location of each of the 815 survey respondents within the United States based on reported ZIP code of residence. Darker points indicate a greater density of responses. The 124-county coastal region where the survey was fielded in shaded dark gray

2011). The counties included in the study region were selected based on their proximity to the shore and, in a few cases, unusually high historical flood insurance claims. All counties along the Atlantic coast from Texas to Long Island were included in the region. Several counties in Texas, Louisiana, and Florida that were not coastal but which had historical claim rates since 1978 greater than 0.02 per capita were also included (Casey, 2017). Additional counties and city jurisdictions in Maryland, Virginia, and Delaware which lie on the Chesapeake Bay or Delaware Bay were included if they met this threshold as well. Harris County (Houston) qualified for inclusion under this criterion but was specially excluded due to the potential for its large population to distort the sample, but many coastal counties in Texas with exposure to Hurricane Harvey in 2017 were included.

Figure 1 shows the 124-county region (highlighted) and the geographic distribution of the 815 valid responses received during the initial sample window (See Section 3 for our inclusion criteria and our attempts to weed out low-quality responses from the sample). As expected, responses are clustered in the most densely-populated parts of the region, including Galveston, New Orleans, Tampa Bay, Miami, Norfolk/Virginia Beach, and the New York City metropolitan area. Florida is home to 45% of the sample.

The survey included questions on basic demographic characteristics, as well as questions about respondents' personal history with flooding and the situation of their home relative to the water. See Appendix E for a complete list of survey questions in the first wave.

To follow up on our initial survey, we sent follow-up survey invitations by email to all participants in December 2018 (Wave 2) and again in June 2019 (Wave 3). This version of the survey elicited the same variables as Wave 1, with additional questions probing each respondent's personal flood experiences in the time since the initial survey (see Appendix C). A total of 436 validated respondents participated in at least one of the two follow-up waves, giving us an effective response rate of 53% for our longitudinal study.

3.2 Additional data sources

In addition to personal survey data on beliefs and preferences, we collect information about local flood risk that we can then match to respondents based on their county of residence. Specifically, we gather county-level disaster declaration data from the Federal Emergency Management Agency that dates back to 1953 and covers virtually every county in the United States, including the entirety of our sample (Fema, 2023).

More fine-grained, municipal-level data on flood history from 1990 to 2007 is available from Gallagher (2014), but only covers 84% of the sample. The remaining 16% of observations include respondents living in rural areas not covered by any formal municipality, or who list municipality names that do not correspond to any municipality listed in the data. While excluding these observations will reduce statistical power, including this data source allows us to control for local flood history on a more granular level. Our main analysis relies on county-level data to maximize our sample size, but the subsample with municipal flood data is analyzed separately as well and largely reinforces our findings.

Each respondent was asked to provide their home elevation in feet, either from memory or by entering their address into a third-party service that provides localized elevation information (see Appendix E, Question 32). Several of the provided answers were deemed unreliable because they exceeded the maximum elevation of their respective counties according to recreational mountain climbing website www.peakbagger.com. In cases where listed home elevations exceeded county high points, we imputed elevation by first using the Google Earth API to find the elevation of the ZIP code centroid. Because elevation can vary significantly even within a single ZIP code, and because even a difference of a few feet in elevation can make a dramatic difference in flood risk near the shore, this imputation process potentially introduces significant measurement error. We find that the results are robust to excluding these respondents.

3.3 Map exposure experiment

To test the effect of exposure to authoritative flood risk information, we randomly assign a 25% subsample of survey respondents to complete an additional section of the survey. These respondents were directed to find their homes on FEMA's online flood insurance rate maps and given a brief primer on how to read the two flood zone designations before answering the key outcome questions (see Fig. 2 for an example

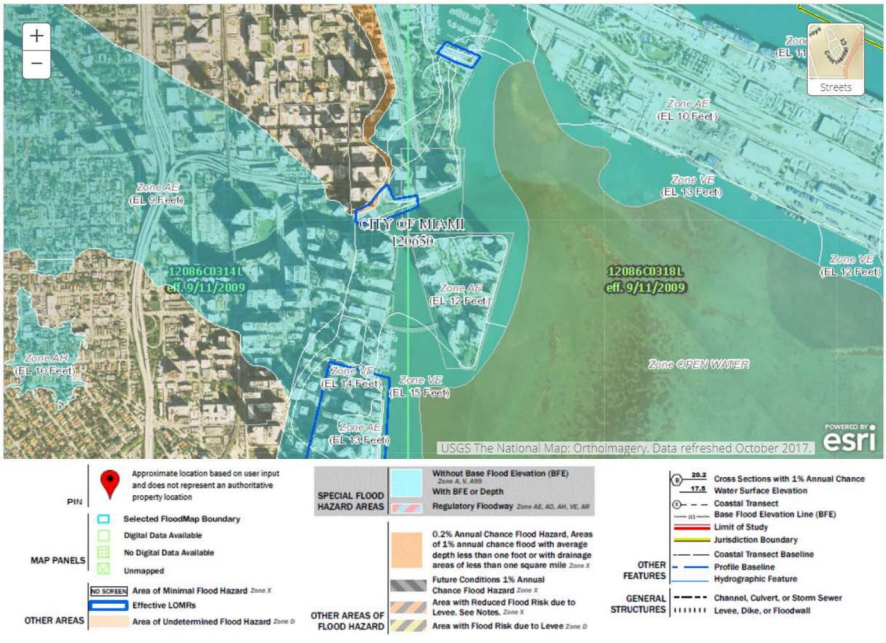

Fig. 2 Sample flood hazard map. A screen capture of the online flood map utility available from FEMA at https://msc.fema.gov/portal/search, featuring a map of downtown Miami, Florida. The blue area is the 100-year flood zone, and the orange area (visible in a small strip in the top-center of the image) is the 500-year flood zone. See Appendix E, Question 20 for more details on how the maps were presented and explained to respondents

of how these maps appear in a web browser) Details of the map interface and instructions are provided in Appendix B.

3.4 Sample composition

Table 1 summarizes participation in the authors' survey across the three waves. A total of 1,769 respondents completed the first survey wave in June/July of 2018, and a majority of that sample completed at least one of the two follow-up surveys in late 2018 and 2019. We subject responses to a battery of validation checks in an attempt to isolate and remove inattentive and unreliable respondents, and retain only 815 participants (46%) as our main analytic sample. Of these, 436 participated in at least one follow-up wave, and comprise our longitudinal sample. Section 3.5 describes the validation process in greater detail.

Table 2 displays descriptive statistics for the main analytic sample after validation checks. The convenience sample that participated in the survey is not intended to be representative of the population in the covered counties, let alone the U.S. population at large. Typically for an online survey, the sample skews quite young, with approximately 58% of the sample under age 35. These people are approaching their prime home-buying years, so their perspective on flood insurance may be of particular interest. The sample is better-educated and more likely to agree with the notion

Table 1 Survey Participation Across Waves

Panel A | All participants	**Control**	**Map treatment**	**Total**
Initial survey (Wave 1) June-July 2018	1,363	406	1,769
Follow-up survey (Wave 2) December 2018-February 2019	659	208	867
Follow-up survey (Wave 3) June-July 2019	391	136	527
Participated in any follow-up wave	753	240	993
Panel B | Validated participants	**Control**	**Treatment**	**Total**
Initial survey (Wave 1) June-July 2018	666	149	**815**
Follow-up survey (Wave 2) December 2018-February 2019	306	76	382
Follow-up survey (Wave 3) June-July 2019	230	55	285
Participated in any follow-up wave	348	88	**436**

Total participation in various waves of the survey, by experimental arm. Participants were dropped from the validated sample if they had any of the following characteristics: inconsistent geographic information, failed attention checks, survey completion time under 5 minutes, non-monotonic preferences on the multiple price list elicitations, missing demographic data, or implausibly large shifts in stated beliefs of flooding between waves. See Section 3.5 for more detail about the validation process. Unless otherwise noted, all models and descriptive statistics are based on the validated sample of 815 respondents

Table 2 Sample composition: cross-sectional sample

Variable	(1) **Analytic sample** *(n = 815)*	(2) **Control group** *(n = 666)* 82%	(3) **Map exposure group** *(n = 149)* 18%
A. Demographics			
Female	0.63	0.63	0.66
White/Caucasian	0.8	0.8	0.81
Age			
Under 35	0.55	0.55	0.58
Over 55	0.13	0.13	0.13
College degree	0.50	0.50	0.51
Income greater than $60k	0.32	0.32	0.32
Homeowner	0.55	0.56	0.54
Carries flood insurance — homeowner	0.40	0.40	0.44
Years in current home	6.7	6.5	7.6
	(7.8)	(7.5)	(8.6)
B. Home characteristics			
Single-family, detached house	0.65	0.64	0.68
Lives less than 20 min walk to water	0.53	0.53	0.52
Lives less than 5 min walk to water	0.14	0.14	0.16
Home elevation (ft)	26.2	26.7	24.1
	(35.4)	(36.5)	(30.4)
County flood and hurricane disasters, 1953-2017	15.3	15.2	15.4
	(4.7)	(4.6)	5.1
Past flood in current home	0.13	0.13	0.13
C. Beliefs			
Identifies as Democrat	0.36	0.35	0.40
Believes climate change is happening	0.81	0.80	0.85
Cannot remember major hurricane in locality	0.15	0.15	0.16
D. Key response variables			
Perceived 10-year flood risk (%)	3.2	3.4	2.4
	(4.9)	(5.0)	(4.4)
Worry about home flooding (1-10 scale)	4.4	4.5	4.0
	(2.5)	(2.5)	(2.4)
WTP for flood insurance ($)	637	637	635
	(650)	(641)	(690)

Sample mean values with standard deviations in parentheses for continuous variables. Control and map exposure groups formed by random assignment of the map exposure treatment during Wave 1 of the authors' survey. Map exposure was assigned to 25% of survey participants, but the final map exposure group is smaller due to differential completion rates and a higher rate of failed attention checks. All variables based on self reports of respondents in Wave 1 of authors' survey with exception of county flood histories, which are based on FEMA disaster records. Home elevation values imputed for some respondents based on municipality or ZIP code. No significant difference in means at the 10% level between the groups for any variable listed except the response variables

that climate change is occurring than the U.S. population at large (Howe et al., 2015), but relatively low-earning given its high educational attainment, possibly owing to age or current enrollment in school.

By design, the sample is much more exposed to the threat of flooding than the American population on average. Many respondents report living within twenty minutes' walking distance of a body of water that could flood their homes. Many are living at very low elevations, with 44% of the sample in homes at an elevation of 10 feet above sea level or less. Most can remember or name past hurricanes that struck their towns of residence, 40% of homeowners report carrying flood insurance policies, and nearly 8% report flooding in their homes in the past year alone.

Table 2, Panel D also displays distributional information on the three key response variables elicited in the survey. These data reinforce the sense that this sample is particularly flood-prone, and regards itself that way. The mean perceived 10-year flood risk is 3.2%, and roughly a quarter of respondents perceive 10-year flood risk at greater than 10%, consistent with a home in the 100-year flood plain. By comparison, Peri et al. (2017) estimate that only 5% of the U.S. population lives in 100-year flood plains as designated by the FIRM maps.

One quarter of the sample was randomly selected for exposure to our experimental treatment described in Section 3.3. This yielded a substantial experimental group sample of 150 validated responses. While precisely one quarter of all participants who began the survey were assigned to the treatment condition, only 18% of the final sample is treated. The discrepancy is explained partly by differential attrition but mostly by the additional attention question that these respondents faced, which caused us to discard a higher percentage of these responses (See Section 3.5). Table 2 shows that the control group (Column 2) and experimental group (Column 3) are quite well-balanced on the observable characteristics, with no statistically significant difference in means across the two groups for any of the listed variables at the 10% level. Nonetheless, there could be some small latent differences introduced by differential attention screens.

Table 3 provides a comparison of the main sample (Column 1) to the longitudinal sample (Column 2), the subset of participants who completed at least one of the follow-up waves in late 2018 and 2019. We find that the follow-up wave responders and non-responders were relatively balanced on observable characteristics, reducing concerns about attrition bias within our initial sample. Still, longitudinal participants were significantly more likely to be homeowners, to have lived longer at their current addresses, to be white, and to be over 55 years of age. Notably, they were about equally likely to have experienced a county flood disaster between waves, reducing our concern that treatment itself would cause respondents to select out of the sample due to life disruptions. Our analysis considers the smaller longitudinal sample separately and does not rely on direct comparisons between the longitudinal and non-longitudinal participants, so any selective attrition that did occur is not a major threat to our analysis.

Additionally, Table 3 provides a comparison of the control group (Column 3) to the treatment group (Column 4) for our second treatment, flood exposure in 2018. This treatment is highly spatially correlated (see Fig. 7), so it is no surprise that the treatment group and control groups are not close counterfactual representations of

Table 3 Sample composition: longitudinal sample

Variable	(1) Analytic sample (*n = 815*)	(2) Longitudinal respondents only: Full sample (*n = 436*)	(3) Longitudinal respondents only: No local flood in 2018 (*n = 258*)	(4) Longitudinal respondents only: Local flood in 2018 (*n = 178*)
A. Longitudinal completion rate		52%	51%	55%
B. Demographics				
Female	0.63	0.67^	0.69	0.64
White/Caucasian	0.80	0.82	0.78	0.88†
Age				
Under 35	0.55	0.45^	0.43	0.48
Over 55	0.13	0.19^	0.20	0.17
College degree	0.50	0.52	0.53	0.51
Income greater than $60k	0.32	0.30	0.31	0.29
Homeowner	0.55	0.62^	0.63	0.61
Carries flood insurance | homeowner	0.40	0.41	0.41	0.40
Years in current home	6.7	7.5^	8.1	6.7
	(7.8)	(8.5)	(8.7)	(8.0)
C. Home characteristics				
Single-family, detached house	0.65	0.67	0.67	0.67
Lives less than 20 min walk to water	0.53	0.56	0.53	0.58
Lives less than 5 min walk to water	0.14	0.17	0.17	0.16
Home elevation (ft)	26.2	27.4	22.8	34.0†
	(35.4)	(37.3)	(29.3)	(45.9)
County flood and hurricane disasters, 1953-2017	15.3	15.3	16.8	13.2†
	(4.7)	(4.7)	(4.0)	(4.8)
Past flood in current home	0.13	0.11^	0.13	0.08
D. Beliefs				
Identifies as Democrat	0.36	0.36	0.36	0.35
Believes climate change is happening	0.81	0.81	0.81	0.81
Could not remember major hurricane in Wave 1	0.15	0.11^	0.07	0.16†
E. Key response variables				
Perceived 10-year flood risk (%)	3.2	3	3.4	2.5
	(4.9)	(4.8)	(5.3)	(3.9)
Worry about home flooding (1-10 scale)	4.4	4.4	4.3	4.4
	(2.5)	(2.5)	(2.5)	(2.4)
WTP for flood insurance ($/year)	637	649	689	593
	(650)	(667)	(689)	(632)
F. Longitudinal characteristics				
Flood disaster in home county, 2017 (before Wave 1)	0.75	0.78^	0.78	0.77
Flood disaster in home county, 2018 (between waves)	0.4	0.41	0	1
Home flooded between waves		0.04	0.03	0.05
G. Updated response variables				
Updated perceived 10-year flood risk (%)		2.9	2.8	3.0
		(4.5)	(4.4)	(4.5)
Updated flood worry (1-10 scale)		4.7	4.7	4.8
		(2.5)	(2.5)	(2.5)
Updated WTP for flood insurance ($/year)		773	838	682
		(1039)	(1146)	(861)

Sample mean values with standard deviations in parentheses for continuous variables. Longitudinal sample is comprised of all validated respondents who participated in either Wave 2 or Wave 3 of the authors' survey. Participants are divided into no-local-flood and local-flood samples (Column 3 and 4) based on whether there were declared flood disasters in their home counties between survey waves. All variables based on self reports of respondents in Wave 1 of authors' survey with exception of county flood histories, which are based on FEMA disaster records. Home elevation values imputed for some respondents based on municipality or ZIP code.

^Significant difference in means at the 5% level between the longitudinal sample and the full analytic sample (Columns 1 and 2).

† Significant difference in means at the 5% level between the no-local-flood and local-flood samples (Columns 3 and 4)

each other. Respondents whose counties were impacted by floods in 2018 were significantly more likely to be white, live at higher elevations, and have fewer historical floods in their home counties. This is of course not a selection effect but a reflection of the fact that the two hurricane disasters in 2018 happened to affect regions with these characteristics. As such, we cannot directly compare these groups and infer a causal effect from hurricane exposure on the key outcome variables. We leverage our longitudinal data to control for Wave 1 beliefs and effectively compare each respondent to her past self, as opposed to a respondent in the other experimental group.

3.5 Sample validation

We subject all responses to a battery of validation checks, excluding 53% of the original survey pool on grounds including failed attention checks, inconsistent geographic information, implausibly fast survey completion times, and non-monotonic price list responses. Our validation process and the validated sample of 815 respondents is described in detail in Appendix A.

4 Approach and hypothesis

4.1 Cross-sectional analysis: Determinants of risk perceptions, worry, and insurance demand

We start by analyzing determinants of our three variables of interest: risk perceptions, levels of worry, and willingness to pay for insurance. These analyses do not have a causal interpretation, but do allow us to document descriptive correlations between characteristics of our survey respondents, their homes, and the response variables.

For this analysis, we run the following regression:

$$\mathrm{Y}_{\mathrm{i}} = \alpha + \delta \mathrm{X}_{\mathrm{i}} + \gamma \mathrm{Z}_{\mathrm{i}} + \eta_{\mathrm{j}} + \epsilon_{\mathrm{i}}, \tag{1}$$

where

- Y_{i} is one of our three response variables,
- X_{i} are individual-level hydrological controls like home elevation and local flood history,
- Z_{i} are demographic controls like age, education, and climate beliefs,
- η_{j} is a fixed effect at the county level (not included in some specifications), and
- ϵ_{i} is an error term.

We will estimate these models for the three response variables listed in Table 2 above:

1. Subjective probability of a home flooding event in the next ten years,
2. self-reported worry on a 1-10 scale about a future home flooding event, and
3. willingness to pay for flood insurance.

We expect proximity to water will be positively associated with risk, worry, and willingness to pay for insurance. We also expect past flood experience to be positively correlated with all our dependent variables, however we expect more recent floods to have a stronger effect. By the same token, we expect elevation to be negatively correlated with all three variables of interest. Finally, we hypothesize that being a self-reported climate-change believer is positively correlated with our variables of interest. In an alternative specification with county fixed effects included, we expect these statistical relationships might be attenuated as differences across county are subsumed by the fixed effects, at least in the case of hydrological features.

4.2 Cross-sectional analysis: experimental map exposure treatment

We take advantage of the crisp identification provided by our randomized controlled trial to estimate the following model of the effect on outcome *Y* for individual *i* in county *j* using OLS:

$$\mathrm{Y}_{\mathrm{i}} = \alpha + \beta \mathbb{I}\{\text{map exposure}\}_{\mathrm{i}} + \delta \mathrm{X}_{\mathrm{i}} + \gamma \mathrm{Z}_{\mathrm{i}} + \eta_{\mathrm{j}} + \epsilon_{\mathrm{i}}, \tag{2}$$

where

- X_{i} are individual-level hydrological controls like home elevation and local flood history,
- Z_{i} are demographic controls like age and education, and
- η_{j} is a fixed effect at the county level,
- ϵ_{i} is an error term,

and β captures the effect of random assignment to map exposure.

We do not have strong priors about whether exposure to the flood maps will raise or lower flood expectations on average. There is a possibility that respondents will be primed by the topic of the study or the FEMA flood map website, and will begin to grow alarmed about the possibility of floods. On the other hand, obtaining information that may be perceived as authoritative could be reassuring to some respondents, and lull them into a sense of security. Generally people underestimate the risk of rare events like floods (Mcclelland et al., 1993), so if the maps serve to shift people toward a more hydrologically-informed view of their personal risk, flood perceptions should increase on average.

Of more central interest is how map exposure affect the *relationship* between home characteristics and flood perceptions, because this could illuminate the learning process of coastal residents. We do not elicit the response variables from participants in the map exposure treatment before they are treated – only after – and thus cannot analyze the map exposure effect at the within-subject level. However, we can identify the informational treatment effect by comparing the control group and experimental group.

To do so, we take the specification in Eq. 2 and add interaction terms for the hydrological variables:

$$\mathrm{Y}_{\mathrm{i}} = \alpha + \beta \mathbb{I}\{\text{map exposure}\}_{\mathrm{i}} + \delta \mathrm{X}_{\mathrm{i}} + \gamma \mathrm{Z}_{\mathrm{i}} + \rho \mathrm{X}_{\mathrm{i}} \times \mathbb{I}\{\text{map exposure}\}_{\mathrm{i}} + \epsilon_{\mathrm{i}}. \quad (3)$$

The coefficients on the interactions terms ρ will indicate whether people who have been exposed to the FEMA flood maps have a different approach to the problem of estimating their risk, one that for instance might rely more on home elevation and less on distance to the water. This could indicate that the map exposure treatment is having subtle but important heterogeneous effects on respondents with different levels of fundamental flooding risk.

We remove the county fixed effects from this model because of the concern that these fixed effects will mask the crucial variation between observations in home elevation and distance from the water that are important to identifying the interaction effect. Additionally, in an interaction between the county fixed effects and the map exposure indicator, many observations would be the lone observation in their cells.

4.3 Longitudinal analysis: flood exposure treatment

After the 2018 hurricane season, we invited the same respondents included in this analysis to complete an additional survey in order to build a longitudinal dataset. To measure the causal effect of flood exposure while accounting for the non-uniform likelihood of treatment, we use two approaches. First, we take the difference of the response variable elicited in the follow-up wave and the initial wave as the dependent variable (which we call the "first difference" approach). We also regress updated values for the dependent variables elicited in waves 2 and 3, $Y_{i,2}$, on both an indicator for treatment and the previous values elicited in the first wave $Y_{i,1}$ (the "control with previous wave" approach). Specifically, to test the impact on outcome Y for individual i in county j, we estimate the following OLS model:

$$\mathrm{Y}_{\mathrm{i},2} = \alpha + \beta \mathbb{I}\{\text{county flood disaster}\}_j + \gamma Y_{i,1} + \rho \mathrm{X}_{\mathrm{i}} + \psi \mathrm{Z}_{\mathrm{i}} + \eta_{\mathrm{i}} + \epsilon_{\mathrm{i}}, \quad (4)$$

where β is the coefficient of interest and the outcome variable Y tested will be one of the following:

1. subjective probability of a home flooding event in the next ten years,
2. self-reported worry on a 1-10 scale about a future home flooding event,
3. willingness to pay for flood insurance.

Following Gallagher (2014), we define the treatment $\mathbb{I}\{\text{county flood disaster}\}_j$ as 1 for counties within the Presidential Disaster Declaration zone for any disaster between August 2018 and November 2018 inclusive, and 0 otherwise. To study how perceived risks, beliefs, and insurance take-up may attenuate as a disaster recedes in memory, we also compare the changes in response across waves from those who experienced flooding in the year prior to our initial survey to all other respondents. In this latter case, the treatment is exposure to a hurricane in one's home county between June 2017 and May 2018 (with Hurricane Harvey in Texas being the most prominent example).

The 2017 hurricane season was more active, with three distinct hurricanes prompting disaster declarations (Harvey, Irma, and Nate) and major population centers including the Texas Gulf Coast and South Florida affected. The 2018 season was more muted, with only two hurricanes prompting declarations (Florence and Michael) and both affecting relatively sparsely populated parts of the coastal zone in our study. As a result, 78% of the longitudinal sample received the 2017 flood treatment while only 41% received the 2018 flood treatment.

$$\mathrm{Y}_{\mathrm{i},2} = \alpha + \beta \mathbb{I}\{\text{county flood disaster}\}_j + \gamma Y_{i,1} + \rho \mathrm{X}_{\mathrm{i}} + \psi \mathrm{Z}_{\mathrm{i}} + \eta_{\mathrm{i}} + \epsilon_{\mathrm{i}}. \quad (5)$$

We estimate an additional model that includes both treatment variables simultaneously to address the strong spatial correlation of the treatments:

$$\mathrm{Y}_{\mathrm{i},2} = \alpha + \beta_1 \mathbb{I}\{\text{county flood disaster 2017}\}_j + \beta_2 \mathbb{I}\{\text{county flood disaster 2018}\}_j + \gamma Y_{i,1} + \rho \mathrm{X}_{\mathrm{i}} + \psi \mathrm{Z}_{\mathrm{i}} + \eta_{\mathrm{i}} + \epsilon_{\mathrm{i}}. \quad (6)$$

The geographic variation in hurricane recollections across respondents illustrates the spirit of this identification strategy (see Fig. 7). For example, most respondents in South Carolina consider Hugo (1989) to be the last major hurricane to strike, while respondents just a few miles to the south in Georgia consider themselves to have been struck by Irma (2017). This divergence between neighbors with similar intristic flood risk suggests that recent disaster experience can act as a source of quasi-experimental variation that is plausibly exogenous to other determinants of belief updating.

4.4 Estimator choice

Our analysis employs three estimators depending on the outcome variable. Perceived flood likelihood (logP) is approximately lognormal and estimated via OLS; in specifications with county fixed effects, the fixed effects are absorbed using the within transformation. Flood worry is reported on a 1-10 ordinal scale and estimated using ordered logit throughout, as the numeric scale values do not carry quantitative significance. In specifications with fixed effects, we use state rather than county fixed effects for the ordered logit models to avoid incidental parameters bias, which would be severe given 105 county groups and fewer than 8 observations per county on average. The coefficients reported for ordered logit specifications are log-odds ratios. OLS and ordered logit estimates of worry are broadly similar but not identical; we treat the ordered logit as the preferred specification throughout given the ordinal nature of the outcome. Finally, willingness to pay for insurance is censored at \$0 and \$6,000 and hence estimated via Tobit instead of OLS.

4.5 Threats to causal inference

Identification of the two treatment effects relies on experimental and quasi-experimental variation, respectively. The map exposure treatment is randomly assigned, and the order in which respondents encounter it is orthogonal to other relevant factors, so the estimated effects can be interpreted causally. One caveat is that only 18% of the final validated sample received the treatment rather than the intended 25%, due

to higher attrition among treated respondents who faced an additional attention check. This could introduce modest selection bias if less conscientious respondents were disproportionately screened out of the experimental group. The quasi-experimental strategy faces additional threats. Treatment boundaries based on Presidential Disaster Declarations may not perfectly capture exposure – residents just outside declared counties may still be affected, while some within declared counties may have experienced little disruption. Media spillovers could further blur the treatment gradient, particularly for high-profile storms like Harvey that generated national coverage. Additionally, the strong spatial clustering of the 2017 and 2018 treatments means treated and untreated respondents differ on county-level characteristics beyond flood exposure itself. We address this by including individual-level controls and clustering standard errors at the state level.

5 Results

5.1 Cross-sectional analysis

We first perform a cross-sectional analysis of the determinants of perceived flood risk, worry, and insurance willingness to pay for all validated respondents participating in Wave 1 of the survey. On the right-hand side of the model, we include a host of hydrological factors like elevation and distance to the water that can potentially help explain beliefs, as well as basic demographics including categorical bins for age, race, income, educational attainment, home type, and risk preference. Additionally, we consider past flooding in a respondent's current residence as a powerful indicator of idiosyncratic flood risk not captured by topography.

The results are displayed in Table 4. In the first set of models (Columns 1-3), we exclude fixed effects in an attempt to most precisely measure the role of hydrological factors like home elevation in determining beliefs and insurance demand. In the second set of models (Columns 4-6), we include geographic fixed effects, which soak up some of the explanatory power of the hydrological characteristics but do not improve our model fit, as measured by adjusted R^2. Models for insurance willingness to pay (Columns 3 and 6) are estimated with a Tobit specification to account for censoring in our multiple price list elicitation mechanism. Additionally, we restrict those models to homeowners only, as the hypothetical question about willingness to pay for insurance is probably less meaningful and intelligible to non-homeowners.

We find that the determinants of risk perceptions and worry are broadly similar, although many do not correlate strongly with willingness to pay for insurance. Lower home elevation, living within 20 minutes walk of the water, a past flood in the home, and shorter home tenure are all statistically significantly linked with greater perceptions of risk and greater worry. This result holds across both the fixed effects and non-FE specifications. Of these, only lower home elevation is significantly associated with greater willingness to pay for insurance. (While the OLS coefficients in columns 1 and 4 and the log-odds ratios reported in columns 2 and 5 are not directly comparable in magnitude, the signs and statistical significance accord well.) Additional fac-

Table 4 Determinants of flood beliefs, worry, and insurance demand

	(1)	(2)	(3)	(4)	(5)	(6)
				County / state fixed effects		
	Log risk *(OLS)*	Worry *(ologit)*	Ins. WTP *(Tobit)*	Log risk *(OLS)*	Worry *(ologit)*	Ins. WTP *(Tobit)*
Home elevation (ft)	-0.006674***	-0.009571***	-1.801***	-0.005948**	-0.007952***	-1.685*
	(0.001281)	(0.00203)	(0.6796)	(0.002305)	(0.001632)	(0.916)
<20 min walk to water	0.7346***	0.7451***	6.215	0.8193***	0.8072***	-66.46
	(0.1455)	(0.1564)	(89.04)	(0.1781)	(0.1747)	(77.87)
<5 min walk to water	-0.1464	-0.07934	138	-0.3476	0.01968	213.1***
	(0.2164)	(0.1465)	(84.99)	(0.2314)	(0.1548)	(77.84)
County floods, 1953–2017	-0.01078	0.01988*	-5.11*		-0.03126	
	(0.00825)	(0.01072)	(2.835)		(0.03418)	
Past flood in home	2.001***	1.745***	88.62	1.956***	1.638***	109.6
	(0.1588)	(0.2439)	(118.5)	(0.1678)	(0.2179)	(144.4)
Climate change believer	0.3201**	0.6094***	96.63	0.4651***	0.5876***	177.4**
	(0.1154)	(0.1362)	(74.72)	(0.1194)	(0.1494)	(78.6)
Female	-0.05797	0.3566***	160.7**	-0.02728	0.3503**	123.2
	(0.2028)	(0.1367)	(65.14)	(0.1462)	(0.1424)	(80.04)
Home tenure	-0.02263***	-0.02561***	0.1291	-0.02541***	-0.02422***	1.597
	(0.006239)	(0.005901)	(4.807)	(0.006038)	(0.004839)	(5.812)
Homeowner indicator	-0.3928	0.0588		-0.4002	0.05339	
	(0.3278)	(0.2837)		(0.3016)	(0.3049)	
Democrat	0.1852**	0.1235	-31.79	-0.02497	0.1545	-62.72
	(0.07483)	(0.09753)	(50.37)	(0.1035)	(0.1091)	(47.8)
Govt. flood support (std)	-0.01174	0.02674	36.45**	-0.03768	0.03429	32.98
	(0.04533)	(0.0466)	(16.87)	(0.04064)	(0.04313)	(30.77)
Has flood insurance			438.9***			445.2***
			(42.58)			(66.96)
Demographic controls	✓	✓	✓	✓	✓	✓
County/state FE				county	state	county
Observations	815	815	449	815	815	449
Adj. R^2 / Pseudo R^2	0.167		0.0141	0.163		0.0270

Specifications 1 and 4 are OLS estimates, specifications 2 and 5 are ordered logit estimates, and specifications 3 and 6 are Tobit estimates with censoring at $0 and $6,000, all with standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The coefficients displayed for specifications 2 and 5 are log-odds ratios. Log risk is the natural log of the respondent's estimated 10-year risk of a home flood. Worry is reported on a 1-10 scale, with greater values indicating greater worry. Willingness to pay for flood insurance is expressed in terms of annual premia for a $100,000 policy, and those models are estimated on the subsample of respondents who are homeowners. Home elevation values imputed for some respondents based on municipality or ZIP code. Home tenure defined as number of years at current residence. Govt. flood support is a standardized measure of beliefs about what share of hypothetical flood damages would be covered by government aid. Demographic controls (not shown) comprise categorical bins for age, race, income, educational attainment, home type, and risk preference. WTP specifications include an indicator for any current flood insurance coverage. Specifications 4 and 6 include county-level fixed effects. There are 22 observations that are the sole observation in their counties and are thus effectively excluded from these models. Specification 5 includes state-level fixed effects

tors like county flood history, gender, political party identification, and beliefs about government aid are only significant sporadically, for some specifications.

Overall, the results are reassuring; they bolster our basic mental model of flood concern and insurance demand as a function of hydrological flood risk and past experiences. They suggest that any sorting by more risk-oblivious people into more risky homes a la (Bakkensen and Barrage, 2022) is not pervasive enough to drive a counterintuitive negative relationship between risk and worry, although that could explain

why respondents living very near the water (<5 minute walk) are not significantly more concerned than the larger group of respondents living within a 20 minute walk.

The general concordance of these results with our hypotheses and intuition also provides some reassurance that our non-incentive-compatible elicitation data on the online survey platform are not too noisy to be usable, at least once non-validated respondents are stripped out of the sample.

We note the inconsistent relationship between county flood history (as defined by the number of disaster declarations for flooding or hurricane-related weather) and flood concern across models. This could reflect the fact that home elevation, distance to the water, and respondents' history of flooding captures the relevant information contained in the flood history variable, which only varies at the county level and may not be representative of a respondent's risk (note this variable is completely absorbed in the fixed effects in specifications 4 and 6). However, the county floods variable, after partialing out these other hydrological factors, presumably contains information about flood risk that is not captured in more obvious and salient markers of risk like proximity to the water. While there is naturally some noise in this data, counties with frequent historic flood disaster declarations relative to residents' elevation and distance to the water may have other characteristics that make severe flooding more likely – characteristics that residents are less likely to perceive. We return to this question in the next section.

5.2 Map exposure treatment

We turn to examining the outcome of our map exposure experiment. We randomly assigned a quarter of the sample to find and view the FIRM map corresponding to their home location. Figure 3 displays histograms showing the distribution of two key response variables across experimental ($n = 149$) and control ($n = 666$) groups. Figure 3(a) tells a basic story: exposure to FEMA flood maps made the average respondent much more sanguine about flood risk, reducing perceived ten-year flood risk on average by 0.481 log points, or 38% ($p = 0.006$, 95% confidence interval 13% to 56%). This effect is statistically and economically significant. We find that map exposure has a similarly strong and negative effect on self-reported worry about floods on a 1-10 scale (not shown in the figure). The 5% of respondents who estimated their risk at exactly zero were arbitrarily imputed a value of $e^{(-5)} = 0.0067$, but the results are robust to different imputed values or their exclusion.

Exposure did not significantly affect the spread of the distribution, but generally redistributed mass from the right side of the distribution, corresponding to flood risk of 1% per year or more, to the left side of the distribution. The qualitative responses of participants reinforce this finding, but also hint at significant heterogeneity in the effect across individuals. After examining the maps, 27% of respondents reported feeling their homes were less at risk than they expected, as compared to 14% who reported feeling more at risk. However, the lower panel in Fig. 3 shows the effect is not statistically significant in the case of willingness to pay for a $100,000 flood insurance policy, with exposure to the map depressing respondents' willingness to pay for insurance by $96 per year ($p = 0.21$).

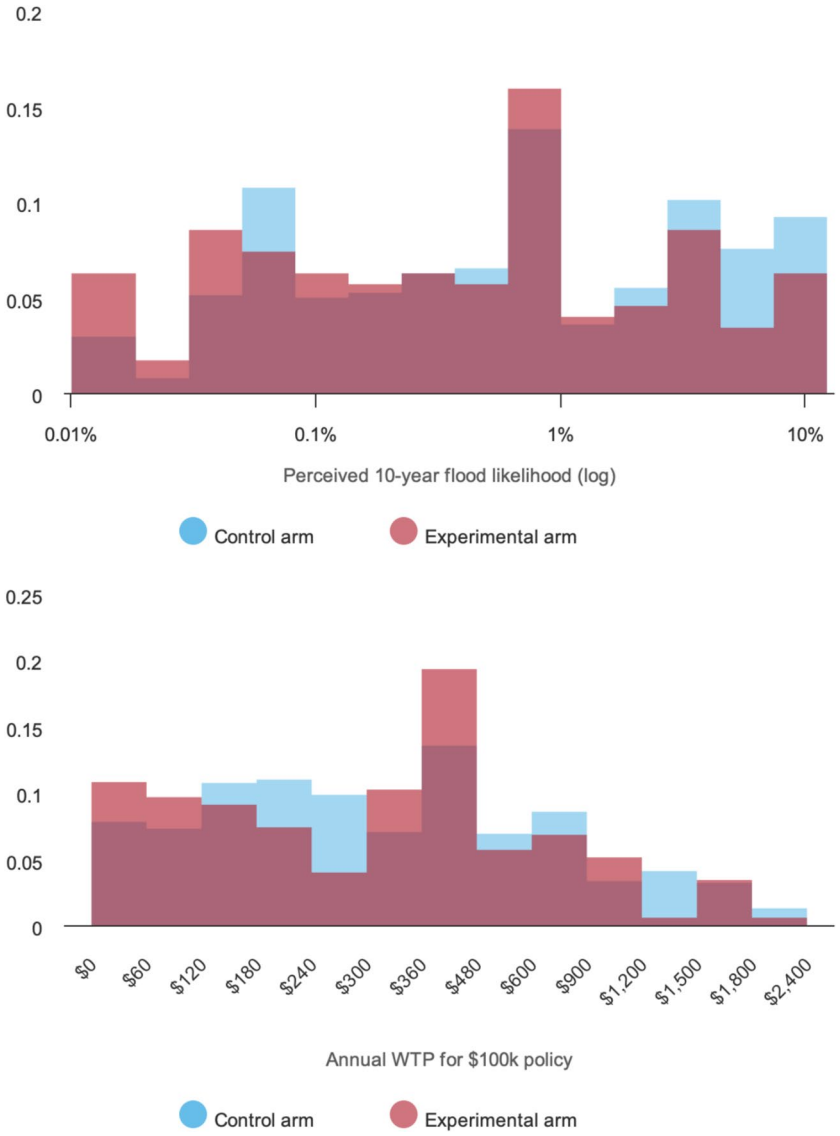

Fig. 3 Effect of exposure to FEMA flood maps. The histograms above show the distribution of two quantities, (**a**) flood beliefs and (**b**) willingness to pay for flood insurance, separately for the control group and the experimental group. The *x*-axis in subfigure (a) is displayed on a log scale as the distribution is roughly lognormal, and the analysis that follows focuses on the logged version of this variable. The *x*-axis in subfigure (a) is nonlinear and reflects the 16 choices elicited on the multiple price list question (see Appendix E, Question 29)

Figure 4 presents a calibration exercise that analyzes the size of this effect in the context of the FIRM risk estimates. The probability ranges are chosen to correspond to the three flood zones demarcated by the flood maps: the 100-year zone, the 500-year zone, and the region outside both zones, under the assumption that the maps

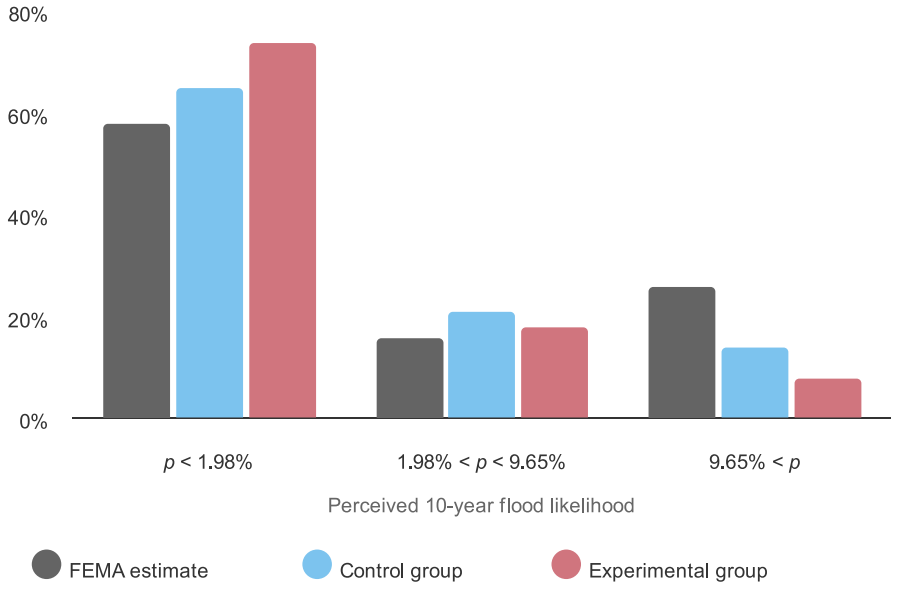

Fig. 4 Calibration of flood risk perceptions to FEMA flood maps. The distribution of perceived 10-year flood likelihoods across three intervals corresponding to FEMA flood maps. The distribution of implicit flood probabilities (gray bars) are obtained by assuming the risk of annual flooding implied by the FEMA flood maps is correct, and that flood outcomes are independent across years. For example, 26% of respondents reported living in the 100-year flood zone, implying a 10-year flood risk of 9.65% or greater for 26% of the sample. The control group (blue) and experimental group (red) distributions, by contrast, are based on actual responses to the survey question about perceived flood likelihood

are accurate and that each year's flood activity can be represented by independent Bernoulli draws. For example, a respondent living in the 100-year flood zone, with annual risk of flooding equal to at least 0.01, would have a 10-year flood likelihood of:

$$\mathrm{p} \geq 1 - (1 - 0.01)^{10} = 0.0965.$$

Fully 26% of the sample reports living in the 100-year flood plain, as compared to an estimated 5% of the U.S. population as a whole (Peri et al., 2017). The gray columns represent the implicit distribution of ten-year flood risk under these assumptions. Thus, the gray column in the third group on the right of the figure represents 26%. While there is no assurance that the distribution of the control group across flood zones matches the experimental group's, the fact that the groups are randomly assigned and very well-balanced on observable characteristics (Table 2) suggests this is a valid comparison for both groups.

The blue and red columns represent the perceived 10-year flood risk elicited from respondents, grouped into the same three ranges. For example, approximately 14% of the control group and 8% of the experimental group report their ten-year flood risk is greater than 9.65%. Under the assumption that approximately 26% of the control sample is also in the 100-year flood zone, we find that the control group is more

optimistic about flood risk than the FEMA maps, with only 15% reporting the high ten-year flood risk that is implied by the definition 100-year flood plain.

This could be explained a number of ways aside from computational error: perhaps the participants do not think flooding across years is serially uncorrelated, do not think the flood map averages apply to them if they have taken special measures to protect their homes, or do not take the flood maps at face value, although it should be noted that critics believe the FEMA flood maps tend to err on the side of *understating* risk (Kelly, 2017). Respondents might also perceive that the exact flooding criterion that they are predicting (chance of at least one inch of water flooding in their homes) does not correspond to the definition of flood that underlies the FIRM maps.

Nevertheless, exposure to flood maps does not pull respondents closer to the distribution given by the gray columns; in fact, it pushes them further away. As shown above, exposure to flood maps has a negative impact on flood risk perceptions; people become more optimistic about flood risk. The differential distributions of the blue and red columns in this figure accords with that finding, and is highly statistically significant according to a χ^2 test of homogeneity.

One potential explanation is that the disclosure of information tends to make people feel more secure and less vulnerable, even if the information itself is adverse (Schade et al., 2012). Perhaps respondents' confidence in government disaster preparations is inspired by the very existence of the detailed FEMA flood maps. That could explain the especially strong effect on self-reported worry, which might capture this security-blanket effect. Other research suggests differential response when people received good and bad news, although studies alternately report that good news or bad news is taken more to heart, depending on domain (see for example Eil and Rao, 2011; Soroka, 2006).

Table 5 displays regression models estimating the effect of map exposure. The models in Columns 1 and 3 correspond to Fig. 3, and shows the large and statistically significant effect of map exposure on beliefs and degree of worry, but the lack of a significant effect on insurance willingness to pay. Specifications 4-6 confirm the strong effect of map exposure and largely replicate the findings of Table 4.

In the preceding section, we argued that a regression model with interaction terms could be used to analyze exposure to flood maps as an information treatment. To explore whether map exposure differentially affects respondents based on their individual risk characteristics, we estimate specifications augmented with interaction terms between map exposure and the hydrological variables, as detailed in Eq. 3 and reported in Appendix C. The interaction results are sensitive to modeling choices and do not yield consistent evidence of heterogeneity in the map exposure effect; we discuss the results in Appendix C.

5.3 Local hurricane and flood exposure treatment

To complement the cross-sectional analysis in the previous section, we use our longitudinal data to identify the effect of a very different type of information shock: a hurricane striking close to home. We define two treatment variables based on exposure to local flood events during the hurricane seasons of 2017 (before Wave 1) and 2018 (after Wave 1, before subsequent waves). We assign a treatment value of 1 to

Table 5 Effect of flood map exposure

	(1)	(2)	(3)	(4)	(5)	(6)
				County / state fixed effects		
	Log risk *(OLS)*	Worry *(ologit)*	Ins. WTP *(Tobit)*	Log risk *(OLS)*	Worry *(ologit)*	Ins. WTP *(Tobit)*
Randomized map experiment	-0.4809**	-0.3397***	-15.79	-0.491**	-0.5049***	-74.54
	(0.1762)	(0.1258)	(67.87)	(0.209)	(0.1294)	(87.21)
Home elevation (ft)				-0.00611**	-0.008243***	-1.673*
				(0.002359)	(0.001575)	(0.9056)
<20 min walk to water				0.819***	0.7981***	-65.42
				(0.1808)	(0.1655)	(78.64)
<5 min walk to water				-0.3306	0.05009	214.3***
				(0.2306)	(0.1627)	(77.84)
County floods, 1953–2017					-0.03054	
					(0.03448)	
Past flood in home				1.935***	1.629***	109.2
				(0.1579)	(0.2219)	(144.3)
Climate change believer				0.4775***	0.6053***	179.5**
				(0.1202)	(0.15)	(80.16)
Female				-0.02449	0.3561**	125.9
				(0.1473)	(0.1393)	(80.52)
Home tenure				-0.02419***	-0.02274***	1.833
				(0.006074)	(0.004449)	(5.762)
Homeowner indicator				-0.43	0.02964	0
				(0.2909)	(0.3)	(0)
Democrat				-0.01587	0.1692	-61.3
				(0.1062)	(0.1082)	(45.74)
Govt. flood support				-0.03461	0.03756	34.36
				(0.03968)	(0.04259)	(32.3)
Has flood insurance						443.1***
						(67.37)
Hydrological controls				✓	✓	✓
Demographic controls				✓	✓	✓
County/state FE				county	state	county
Observations	815	815	451	815	815	449
Adj. R^2 / Pseudo R^2	0.006		0.0000	0.170		0.0271

Specifications 1 and 4 are OLS estimates, specifications 2 and 5 are ordered logit estimates, and specifications 3 and 6 are Tobit estimates with censoring at $0 and $6,000, all with standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The coefficients displayed for specifications 2 and 5 are log-odds ratios. Log risk is the natural log of the respondent's estimated 10-year risk of a home flood. Worry is reported on a 1-10 scale, with greater values indicating greater worry. Willingness to pay for flood insurance is expressed in terms of annual premia for a $100,000 policy, and those models are estimated on the subsample of respondents who are homeowners. Home elevation values imputed for some respondents based on municipality or ZIP code. Home tenure defined as number of years at current residence. Demographic controls (not shown) comprise categorical bins for age, race, income, educational attainment, home type, and risk preference. Specifications 4 and 6 include county-level fixed effects. There are 22 observations that are the sole observation in their counties and are thus effectively excluded from these models. Specification 5 includes state-level fixed effects

respondents if their original residence as reported in Wave 1 was in a county that received a Presidential Disaster Declaration during each period. Recall that 78% of the sample received the 2017 flood treatment while only 41% received the 2018 flood treatment. Figure 5 displays a map of the five hurricanes and associated treatment regions, while Table 6 details the distribution of observations in each treatment category across states.

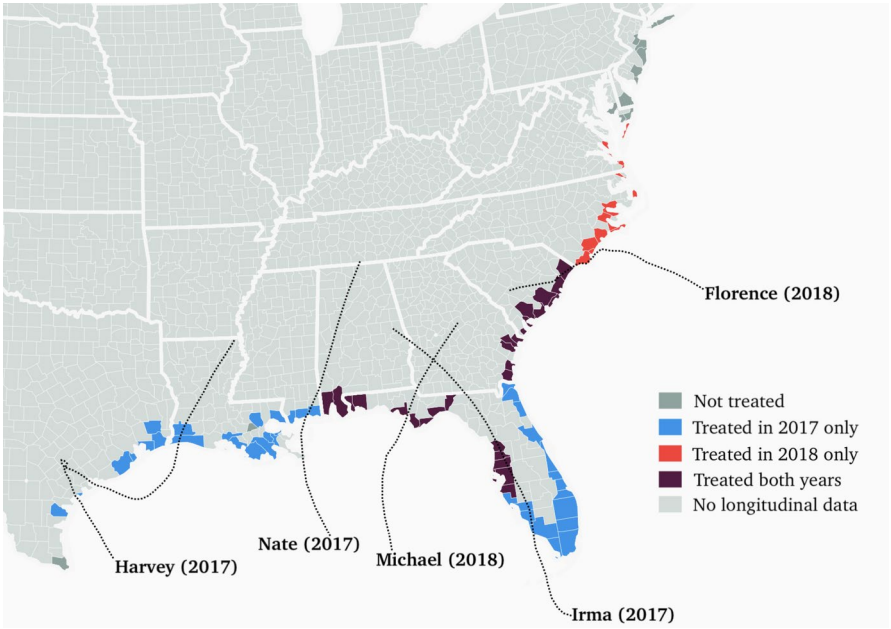

Fig. 5 Local flood treatments in 2017 and 2018 This figure displays the tracks of the five hurricanes that spurred disaster declarations in the survey region during the 2017 and 2018 seasons. Counties are also color coded according to the set of treatments they received. Counties are considered treated in a given year if a Presidential Disaster Declaration pertaining to one of the hurricanes was declared for that county in that year. Counties with no longitudinal observations are not colored, regardless of treatment status. Table 6 details the number of observations falling in each treatment category. Hurricane track data obtained from the International Best Track Archive for Climate Stewardship (Knapp et al., 2010)

Table 7 reports estimates from the models specified in Equations 4, 5, and 6, and Fig. 6 illustrates the divergence in mean flood risk perceptions between treated and untreated counties across waves. As with our cross-sectional analysis, models for insurance willingness to pay (Columns 3 and 6) are estimated with a Tobit specification to account for censoring in our multiple price list elicitation mechanism, and are restricted to homeowners only. The results are hard to reconcile with a simple model of Bayesian updating, even heavily discounted Bayesian updating in the mold of Gallagher (2014).

First, we expect significant positive coefficients on the first treatment – county flood between waves – because respondents who experience a flood event between Wave 1 and the follow-up wave will presumably be updating their risk upward over that timespan.[1] A simple Bayesian updating model would predict that anyone experiencing a flood disaster would grow more pessimistic about local flooding risk in the future. We do find statistically significant effects of flood exposure, conditional

[1] Even the most flood-prone counties in the country experience less than one flood disaster a year on average, and no county outside Louisiana experienced more than 0.52 flood disaster events per year in the 1953-2017 timespan. This means that a year with even just one flood disaster is worse than the average year for residents anywhere in the United States, and should prompt a positive update to beliefs about flood risk.

Table 6 Joint distribution of local flood exposure variables across states

	County flood event in 2018	
County flood event in 2017	**Yes**	**No**
Yes	Florida (85) Georgia (22) South Carolina (21) Texas(5) Louisiana (1)	Florida (132) Louisiana (35) Texas (26) Mississippi (9)
No	North Carolina (17) Virginia (24)	Delaware (1) Maryland (5) New Jersey (29) New York (15)

Local flood event treatments are defined at the county level based on Presidential Disaster Declarations that include references to flooding or hurricanes. However, in every case besides Florida, Texas, and Louisiana, all respondents in a given state had the same value for both treatments. States are listed along with the number of observations in the appropriate quadrant of the table. Figure 5 provides a geographic visualization of this information. A total of 63 counties in the sample were affected by at least one of three disasters declared in the summer and autumn of 2017 pertaining to Hurricanes Harvey, Irma, and Nate. A total of 41 counties in the sample were affected by at least one of the two disasters declared in the summer and autumn of 2018 pertaining to Hurricanes Florence and Michael

on observables and each respondent's previous flood risk elicitation. The coefficient from the first-differences specification (Column 1) implies that flood exposure increases average beliefs by exp(0.221) = 25%. The ordered logit specification when we control with the previous wave belief (Column 5) similarly finds a positive and significant effect of flood exposure between waves on worry (log-odds = 0.311, p = 0.048), corresponding to an average marginal effect of 5.2 percentage points on the probability of reporting worry of 6 or above. However, Panel A also indicates no robust, statistically significant effect of flood exposure between waves on willingness to pay for insurance, continuing to suggest a disconnect between updated risk perceptions and economic behavior.

The more striking finding is that residents who experienced a flood event during the 2017 hurricane season – before Wave 1 – showed greater increases in perceived likelihood of home flooding, and greater increases in worry between waves (Panel B). These results defy a basic model of Bayesian updating, which predicts that the information shock contained in the flood treatment in 2017 should be fully incorporated into beliefs during Wave 1, and beliefs about flood risk should be flat or declining in the period thereafter, barring another flood. The sharply positive effect, which is on par with the effect of a flood between periods, masks considerable variability in the changes in reported likelihood, This pattern of delayed updating – wherein a flood shock experienced before Wave 1 continues to drive belief revision between waves – is consistent with the conservatism hypothesis outlined in Section 2. Rather than fully incorporating the information shock at the time of the disaster, respondents

Table 7 Flood treatment before and between waves: main effect

		(1)	(2)	(3)	(4)	(5)	(6)
		First difference model			Control with previous wave		
		Log risk	Worry	Ins. WTP	Log risk	Worry	Ins. WTP
Panel A	Flood between waves	0.2206**	0.243	-30.45	0.2597***	0.3111**	-23.58
		(0.07746)	(0.1427)	(49.84)	(0.0758)	(0.1575)	(60.01)
Panel B	Flood year preceding Wave 1	0.2389*	0.05203	140.2	0.2345*	0.3014***	114.9
		(0.1241)	(0.1292)	(109.3)	(0.1105)	(0.08737)	(115.8)
Panel C	Flood between waves	0.2022**	0.2411	-47.09	0.2422**	0.2882*	-36.78
		(0.07507)	(0.1469)	(52.22)	(0.08281)	(0.1622)	(65.2)
	Flood year preceding Wave 1	0.2137*	0.02203	147.6	0.2043*	0.2638*	120.5
		(0.1115)	(0.1745)	(103.3)	(0.1018)	(0.1523)	(112.9)
Demographic controls		✓	✓	✓	✓	✓	✓
Observations		436	436	265	436	436	265

Each panel reports 6 models. Cols. 1–3: first-difference specifications (OLS for log risk and worry change, Tobit for WTP change). Cols. 4–6: control-with-previous-wave specifications (OLS for log risk, ordered logit for worry level, Tobit for WTP level). Standard errors clustered at state level. *** p<0.01, ** p<0.05, * p<0.1. Demographic controls include the same specification as Tables 4–5. WTP models restricted to homeowners. The dependent variable in the "first difference" models is the value of the response variable elicited in the first wave subtracted from the value of the same variable in the last follow-up wave. The dependent variable in the "control with previous wave" models is the value of the response variable elicited in the follow-up wave, while the value of the variable elicited in the first wave is included as a control variable (not reported). The right hand side of the models have the same specification as the models in Table 3, but only the main effect is reported for each. Each column contains results from three models run with three different specifications of the treatment variable

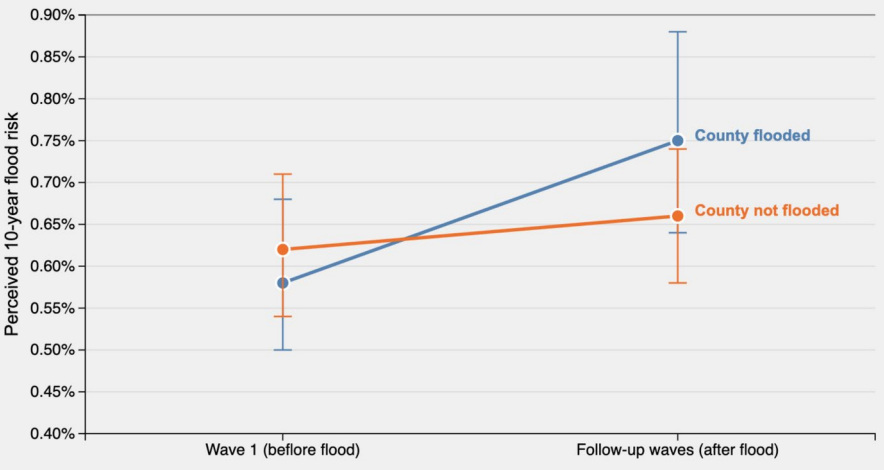

Fig. 6 Belief evolution between waves, by treatment status This figure illustrates how mean perceptions of flood risk evolve across waves in the longitudinal sample by treatment group. Respondents were considered treated if their home county was covered by a Presidential Disaster Declaration between August 2018 and November 2018

appear to update gradually, in a manner more consistent with Edwards (1968) than with standard Bayesian learning.

The results from Panel C confirm this is not a spurious effect driven by a correlation between flooding across the two years; even controlling for flood experience between waves, these results largely hold, although the simultaneous identification of two spatially correlated treatments reduces precision and statistical significance becomes more marginal for both ($p = 0.08$).

5.4 Additional robustness checks

The main findings are robust to three alternative modeling choices: substituting municipal-level flood history for county-level data, including respondents who failed attention checks, and excluding observations with imputed home elevations. Results are reported in Appendix D.

6 Conclusion

Increasing disaster risk driven by climate change highlights the need to understand how respondents update beliefs about disaster risk and pursue adaptation strategies such as insurance purchase. We analyze the results of a longitudinal survey of 124 coastal U.S. counties fielded in two waves, before and after the 2018 hurricane season, to answer this question in the context of home flooding. In order to focus on those who are most likely to be affected by hurricanes, we include coastal counties on the Atlantic seaboard and the Gulf Coast of Mexico in our survey region. The first wave of our survey was fielded on the Amazon Mechanical Turk survey platform during June and July 2018, prior to the bulk of the 2018 hurricane season, and yielded 815 validated responses. After the 2018 hurricane seasons ended, we sent follow-up surveys to our initial respondents. This second wave of surveys ran during the end of 2018 and first half of 2019 and yielded 436 validated responses.

We begin by analyzing cross-sectional data from the first wave of our survey to understand the main factors that contribute to perceived risk, willingness to pay for hypothetical insurance, and actual flood insurance take-up. We find that proximity to water has a positive and significant relationship with all three dependent variables, and home elevation has a negative and significant relationship with all three variables, in line with a basic rational expectations model of insurance demand.

In the first wave of our survey, we also conduct a randomized experiment to test how coastal residents react to government-provided flood maps. A subsample was randomly selected to visit the online flood map portal maintained by the Federal Emergency Management Agency (FEMA), find the flood maps for their neighborhoods, and receive a very brief tutorial on reading the maps. Exposure to these maps significantly reduced risk perceptions on average, but did not affect willingness to pay. Additional models with interaction terms do not yield consistent evidence of heterogeneous responses to map exposure across observable risk characteristics; these results are reported in Appendix C.

Next, we exploit the longitudinal nature of our data to measure how hurricane shocks affect perceived risk, willingness to pay, and insurance take-up over time. We do find that residents who were shocked by local floods in 2018 between survey waves, such as those in North Carolina affected by Hurricane Florence, exhibit increased risk or increased willingness to pay from one wave to the next. Given heavy hurricane activity in 2017 relative to 2018, we also focus on attenuation of risk perceptions as disasters recedes from memory, especially in areas like coastal Texas that were battered by Hurricane Harvey in 2017 but saw no follow-up hurricanes in 2018. Contrary to our expectations, we find evidence that people whose homes were flooded in the year report significantly higher perceived risk in the follow-up wave, controlling for 2018 flooding exposure.

This work contributes to the literature in three main ways. First, we are able to confirm that basic risk factors like home elevation and proximity to the water can partially explain respondents' disaster beliefs and insurance demand. Second, we are able to test the effect of exposure to flood information (separate from flood damages) utilizing FEMA's flood maps and a randomized experiment. And finally, we use novel longitudinal microdata to study the belief updating process in action, with surprising results that challenge previous understanding of updating patterns.

These findings have implications for how policymakers should try to help consumers process changes in risk, which will be important as flood risk changes rapidly in low-lying areas prone to sea level rise. The cross-sectional evidence suggests that people understand that proximity to water increases the likelihood of flooding and high elevation decreases chances of flooding, but that FIRM maps contain additional information that is not already "priced in" to consumer beliefs.

Our findings from the longitudinal study also provide behavioral insights that should be incorporated into information interventions pertaining to flood risk. As flooding risk changes rapidly in response to climate change, exposure to sporadic flooding may not cause the smooth, persistent belief updating that a simple economic model would predict. The juxtaposition between respondents' apparent ability to process risks associated with proximity and elevation, and the large and delayed swings in beliefs after hurricanes we see in the longitudinal data, suggests a complicated decision-making process that is not well-suited to climate adaptation.

Appendix A: Sample and validation

While online survey platforms present an opportunity to reach a broad population living across a wide geographic area – a key factor in this study – they also have some important limitations (Paolacci et al., 2010). One major drawback of online survey-taking is that respondents are answering questions in private and not in a supervised laboratory environment free of distractions. Respondents may be working on multiple other tasks online and off, and have monetary incentive to move quickly through the questions but little incentive to provide truthful answers. In the context of this survey, for which accurate geographic information is crucial to evaluating flood risk and for assigning the treatment variable in the case of the hurricane exposure analysis, the inability to verify respondents' location is potentially compromising.

While we discard respondents who listed non-existent ZIP codes or whose ZIP codes and municipalities were mutually inconsistent, we cannot verify that respondents are actually living in the ZIP codes and municipalities that they claim.

We perform several validation checks to ensure that our sample is not contaminated by these issues and end up excluding 53% of our original survey pool that fail one or more validation checks (see Table 8). First, we include two attention questions to test whether respondents are rushing through the survey without carefully considering the questions or their responses. Respondents must be paying attention to the survey task and the attention question specifically to record the right answer. Overall, the performance on these questions was somewhat poor, with 21% of the sample failing to correctly recall the depth of floodwaters discussed in a previous question, and 31% of the map experimental arm sample failing to correctly identify which color on the flood map zone corresponded to the highest flood risk. These respondents were excluded from the analysis. A small share of respondents who answered the attention questions correctly nonetheless completed the survey very quickly, while some also gave internally inconsistent answers on the multiple price list elicitation. We exclude these observations from all analyses, except a robustness check shown in Section 5.4. Finally, a small share (6%) of the longitudinal participants gave updated beliefs about flood risk that were wildly divergent from their previous beliefs; we exclude respondents from the longitudinal analysis if they reported a belief update of two orders of

Table 8 Problematic observations

Issue	Count	Share
Total responses collected, Wave 1	1,769	
Did not appear to live in eligible county	344	0.19
Admissible sample	1,425	0.81
Admissible sample	1,425	
Completed survey in under 5 minutes	68	0.05
Failed either attention question	407	0.29
Inverted or non-monotonic multiple price list	303	0.21
Missing or unreliable demographic data	15	0.01
Missing or unreliable home elevation data	11	0.01
Belief shift by 2+ orders of magnitude	26	0.03
None of the problems listed above	815	0.56
Validated sample	815	

Respondents with “inverted" multiple price lists reported that they were not willing to pay small amounts for the insurance contract, but were willing to pay larger amounts, apparently reversing the two responses (see Appendix E, Question 29). Respondents with non-monotonic price lists had internally inconsistent answers to Question 29 and their WTP data was discarded. See Appendix E for attention questions; Question 26 was presented to all respondents and Question 22 was presented to the map experimental group only

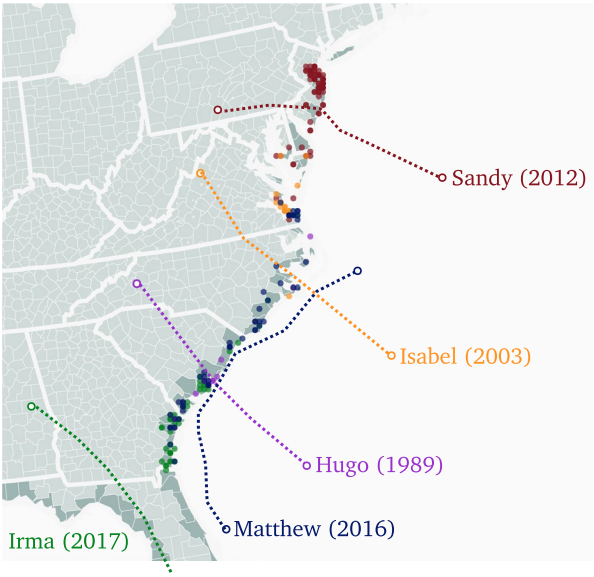

Fig. 7 Hurricane recollections. This figure displays the tracks of five major hurricanes from the past 30 years, along with the locations of respondents in seven states on the Eastern Seaboard who named one of these storms in response to a question about "the last time [your city or town] experienced a major hurricane or major flood." Respondents who listed other storms or could not remember any major hurricanes are not shown (see Appendix E, Question 35). Responses from New York (Sandy) and Florida (Irma) were nearly unanimous and are not shown. Hurricane track data obtained from the International Best Track Archive for Climate Stewardship (Knapp et al., 2010)

magnitude or greater (e.g. a ten-year flood likelihood belief of 0.1% updating to a belief of 10% or vice versa). We assume such dramatic swings indicate inattention or lack of comprehension and are artifacts of the survey process rather than genuine updating. However, one of our findings is sensitive to the inclusion of these respondents, so we include them in some analyses and discuss the reliability of the results. We do not count them as part of our main validated sample of 815 respondents who passed all validation checks (Fig. 7).

Appendix B: Survey instruments and map experiment design

To test the effect of exposure to authoritative flood risk information, we randomly assign a 25% subsample of the survey respondents to complete an additional section of the survey. Respondents in this condition were directed to find their homes on online flood insurance rate maps maintained by FEMA at `https://msc.fema.gov/portal/home` *before* they answered the key questions about perceived flood risk, flood worry, and willingness to pay for insurance. At the time the survey was fielded, the data underlying these maps was used to determine premiums for flood insurance available through the National Flood Insurance Program, although subsequent changes to the program have decoupled insurance rates from the maps (Mulder et al., 2014).

The maps available online feature two brightly-colored zones: an orange zone indicating the “500-year flood zone" corresponding to a $\geq$0.2% annual chance of inundation and a blue zone indicating the “100-year flood zone" corresponding to $\geq$1% annual risk (see Fig. 2 for an example of how these maps appear in a web browser). Respondents were given a brief primer on the flood zone definitions and were asked to look at the maps to see if their home or any homes nearby were in either the 500-year or 100-year flood zone. They were also asked to indicate qualitatively how their beliefs about flood risk changed upon seeing the map. Additional designations on the maps indicate whether certain areas are at particular risk for e.g. storm surge, but we did not make those salient to respondents. There are questions about the reliability of these government-provided flood maps, especially with respect to future flooding, due to out-of-date modeling techniques (Xian et al., 2015) and lack of projections incorporating information about climate change (Noonan et al., 2022). We do not address these questions with respondents, nor do we specifically endorse the accuracy of the flood maps (see Appendix E, Question 20 for our presentation).

We tailored the treatment as narrowly as possible to serve as a simple exposure to flood maps without any extraneous impacts. But there is always the possibility that the acts of searching for one’s flood map online through the FEMA portal and reading our brief explanation of the flood zone definitions could have had their own effects separate from the pure informational effect of viewing the map. We perform placebo tests by assessing the “effect" of map exposure on two past events that could not possibly be affected by experimental assignment at the time of the survey: whether the respondent has already purchased flood insurance, and whether she has had a past flood in her current home. Additionally, we test the effect on attitudinal questions that we hypothesize should not have been affected by exposure to flood maps. Finally, we test whether respondents anchored on the two numerical percentages specifically mentioned in the brief explanation of flood zones: 1% and 0.2% (see Appendix E, Question 20). Especially since these numbers were referring to one-year risk levels and the question asked respondents to estimate ten-year flood likelihoods, the inclusion of these numerical values might have confused respondents and induced them to think about one-year risk levels. The results are available in Table 9 and show that the map exposure treatment generally did not have unexpected consequences, except a marginally significant effect on belief that climate change is happening. The latter could be due either to random variation or possibly a genuine effect of the map exposure.

Table 9 Placebo test of flood map exposure

Variable	Control Group	Experimental Group	*p*-value (H_0: equal means)
Past events			
Carries flood insurance	0.40	0.42	0.75
	(0.02)	(0.05)	
Past flood in home	0.14	0.13	0.87
	(0.01)	(0.03)	
Attitudinal questions			
Perceived cost of flood ($ thousands)	71.9	69.1	0.86
	(12.3)	(9.5)	
Government flood support	30.0%	30.0%	0.95
	(1.0)	(1.9)	
Any memory of past local hurricane	0.85	0.84	0.82
	(0.01)	(0.03)	
Believe climate change is occurring	0.80	0.86	0.05
	(0.01)	(0.03)	
Anchoring test			
Listed ten-year flood likelihood of 1% or 0.2%	0.054	0.057	0.87
	(0.01)	(0.02)	

Sample mean values with standard errors in parentheses. Government flood support is the fraction of flood damages that respondents believe would be covered by the government (See Appendix E, Question 28). A two-sample *t*-test of the equality of means with unequal variances was performed on each variable, and the *p*-value for each test is listed in the right-most column. See Section 3.3 for an explanation of these tests

Appendix C: Heterogeneity analysis

Table 10 reports specifications augmented with interaction terms between the randomized map exposure treatment and the hydrological characteristics of respondents' homes, as detailed in Eq. 3 of the main text. These models have no geographic fixed effects, as county fixed effects would absorb the within-county variation in home elevation and distance to water that is essential for identifying the interaction effects. The main effects in the upper panel are broadly consistent with those reported in Tables 4 and 5.

The interaction of primary theoretical interest is between county flood history and map exposure. We hypothesize that people living in areas with more frequent floods will update relatively more negatively upon seeing authoritative information about flood risk. In our preferred ordered logit specification, this interaction is indistinguishable from zero across all three outcomes. An alternative specification estimating the worry equation via OLS yields a marginally significant interaction for this term. Given the ordinal nature of the worry outcome, we treat the ordered logit as the more reliable specification. We find no evidence that respondents in counties with greater historical flood experience respond differentially to map exposure – a null result that argues against a pure representativeness or recency bias account in which salient local flood history should amplify updating in response to new information.

Among the remaining interaction terms, the only statistically significant result is between home elevation and reported worry, indicating that lower-elevation respondents update their worry upward more – or downward less – upon viewing the maps

Table 10 Interaction between map exposure and hydrological characteristics

	(1) Perceived flood likelihood (log)	(2) Worry about floods	(3) WTP for flood insurance
Main effects			
Randomized map exposure	-1.03**	-0.08862	-10.45
	(0.4551)	(0.5957)	(312.2)
Home elevation (ft)	-0.00587***	-0.008585***	-1.39*
	(0.001652)	(0.001807)	(0.7703)
<20 min from water	0.7332***	0.8175***	38.17
	(0.192)	(0.142)	(95.7)
<5 min from water	-0.0769	-0.186	155.5
	(0.278)	(0.1695)	(97.71)
County floods, 1953–2017	-0.01867	0.02029	-7.081
	(0.01193)	(0.01396)	(4.345)
Past flood in home	2.07***	1.776***	126.9
	(0.1923)	(0.274)	(112.2)
Interaction effects			
Home elevation × Map	-0.00583	-0.009809**	-3.2
	(0.005059)	(0.004003)	(2.026)
<20 min from water × Map	-0.07586	-0.5133	-188.5
	(0.4768)	(0.3855)	(181.4)
<5 min from water × Map	-0.1664	0.7115	-26.05
	(0.4953)	(0.4505)	(200)
County floods, 1953–2017 × Map	0.03593	0.00189	7.334
	(0.02417)	(0.02146)	(10.52)
Past flood in home × Map	-0.4266	-0.07435	-178.3
	(0.6014)	(0.2813)	(137.7)
Govt. flood support × Map	0.09259	-0.1102	-47.47
	(0.3503)	(0.2625)	(46.75)
Demographic controls	✓	✓	✓
Observations	815	815	449
Adj. R^2 / Pseudo R^2	0.172		0.0147

Specifications 1: OLS estimates; specifications 2: ordered logit estimates; specifications 3: Tobit estimates with censoring at $0 and $6,000, all with standard errors clustered at the state level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. The coefficients displayed for specification 2 are log-odds ratios. Log risk is the natural log of the respondent's estimated 10-year risk of a home flood. Worry is reported on a 1-10 scale, with greater values indicating greater worry. Willingness to pay for flood insurance is expressed in terms of annual premia for a $100,000 policy, and those models are estimated on the subsample of respondents who are homeowners. Home elevation values imputed for some respondents based on municipality or ZIP code. Home tenure defined as number of years at current residence. Govt. flood support is a standardized measure of beliefs about what share of hypothetical flood damages would be covered by government aid. Demographic controls (not shown) comprise categorical bins for age, race, income, educational attainment, home type, and risk preference. No geographic fixed effects are included in these models

relative to higher-elevation respondents. Following Ai and Norton (2003), who note that interaction effects in nonlinear models cannot be evaluated from the sign and significance of the interaction term alone, we compute average marginal effects of the interaction. The contrast in marginal effects between map-exposed and unexposed respondents is statistically indistinguishable from zero ($p = 0.887$), indicating that the significant log-odds interaction term does not translate into a significant differential effect on the probability scale. We therefore treat this as a null result.

Appendix D: Robustness checks

To assess the robustness of our findings, we perform checks using alternative data sources and sample restrictions. In general, the main findings hold under these alternative modeling choices.

Our first alternative modeling choice is including respondents who failed attention check questions from our survey. We include two attention questions to test whether respondents are rushing through the survey without carefully considering the questions or their responses (see Appendix E, Questions 26 and 22). A relatively large number of respondents failed at least one attention question, indicating that many respondents were not fully invested in answering the questions with care. This might simply add noise to the data, but it could also create artificial patterns or correlations if certain respondents always select the first answer in a multiple choice list, for example. When we include the full sample and rerun the models displayed in Tables 5 and 7 measuring the main effects of exposure to the FIRM maps and exposure to local flood events, the results are unchanged (available upon request).

Additionally, we test versions of our model where respondents with imputed home elevations are excluded. Each respondent was asked to provide her home's elevation in feet, either from memory or by entering her address into a third-party service that provides elevation information (See Appendix E, Question 32). Several of the provided answers were deemed unreliable because they exceeded the maximum elevation of their respective counties according to recreational mountain climbing website www.peakbagger.com. In cases where listed home elevations exceeded county high points, we imputed elevation by first using the Google Earth API to find latitude and longitude coordinates for each respondent's ZIP code, and then finding the elevation at that point.

Because elevation can vary significantly even within a single ZIP code, and because even a difference of a few feet in elevation can make a dramatic difference in flood risk near the shore, this imputation process potentially introduces significant measurement error. To assess this possibility, we rerun the models displayed in Tables 4, 5, 6, 7, 8, 9 and 10 without any of the observations affected by the imputation procedure. The results are shown in Table 11 and largely match the main results, with the exception that the relationship between home elevation and willingness to pay for insurance becomes more statistically marginal.

Finally, Table 12 replicates the models from Table 7 while including the 12 longitudinal respondents excluded from the main sample due to extreme belief shifts of two or more orders of magnitude. The Panel B result – greater increases in worry among those who experienced a flood before Wave 1 – is substantially stronger in this sample, with larger and more precisely estimated coefficients. The Panel A result is attenuated and loses significance, suggesting it is sensitive to the inclusion of extreme updaters.

Table 11 Determinants with no imputed elevation data

				County / state fixed effects		
	(1) Log risk	(2) Worry	(3) Ins. WTP	(4) Log risk	(5) Worry	(6) Ins. WTP
Home elevation (ft)	-0.007904***	-0.009852***	-1.66*	-0.007505***	-0.008374***	-0.9289
	(0.0009817)	(0.001572)	(0.8517)	(0.002351)	(0.001458)	(1.403)
<20 min walk to water	0.6874***	0.792***	45.01	0.8353***	0.8826***	-64.01
	(0.152)	(0.2042)	(100.1)	(0.2072)	(0.2356)	(108)
<5 min walk to water	-0.2208	-0.2546	52.87	-0.469**	-0.205	194***
	(0.2406)	(0.1725)	(76.37)	(0.205)	(0.1568)	(72.33)
County floods, 1953–2017	0.001051	0.03062**	-1.783		-0.02276	
	(0.01122)	(0.01464)	(3.115)		(0.0372)	
Past flood in home	2.023***	1.748***	-22.09	1.817***	1.573***	-94.51
	(0.2152)	(0.2338)	(103)	(0.219)	(0.1954)	(119)
Climate change believer	0.1817	0.5324***	70.75	0.2786	0.4953***	189.2*
	(0.1234)	(0.1392)	(107.5)	(0.1645)	(0.1552)	(101.5)
Female	-0.07832	0.3894**	165.5**	-0.02078	0.4036**	174**
	(0.2234)	(0.167)	(69.81)	(0.1512)	(0.1662)	(84.35)
Home tenure	-0.02466***	-0.02629***	-1.338	-0.02469***	-0.02449***	-1.745
	(0.007506)	(0.009446)	(4.843)	(0.006518)	(0.009005)	(5.507)
Homeowner indicator	-0.5346	0.06008	0	-0.5532	0.03261	0
	(0.3421)	(0.2971)	(0)	(0.3109)	(0.3111)	(0)
Democrat	0.004179	-0.006227	33.08	-0.24*	-0.00866	-45.16
	(0.08823)	(0.116)	(82.29)	(0.1297)	(0.1386)	(81.63)
Govt. flood support	0.01898	0.06825	32.57	-0.03567	0.07429	37.42
	(0.06023)	(0.05774)	(22.7)	(0.06038)	(0.05136)	(28.01)
Has flood insurance			471.4***			516.6***
			(58.42)			(77.49)
Demographic controls	✓	✓	✓	✓	✓	✓
County/state FE				✓	✓(state)	✓
Observations	659	659	359	659	659	359
Adj. R^2 / Pseudo R^2	0.172		0.0144	0.167		0.0316

Robustness check for Table 4 excluding the 22 observations whose home elevation values were imputed from municipality or ZIP code centroid rather than from the respondent's address. Cols. 1–3: OLS (log risk), ordered logit (worry), Tobit (WTP), no fixed effects. Cols. 4–6: xtreg within-estimator (log risk), ordered logit with state fixed effects (worry), Tobit with county fixed effects (WTP). Standard errors clustered at state level. *** p<0.01, ** p<0.05, * p<0.1. Flood insurance coverage included in WTP models only. WTP models restricted to homeowners

Appendix E: survey questionnaire

The questions included in Wave 1 of the online survey are reproduced below. Some graphics and visual slider elements are omitted.

1) Demographics and location Location. Enter your home municipality. The municipality is the town/city where you live.
2) Location. Enter your home ZIP code. You must enter a valid 5-digit U.S. ZIP code.
3) Gender. What is your gender?
4) Age. How old are you?

- 18 - 24
- 25 - 34
- 35 - 44

Table 12 Flood treatment effects: full longitudinal sample (outliers included)

		(1)	(2)	(3)	(4)	(5)	(6)
		First difference model			Control with previous wave		
		Log risk	Worry	Ins. WTP	Log risk	Worry	Ins. WTP
Panel A	Flood between waves	-0.01297	0.06602	-46.87	0.1115	0.0908	-40.29
		(0.1178)	(0.1451)	(58.97)	(0.0907)	(0.1393)	(65.84)
Panel B	Flood year preceding Wave 1	0.5286***	0.2102	194.4*	0.4209***	0.4173***	165.1
		(0.06346)	(0.1518)	(108.4)	(0.07885)	(0.1202)	(109.1)
Panel C	Flood between waves	-0.05209	0.053	-62.88	0.0806	0.06083	-53.32
		(0.09203)	(0.1444)	(61.89)	(0.08895)	(0.1393)	(71.82)
	Flood year preceding Wave 1	0.5341***	0.2052	201.5**	0.4121***	0.4105***	170.9*
		(0.06209)	(0.1664)	(99.47)	(0.08127)	(0.1362)	(103.1)
Demographic controls		✓	✓	✓	✓	✓	✓
Observations		448	448	282	448	448	282

Each panel reports 6 models. Cols. 1–3: first-difference specifications (OLS for log risk and worry change, Tobit for WTP change). Cols. 4–6: control-with-previous-wave specifications (OLS for log risk, ordered logit for worry level, Tobit for WTP level). Standard errors clustered at state level. *** p<0.01, ** p<0.05, * p<0.1. Demographic controls include the same specification as Tables 4–5. WTP models restricted to homeowners. The dependent variable in the "first difference" models is the value of the response variable elicited in the first wave subtracted from the value of the same variable in the last follow-up wave. The dependent variable in the "control with previous wave" models is the value of the response variable elicited in the follow-up wave, while the value of the variable elicited in the first wave is included as a control variable (not reported). The right hand side of the models have the same specification as the models in Table 3, but only the main effect is reported for each. Each column contains results from three models run with three different specifications of the treatment variable

- 45 - 54
- 55 - 64
- 65 - 74
- 75 - 84
- 85 or older

5) Income. What is your total annual household income?

- Less than $15,000
- $15,000 - $29,999
- $30,000-$44,999
- $45,000-$59,999
- $60,000-$74,999
- $75,000-$99,999
- $100,000-$149,999
- $150,000-$199,999
- Greater than or equal to $200,000

6) Ethnicity. What is your ethnicity or race?

- Caucasian/White

- Hispanic or Latino
- African American
- Asian/Pacific Islander
- Native American/American Indian
- Other

7) Education. What is the highest degree or level of schooling you have completed?

- 8th grade or less
- Some High School
- High School Graduate or GED
- Some College
- Trade/Technical/Vocational Training
- Associates Degree
- Bachelor's Degree
- Some Graduate School
- Graduate Degree

8) Political Affiliation. What is your political affiliation?

- Democrat
- Independent
- Republican
- None
- Other

9) Homeownership status. Which of the following best describes you?

- I own my primary residence.
- I do not own my primary residence, and pay rent to a landlord or property owner.
- Other

10) Home type. What type of structure is your home?

- Detached, single-family home
- Townhouse or row house
- Condo, apartment, or multiresidence
- Mobile home
- Other

11) How many years have you lived in your current residence?

12) Risk preferences Risk attitudes. Suppose you must choose one of the fol lowing games to play. In each game, a fair coin is flipped, with a 50% chance of coming up heads and a 50% chance of coming up tails. You earn a prize that depends on which side comes up. Which of the following games would you most like to play?

- Heads: you get $70 / Tails: you get $2
- Heads: $60 / Tails: $12
- Heads: $52 / Tails: $16
- Heads: $44 / Tails: $20
- Heads: $36 / Tails: $24
- Heads: $28 / Tails: $28

13) Gambling. Do you bet on lotteries, casinos, sporting events, or horse races?

- Don't gamble at all
- Used to gamble, but quit gambling now
- Hardly gamble
- Several times a year or so
- Once a month or so
- Once a week or so
- Almost every day

14) Which of the following hypothetical payment options would you prefer? Indicate a preference for each pair of options.

- A) $100 one month from now vs. B) $105 one year from now
- A) $100 one month from now vs. B) $110 one year from now
- A) $100 one month from now vs. B) $125 one year from now
- A) $100 one month from now vs. B) $150 one year from now

15) How worried are you about the risk of an asteroid impact from outer space that destroys your city or town over the next 10 years, again on a scale of 1 to 10 where "1" means "not worried at all" and "10" means "very worried"? [slider, 1 - 10]

16) Situation of your home How close do you live to a body of water that could flood your home? This would include the ocean, a lake, or a river.

- Walking distance (could walk to the water in less than 20 minutes)
- Driving distance

17) How long would it take you to walk to the closest body of water? [slider, 0 - 20 minutes]
18) How long would it take you to drive to the closest body of water? [slider, 0 - 120 minutes]

19) Do you think your home is at more or less risk of flooding than other homes nearby?

- More risk
- About the same
- Less risk

20) Flood maps [this section assigned randomly to one quarter of the sample and not repeated in Waves 2 or 3] Please follow the steps below.

- Access the FEMA Flood Map here [https://msc.fema.gov/portal/search]. The link will open in a new window or tab in your web browser so that the survey will not be interrupted.
- Enter your exact street address with city and zip code in the box provided (see example below). Note: this information is being entered on an external website operated by the Federal Emergency Management Agency and will not be accessible to the researchers administering this study.
- Find your home on the map. It is recommended that you zoom out to get a better view of your neighborhood.
- See below for an example flood hazard map of downtown Newport, Rhode Island. Note the orange and blue zones near the coast. These zones indicate increased flood risk, and are more likely to appear near rivers, beaches, and other areas that are vulnerable to flood. Look to see if there are orange and blue zones on the map near your home. Your neighborhood may or may not have any orange or blue zones, depending on the flood risk in your area.
- The blue zone on the flood hazard map is called the "100-year flood zone." The U.S. Army Corps of Engineers assesses flood risk at greater than 1% per year in this area. The blue areas on the map will be flooded approximately once every century. The orange zone on the flood hazard map is called the "500-year flood zone." The U.S. Army Corps of Engineers assesses flood risk at greater than 0.2% per year in this area. The orange areas on the map will be flooded approximately once every five centuries.

21) item Flood risk. Which of the following is true of your home, according to the map?

- My home is in the 100-year flood zone (blue zone)
- My home is in the 500-year flood zone (orange zone)
- My home is not in either flood zone, but other parts of my neighborhood are
- I don't see any flood zones in my neighborhood

22) Which of the two color zones discussed on the previous page has a higher risk of flood?

- Green zone
- Blue zone

- Red zone
- Orange zone

23) Your reaction. Which of the following best describes your reaction to seeing the flood map for your area?

- My home's flood risk is a lot higher than I thought
- My home's flood risk is a bit higher than I thought
- My home's flood risk is about what I thought
- My home's flood risk is a bit less than I thought
- My home's flood risk is a lot less than I thought

24) Flood insurance perceptions and demand How worried are you about risk of your home flooding over the next 10 years, on a scale of 1 to 10? [slider, 1-10]
25) Chances of a flood in your home. Consider your residence and the surrounding area. What do you think are the chances of your home being flooded to a depth of at least one inch at any point in the next ten years? Note: do not consider internal water damage from e.g. leaky pipes. [After answering this question, respondents were presented with sliders that allowed them to pick an exact value.]

- About one in a thousand (0.1%) or less
- More than one in a thousand chance but less than one in a hundred chance
- About one in a hundred (1%)
- More than one in a hundred but less than one in ten chance
- About one in ten (10%) or more

26) We asked you to consider the chances of your home flooding to at least what depth?

- One inch
- Six inches
- One foot
- Five feet

27) Cost of a flood. Now, imagine that your home floods to a depth of one foot. How much do you think it would cost to repair the damage to the building and replace belongings that are destroyed, in dollars? Consider how much it would cost in total, even if you couldn't afford to pay the entire amount or if some of the damage would be covered by insurance.
28) Of the total damages, about what percentage would you expect to be paid back through public assistance from the government? [slider, 0% - 100%]
29) Flood insurance. Assume you are offered a flood insurance policy that will cover you for the next year. It will cover any damages to your home caused by external flooding up to $100,000. Consider how much you would be willing to pay per month

or per year for that insurance policy, compared to going without insurance. For each hypothetical premium below, indicate whether you think it would be worth it to buy flood insurance at that price or not. Note: if you are not a homeowner, imagine that you own the residence where you currently live and must make the purchase decision for that residence.

- monthly premium $5 | annual premium $60
- monthly premium $10 | annual premium $120
- monthly premium $15 | annual premium $180
- monthly premium $20 | annual premium $240
- monthly premium $25 | annual premium $300
- monthly premium $30 | annual premium $360
- monthly premium $40 | annual premium $480
- monthly premium $50 | annual premium $600
- monthly premium $75 | annual premium $900
- monthly premium $100 | annual premium $1,200
- monthly premium $125 | annual premium $1,500
- monthly premium $150 | annual premium $1,800
- monthly premium $200 | annual premium $2,400
- monthly premium $300 | annual premium $3,600
- monthly premium $400 | annual premium $4,800
- monthly premium $500 | annual premium $6,000

30) If you own your home, do you currently have a flood insurance policy covering your home? Keep in mind that standard homeowner's insurance policies do not include flood insurance and it must be purchased separately.

- Yes
- No
- Unsure
- I am not a homeowner/not applicable

31) Suppose your city or town is struck by a major hurricane or flood in 2018 that causes widespread devastation. If that happens, would you think another hurricane or flood is more likely or less likely to come in 2019?

- More likely
- About the same
- Less likely

32) Memory of past floods What is the elevation of your home in feet? You can check by entering your exact street address here: https://www.whatismyelevation.com/

33) Have you experienced a flood at your current residence where water entered your home and/or damaged your property?

34) What year did your home flood? Enter only the most recent year if you have experienced multiple floods.
35) Try to remember the last time the town or region where you live experienced a major hurricane or major flood. This may have occurred before you moved to the area or even before you were born.

- I can remember, or have heard about, the last major hurricane or flood in my area
- I don't know of any major hurricanes or floods that have ever struck my area

36) If applicable, what was the name of the hurricane or flood that hit your region?
37) Enter the approximate year of the most recent hurricane or flood that hit your area.

38) Attitudes about climate change In your view, the risk of your home flooding twenty years in the future will be:

- Much greater than today
- Somewhat greater
- About the same
- somewhat less
- Much less than today

39) In your own words, why did you answer the above question the way you did?
40) At this point we'd also like to ask you about your opinions on climate change. Do you believe that the global climate is changing?

- Yes, it is changing
- Unsure
- No, it is not changing

41) To the best of your knowledge, how much have global temperatures increased in the last 200 years? Enter the number of degrees Fahrenheit.
42) What are your reasons for completing this survey?

Questions included only in the follow-up waves of the survey (Waves 2 and 3) are reproduced below.

1) Relocated to new residence Have you moved to a new residence since you filled out the first round of this survey in June/July 2018? What was the reason for your move to a new residence since July 2018?
2) In a few words, briefly explain why you moved:
3) Homeownership status. Which of the following best describes you?

- I own my primary residence.
- I do not own my primary residence, and pay rent to a landlord or property owner.
- Other

4) Home type. What type of structure is your new home?

- Detached, single-family home
- Townhouse or row house
- Condo, apartment, or multiresidence
- Mobile home
- Other

5) How close do you live to a body of water that could flood your new home? This would include the ocean, a lake, or a river.
6) How long would it take you to walk to the closest body of water?
7) How long would it take you to drive to the closest body of water?
8) "What is the elevation of your new home in feet? You can check by entering your exact street address here: https://www.whatismyelevation.com/

9) Personal flooding event between waves Did your previous home flood in the time since June/July 2018 when you completed the first round of this survey?
10) How deep was the water in your home, in feet? - water depth (feet)
11) What was the approximate cost of the damage to your home, in dollars? Please include all costs, regardless of whether they were covered by insurance and regardless of whether you paid to actually repair the damage.
12) Was your home flooding associated with Hurricane Florence, which made landfall in North Carolina on September 14, 2018?
13) Was the previous town or city where you lived in June/July 2018 impacted by Hurricane Florence, which made landfall in North Carolina on September 14, 2018?
14) In a few words, explain how Hurricane Florence affected your previous town or city.

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Data Availability Anonymized data and aggregated data sufficient to replicate the results can be shared, but raw response data indicating respondents' geographic locations cannot be shared under the terms of IRB approval.

Declarations

Competing interests The authors declare no competing interests.

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