NBER WORKING PAPER SERIES

BROAD BRACKETING FOR LOW PROBABILITY EVENTS

Shereen J. Chaudhry
Michael Hand
Howard Kunreuther

Working Paper 27319
http://www.nber.org/papers/w27319

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2020

Support for this research comes from a grant from the Sloan Foundation (G-2018-11100 / SUB18-04); a National Science Foundation (NSF) grant (EAR-1520683 / SUB0000091) through Princeton University; the Geneva Association; the Travelers Risk Management and Leadership project; and the Wharton Risk Management and Decision Processes Center's Extreme Events project. We wish to express gratitude to Allyson Root, Rick Larrick, and Ray Sin for feedback on the experimental design and/or earlier drafts of this paper. We appreciate helpful discussions with our colleagues at the Wharton Risk Center throughout the development of this paper: Jeff Czajkowski, Carol Heller, Carolyn Kousky, Brett Lingle, Robert Meyer, Marilyn Montgomery, and Gina Tonn. We would also like to thank Nyzinga Patterson for administrative support in running the experiments. We appreciate additional feedback from participants at the 2018 SABE sessions at the WEAI conference and the 2018 meeting of the Society for Judgment and Decision Making. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

Broad Bracketing for Low Probability Events
Shereen J. Chaudhry, Michael Hand, and Howard Kunreuther
NBER Working Paper No. 27319
June 2020
JEL No. D01,D81,D91

ABSTRACT

Individuals tend to underprepare for rare, catastrophic events because of biases in risk perception. A simple form of broad bracketing—presenting the cumulative probability of loss over a longer time horizon—has the potential to alleviate these barriers to risk perception and increase protective actions such as purchasing flood insurance. However, it is an open question whether broad bracketing effects last over time: There is evidence that descriptive probability information is ignored when decisions are made from "experience" (repeatedly and in the face of feedback), which describes many protective decisions. Across six incentive-compatible experiments with high stakes, we find that the broad bracketing effect does not disappear or change size when decisions are made from experience. We also advance our understanding of the mechanisms underlying broad bracketing, finding that, while cumulative probability size is a strong driver of the effect, this is dampened for larger brackets which lead people to be less sensitive to probability size.

Shereen J. Chaudhry
University of Chicago
Booth School of Business
5807 S Woodlawn Ave
Chicago, IL 60637
USA
shereen.chaudhry@chicagobooth.edu

Michael Hand
U.S. General Services Administration
Office of Evaluation Sciences
1800 F St NW
Washington, DC 20405
USA
michaels.hand@gsa.gov

Howard Kunreuther
Wharton Risk Management
and Decision Processes Center
The Wharton School
University of Pennsylvania
3819 Chestnut Street, Suite 330
Philadelphia, PA 19104-6366
and NBER
kunreuth@wharton.upenn.edu

1. Introduction

Companies and individuals often fail to protect themselves against rare, catastrophic events, such as flooding form natural disasters. Protective decisions can generally be thought of as choices that present two options, both with negative expected value: incur some cost for sure (e.g., purchasing flood insurance) or take a *chance* of incurring a much larger cost than the sure cost (e.g., a flood occurs causing one to pay out of pocket to repair flood damage to their house). In flood-prone areas in the United States, relatively few individuals have voluntarily taken the protective action of purchasing insurance, opting to take their chances with flood to their detriment. For example, the Federal Emergency Management Agency (FEMA) estimated that only 17 percent of residents most affected by Hurricane Harvey had flood insurance (Long, 2017), and as a result, the U.S. federal government paid out over $1.2 billion dollars in housing assistance (FEMA, 2018). Thus, not taking protective actions harms flood victims and creates substantial negative externalities for governments.

Individuals' failure to engage in protective behaviors against rare catastrophes like flood likely stems from a combination of two judgment processes that defy the assumptions of expected utility theory. First, people often consider small probability events too rare to pay attention to, or below their *threshold level of concern* (Kunreuther, 1996; Robinson & Wouter Botzen, 2018; Slovic et al., 1977). Though in many cases people overweight small probabilities as suggested by prospect theory (Kahneman & Tversky, 1979), in situations like natural disasters, where people receive feedback over time and update their beliefs about likelihoods of occurrence, people tend to *underweight* small probabilities (Barron & Erev, 2003; Hertwig, Barron, Weber, & Erev, 2004; Weber, Shafir, & Blais, 2004). Because the probability of events like natural disasters are often communicated in one-year increments (e.g., 1% annual chance), this may lead people to take their chances with the rare event rather than incur the upfront cost of protection without really considering what is optimal given their risk preferences.

Second, people exhibit myopia, or *narrow bracketing*, in their judgments and decisions—that is, people tend to focus on short time periods rather than acknowledging long-term exposure (Kahneman & Lovallo, 1993; Read, Loewenstein, & Rabin, 1999; Redelmeier & Tversky, 1992). Thus, a homeowner may consider the annual 1% risk of a flood damage to their house to be too small to care about and not think about the fact that, because they plan on living in their house for 10 years, the risk they face is actually a 10% chance of one or more floods over that period. Furthermore, even when people do consider the long-term, they tend to underestimate the cumulative effect of repeated exposure to risk (Doyle, 1997; Fuller, Dudley, & Blacktop, 2004; Keller, Siegrist, & Gutscher, 2006; Knäuper, Kornik, Atkinson, Guberman, & Aydin, 2005; Linville, Fischer, & Fischhoff, 2015; Shaklee & Fischhoff, 1990; Slovic, Fischhoff, & Lichtenstein, 1978).

Previous research suggests that directly countering the narrow bracketing tendency by using *broad bracketing* might lead people to behave differently and to opt more often for the protective action. Broad bracketing involves conveying cumulative information about the distribution of possible outcomes of a gamble over a certain *broad* period of time. For example, when converted to a 50-year bracket, a 1% annual chance of flood translates to the following: 60% chance of no flood, 31% chance of one flood, 8% chance of two floods, 1% chance of three or more floods. Researchers found that providing the broad bracket distribution for choices between a sure loss (-$4) and a pure loss gamble played 50 times (-$0.10, 90%; -$0.50, 10%) led people to avoid the gamble and choose the sure loss more often (Webb & Shu, 2017). This setup is analogous to protective decisions and so we will use the terms *protective decisions* and *pure loss choices* interchangeably, as well as the terms *protective action* and *sure loss*.

We suggest that applying broad bracketing to the context of protective decisions will lead more people to opt for the protective action. In this investigation, we implement and test whether this effect holds for a simpler, more scalable version of broad bracketing: We present only the *cumulative loss probability*, or the cumulative probability of one or more losses over a given time horizon (e.g., 39% chance of one or more floods over 50 years). Two studies have examined the effect of such cumulative loss probabilities on risk perceptions (De La Maza, Davis, Gonzalez, & Azevedo, 2019; Keller et al., 2006), but no prior work has examined the impact on actual behavior (for a review see Visschers, Meertens, Passchier, & de Vries, 2009).

Central to our investigation is addressing the following concern: While the evidence from Webb and Shu (2017) suggests that broad bracketing will result in increased protective action, there is evidence that its effect will diminish over time due to people's experiences. Experience is a key feature of protective decisions, which often must be made monthly or annually and after learning whether the event has occurred or not (e.g., annually purchasing flood insurance, regularly clearing drains or checking that one's sump pump is operational). Prior to a disaster, individuals ignore the possibility (optimism), then reverse and pay it heightened attention after the disaster occurs (availability), and finally, after time passes without another loss, they forget the impact the disaster had and revert to ignoring its possibility again (amnesia; Meyer & Kunreuther, 2017). Research has shown that, even if people learn objective probability information about disasters (i.e., *descriptive* information), they begin to ignore this descriptive information (which the broad bracket is) in favor of information based on their own experience (Jessup, Bishara, & Busemeyer, 2008; Lejarraga & Gonzalez, 2011; Newell, Rakow, Yechiam, & Sambur, 2016; Rakow, Demes, & Newell, 2008; Yechiam & Busemeyer, 2006).

Neither the Webb and Shu article nor other research on broad bracketing (Benartzi & Thaler, 1999; Hardin & Looney, 2012; Looney & Hardin, 2009; Thaler, Tversky, Kahneman, & Schwartz, 1997) has demonstrated that broad bracketing would be robust to experience. We are the first to examine the role of experience and test whether broad bracketing can shift people strongly and consistently toward protective behavior when decisions are made repeatedly in the face of feedback. The results have implications for whether or not broad bracketing could serve as a useful tool for risk communication regarding protective decisions.

Past research has also left unknown the mechanism underlying broad bracketing by always confounding two features in the broad bracket manipulation: probability size and time horizon length. As a result, it is unclear whether people only respond to the larger probabilities or whether they also consider the time horizon over which the probabilities are calculated. We expect that, while larger probabilities will lead more people to attend to and respond to the broad bracket, longer time horizons will moderate that response. For example, we expect that presenting people with a 10% chance of flood over 10 years will lead to more protective behavior than presenting them with a 10% chance over 200 years. Utilizing our more scalable version of broad bracketing allows us to isolate the key attributes of the framing technique (probability size and time horizon length) in a way that has not been done before and uncover the separate roles played by these two components in the broad bracket effect. These findings provide insight into the judgment processes underlying broad bracketing and serve as guidance for how to structure a broad bracket intervention for risk communication.

In the next section, we review previous work on risk perception and broad bracketing and discuss how the present work contributes. In Section 3, we provide an overview of the experiments. In Sections 4-9, we describe the rationale, methods, and results for Studies 1-6. In Section 10, we discuss the implications of our findings and suggest future directions.

2. Risk perception and decisions from experience

People tend to underweight small probabilities when learning about probabilities *through experience* rather than through explicit presentation or description (Barron & Erev, 2003; Hertwig et al., 2004; Weber et al., 2004). People also sometimes engage in an editing process wherein they ignore events with probabilities below their threshold level of concern (Kunreuther, 1996; Robinson & Wouter Botzen, 2018; Slovic et al., 1977). Because individuals often assess the probability of an event, like a natural disaster, using their own experiences and because these events tend to be rare, it is likely that these facts helps explain why people choose not prepare for future disasters (Kunreuther et al., 1978; McClelland, Schulze, & Coursey, 1993; Meyer & Kunreuther, 2017). The upfront cost may not seem worth it given the infrequent observation of occurrence.

Even if a person faces a one-time risk that is very small, the chances of a negative event occurring increases with repeated exposure to that risk. In an extreme example, a 1% annual chance of flood translates to an approximately 0.0027% daily chance of flood. On any given day the miniscule chance of flood likely falls below the threshold of concern for most people. But a 1% annual chance of flood also translates to a 26% chance of at least one flood over 30 years, the term of a typical mortgage. If the broad bracket (i.e., 30-year time horizon) more closely aligns with how people make residential location and home purchase decisions than the narrow bracket (i.e., one year or one day), then the broad-bracket probability information may be more relevant for aligning protective actions to risk preferences.

Evidence suggests people tend to narrowly bracket their judgments and decisions, thinking only about the near term (Kahneman & Lovallo, 1993; Read et al., 1999; Redelmeier & Tversky, 1992). This myopic behavior is reinforced by the external choice context, which often frames risks over a small time period. For example, flood risks are normally presented in terms of the annual chance of a disaster, rather than a time horizon that is likely to be relevant for residential location decisions.

People also underappreciate how risks accumulate over time because of the complexity of the calculation (Doyle, 1997; Fuller et al., 2004; Keller et al., 2006; Knäuper et al., 2005; Linville et al., 2015; Shaklee & Fischhoff, 1990; Slovic et al., 1978). Thus, even when asked to consider a larger bracket, unless people are given explicit probabilities, they tend to underestimate how much aggregation over time or repeated exposure affects the probabilities (Redelmeier & Tversky, 1992).

2.1 Broad bracketing and cumulative probabilities

Given people's tendencies to underweight low probability events, to narrowly bracket their decisions, and to underappreciate the aggregation of risk over time, explicitly presenting cumulative probabilities over a longer span of time—such as broad bracketing—could help people consider rare, catastrophic events as within their threshold level of concern. Broadly bracketed information about the risk of a catastrophic loss may reduce risk seeking (preference for the pure loss gamble) and encourage protective actions (preference for the sure loss) like purchasing insurance. While previous work on broad bracketing involved presenting the full distribution of outcomes, we present only a single number representing the probability of *at least* one loss within that time, i.e., the *cumulative loss probability*, which is most relevant for considering losses from natural disasters. The risk perception literature has documented people's difficulty in recognizing and understanding cumulative risk (Doyle, 1997; Fuller et al., 2004; Keller et al., 2006; Knäuper et al., 2005; Linville et al., 2015; Shaklee & Fischhoff, 1990; Slovic et al., 1978), and two papers have examined the effect of cumulative loss probabilities on judgments (De La Maza et al., 2019; Keller et al., 2006), but no work has examined how explicitly presenting cumulative risk can be helpful in changing *behavior*.

Hypothesis 1 (H1). *Compared to narrow bracketing, broad bracketing operationalized as the "cumulative loss probability" will increase the likelihood that people will choose a sure loss over a pure loss gamble.*

2.2 Broad bracketing in the face of repetition and feedback

Critically, many real-world decisions, especially those involving protective actions, require people to make decisions *repeatedly.* In other words, the decisions cannot be locked in for the period of the broad bracket and, furthermore, the decision maker cannot avoid receiving *feedback* over time. For instance, homeowners normally decide annually whether to purchase a one-year flood insurance policy, even if they plan to live in their homes for several years. This decision is then followed by feedback about whether or not a flood has actually occurred during that year.

Because evidence suggests that broad bracketing will *not* hold up in the context of experience with repetition and feedback, this an important feature to test. Research suggests that decisions makers tend to lean more on information they get from experience and less on the explicit descriptions of event probabilities they are given (with broad bracketing falling into the description category; Jessup et al., 2008; Lejarraga & Gonzalez, 2011; Newell et al., 2016; Rakow et al., 2008; Yechiam & Busemeyer, 2006). For low probability events, people may begin to ignore the broadly bracketed information over time and switch from sure loss (protective action) to the pure loss gamble (no protective action).

This is the first investigation to examine whether broad bracketing lasts and is robust to experience. Most empirical research on broad bracketing has focused on decisions in which participants make a single decision that is locked in for the period of the broad bracket (Benartzi & Thaler, 1999; Gneezy & Potters, 1997; Redelmeier & Tversky, 1992; Webb & Shu, 2017). Studies with repeated decisions and feedback have not explicitly examined the effect of experience or asked whether it interacted with broad bracketing (Hardin & Looney, 2012; Looney & Hardin, 2009). These designs also did not involve rare events, which are the types of events for which one is most likely to observe a deviation in experience-based and description-based decisions over time.

To formulate our hypothesis about the interaction of broad bracketing with experience, we account for two strong patterns of behavior that have been observed in decisions from experience: the recency effect and the gambler's fallacy (Croson & Sundali, 2005; Plonsky, Teodorescu, & Erev, 2015; Yin, Chen, Kunreuther, & Michel-kerjan, 2017). The *recency effect* leads to excessive focus on recent experiences (Barron & Yechiam, 2009; Hertwig et al., 2004; Hogarth & Einhorn, 1992) or those experiences most available in memory (Tversky & Kahneman, 1973). In a flood context, the recency effect suggests that a homeowner is likely to be more risk averse and engage in protective behavior in the immediate aftermath of a flood; as time passes without the occurrence of another flood, the emotional effects of the experience fade and the homeowner may decide to take their chances and drop their insurance. Behavior depends on events in the decision maker's most recent memories, which changes over time.

The *gambler's fallacy* is due to a mistaken understanding of what randomness looks like: People assume that events that have just occurred are less likely to occur next period but are more likely to occur as time passes since their last occurrence (Ayton & Fischer, 2004; Jarvik, 1951; Kahneman & Tversky, 1972). The gambler's fallacy suggests that a homeowner would be less likely to purchase insurance immediately after experiencing a flood but would start to worry that another flood is "due" to happen and invest in protection as time passes without the occurrence of a flood.

Both the recency effect and gambler's fallacy imply that experience-based choices exhibit some baseline variation over time. To account for the possible influence of the recency effect or gambler's fallacy on broad bracketing, we consider two timeframes: the "immediate" behavioral response following the occurrence of a negative event and the "delayed" behavioral response over time.

Hypothesis 2 (H2). *The effect of broad bracketing on choice of the sure loss will not be eliminated with experience (i.e., repetition and feedback). In particular, the effect will not be eliminated either (H2A) as time passes without the occurrence of a loss as a result of the recency effect, or (H2B) in the immediate aftermath of a loss as the result of the gambler's fallacy.*

2.3 Judgment processes behind broad bracketing

There is no guidance in the literature on how best to construct a broad bracket because very little work has explored the underlying mechanism. It is currently unclear whether only the probability of loss drives the broad bracket effect or whether decision makers also attend to the size of the bracket (which in this case corresponds to the length of the time horizon). That is, we do not know whether people respond the same to a 26% chance of at least one flood over 30 years as they would to a 26% over 200 years, despite the former representing a risk that is almost seven times larger (i.e., a 1% per year compared to a 0.15% per year).

Recent research suggests, but does not explicitly test, that people ignore the time horizon and attend primarily to the probability size. People weight losses more when receiving broadly bracketed information about a pure loss gamble, and this leads to increased risk aversion and more frequent selection of the sure loss (Webb & Shu, 2017). The authors further show that the framing technique does not operate by getting people to focus more on the number of times they will be exposed to a risk. If people focus primarily on probability size, then a longer time horizon (i.e., a broader bracket) will have a greater impact on protective behavior simply because the probability of a loss appears larger. Thus, people would be more likely to engage in protective behavior when they learn that there is a 26% chance of at least one flood over 30 years than when they learn there is a 5% chance over 5 years, despite the fact that the underlying likelihood of flood is the same.

Hypothesis 3 (H3). *For a pure loss gamble, the larger the bracket that is used for communicating the cumulative loss probability, the more likely people are to select the sure loss.*

If people also pay attention to the time horizon, they might discount larger cumulative probabilities if those probabilities are associated with longer time horizons. People are somewhat (but not fully) aware of how probabilities of outcomes change with multiple exposures to a risky event, like a gamble (e.g., Redelmeier & Tversky, 1992; Samuelson, 1963). This means that people might respond more strongly to a 26% chance of at least one flood over 30 years than to a 26% chance over 200 years. Thus, lengthening the bracket, i.e., increasing *both* the cumulative probability and time horizon, as suggested in H3 might not necessarily increase protective behavior. Instead, the length of the time horizon may moderate the impact of cumulative probability size on behavior:

Hypothesis 4 (H4). *(H4A) The greater the cumulative probability displayed, the more likely people will be to select the sure loss, but (H4B) for a given cumulative probability, the larger the bracket associated with that probability, the less likely people will be to select the sure loss.*

3. Overview of current work

This paper investigates whether the effect of broad bracketing on choices in the loss domain is robust to experience—i.e., repeated decision making with feedback—which is a critical feature for it to function as a robust risk communication tool. The role that experience plays in the effect of broad bracketing has not been studied before. We examine a simpler, more scalable form of the broad bracket, which also allows us to shed light on the mechanism. In particular, we investigate how the size of the cumulative probability and the length of the time horizon moderate the broad bracket effect, and doing so provides guidance to risk communicators for how to structure a broad bracket in their messages.

We conducted six web-based experiments with real payoffs that ask study participants to make choices between accepting a certain small loss or accepting a *chance* of a much larger loss. All six experiments share the same basic experimental design, which is based on an incentive-compatible design from a previous paper on insurance decisions (Kunreuther & Michel-Kerjan, 2015). Studies 1-5 test whether broad bracketing increases the proportion of people who take protective action and whether that effect is robust to experience. Study 3 examines whether the effect of broad bracketing is impacted by allowing earnings from previous choices to accumulate over rounds and whether it is robust to increasing the cost of insurance. Study 4 tests whether the results from the previous studies generalize to participants who actually live in highly flood-prone areas, and whether the broad bracket effect is robust to simultaneous presentation with the narrow bracket. Study 5 examines whether the effect of broad bracketing increases with longer time horizons and Study 6 investigates the relative contribution of probability size and time horizon length to the broad bracket effect. All data, power analyses, and pre-registrations are available on the Open Science Framework (OSF) at: https://osf.io/mejf5/?view_only=9b8760dca9714e85bc178f290140125e.

3.1 General study set up and procedures

Choices in the studies mimicked the structure of insurance decisions, where there is a small premium a person can incur to avoid a much larger loss that could occur with a low probability (1% chance per round). The survey was designed using the online survey platform Qualtrics. After reading the instructions and completing a comprehension check, participants began a 15-round phase of the experiment (except for study 6, which was a single round). At the beginning of each study individuals received an endowment and in each round had to select between two options: (1) a sure loss (the insurance premium) or (2) a gamble with a 1 percent chance of losing a portion of the endowment (pure loss gamble) in any round. Participants knew they would make this decision multiple times, but they did not know the precise number (15 rounds in studies 1-5, 1 round in study 6), only that it would be fewer than 25. We did this to avoid different behavior in the last round of the experiment (i.e., round 15), a common occurrence in multi-period experiments.

After each round, participants were given feedback about whether or not the gamble resulted in a loss, regardless of which option they had chosen. In studies 1, 2, and 4 the $95 endowment was restored to the initial level so that the financial impact of decisions in previous rounds would have no impact on their decision in the current round. Studies 3 and 5 allowed the endowment from the previous round carry over to the next round (e.g., a lower endowment if the participant experienced an uninsured loss). The exact wording of the study scenarios can be found in the appendix.

3.2 Participant recruitment and power analysis

We recruited a planned number of participants meeting required criteria for each study from Amazon's Mechanical Turk (MTurk). Statistical power to detect relevant effect sizes for parameters of interest was estimated for each study using a simulation-based procedure (conducted in Stata/IC version 14.2, code available upon request). Simulations created 500 replicate estimates of the relevant statistical model for a given planned number of participants and effect size. Statistical power was defined as the percent of replicates where a true effect was detected and could rule out a null effect with 95% confidence. The power analysis for all studies assumed a baseline rate of choice of sure loss of 0.617 (the baseline rate of choice of a sure loss from Kunreuther & Michel-Kerjan (2015), who used a similar experimental setup). See Table A0 in the appendix for results of the power analyses, including planned sample sizes and minimum detectible effects for Studies 1-6.

3.3 Payments

Participants were endowed with a large amount of money ($95), so the decisions would be treated as important and consequential; study 1 (abstract context) expressed the endowment in dollars, while studies 2-6 (flood insurance context) expressed the endowment in "talers," a fictional currency (1,000 talers = $1) We used the artificial currency in studies framed in an insurance context so participants would consider larger numeric amounts as would be the case in a real flood insurance decision.

To keep the experimental costs reasonable, one in every 100 participants was selected at random to play the experiment for real money. Earlier studies demonstrated that this strategy is just as effective in motivating behavior as paying all participants (Charness, Gneezy, & Halladay, 2016; Clot, Grolleau, & Ibanez, 2018). We used a procedure from Kunreuther and Michel-Kerjan (2015) to randomly select participants for real money play in a way that was credible to them. Each participant was randomly assigned a number between 0 and 99, which was displayed on the screen at the beginning and end of the experiment. They were given a specific future date and time of the Florida Pick-2 Lottery (http://www.flalottery.com/pick2) and told that if their number was chosen at that time they would be paid based on their decisions from a random round of the experiment. The number of the random round was displayed on the screen at the end of the experiment.

4. Study 1: Abstract Context

The first experiment determines whether presenting the cumulative loss probability decreases risk-seeking in the loss domain in the context of abstract gambles (H1) as has been shown for the standard form of broad bracketing (Webb & Shu, 2017), and whether this change is robust to experience, i.e., repetition and feedback (H2). In each round of this experiment, participants simply choose either a small sure loss or a pure loss gamble that has a low probability of a large loss and a high probability of no loss. We expect that presenting the broad bracket will increase the proportion of people selecting the sure loss and that this will not change substantially across rounds.

4.1 Methods

Procedure. At the beginning of each round, each individual was endowed with $95 and had to select between two options: (1) a sure loss of $0.45 (sure loss) or (2) a gamble with a 1 percent chance of losing $45 (pure loss gamble) in any round. At the beginning of each round, the $95 was restored to the initial level so that the financial impact of decisions in previous rounds would have no impact on their decision in the current round.

Design. The experimental design was a 2 (bracket size: 1-play, 30-play) x 3 (occurrence of loss: no loss, early loss, late loss) between-subjects design so that participants were randomly assigned to one of six conditions. Half the participants learned the 1-play likelihood of a loss occurring from selecting the gamble (1%), while the other half learned the 30-play likelihood of at least one loss occurring (26%). One third of the participants were assigned to witness the gamble result in a loss in round 4 (early-loss group), one third in round 11 (late-loss group), and the last third would not observe any losses from the gamble (no-loss group).

Measures. After reading the initial instructions, but before beginning the first round of the experiment, participants had to correctly answer three comprehension check questions in order to proceed (see the appendix for more detail). We assessed risk preferences using two measures: one self-reported and one choice-based. The self-reported measure was as follows: "How do you see yourself: Are you generally a person who likes to take risks or do you try to avoid taking risks?" [sliding scale: 0 = not at all willing to take risks to 10 = very willing to take risks] (Dohmen et al., 2011). The choice-based measure was adapted from Gneezy & Potters (1997): We endowed participants with $0.50 and gave them an opportunity to invest in an option with a 50% chance of succeeding and a 50% chance of failing. If the option succeeded, they would receive 2.5 times the amount they invested, but if it failed, they would lose the amount they invested. Participants had to decide how much of the 50 cents they wanted to invest in the option (in 1-cent increments). Participants also answered questions about their age, gender, income, education, employment status, and political identity.[1] At the end of the experiment, participants were told whether the option succeeded or failed and how much money they earned from their decisions.

Participants. A total of 1,076 participants completed the study and passed the comprehension check (56% female, M_{age} = 36 years, SD =11.2).

Payment. Participants were paid on average $1.07 (range: $0.50-$1.75), which included a bonus based on their answer to the Gneezy-Potters investment task. Five people chosen at random were each paid $94.55 based on their choices in the experiment.

4.2 Results and discussion

Participants made 15 rounds of choices with respect to opting for a sure loss or engaging in a pure loss gamble. Table 1 summarizes the results of random-effects linear panel regressions with heteroscedasticity-consistent standard errors (MacKinnon & White, 1985) for Studies 1-4, with results for Study 1 in column 1.[2] The predicted variable is the percentage of people choosing the sure loss. The reference or baseline group in the regression is the no-loss 1-play condition in round one, and the average proportion selecting the sure loss in this group is represented

[1] To check whether behavior and perceived risk mirrored each other, after rounds 1, 4, 11, and 15, participants indicated their perception of the likelihood that a loss would occur in the next round. We only asked this question four times to reduce decision fatigue. We chose rounds 1 and 15 to assess participants' initial beliefs without experience and their final beliefs. We included rounds 4 and 11 to examine the immediate effect of witnessing a loss on participants' beliefs. The question was asked after participants learned the outcome for that round and was posed as follows: "How likely do you feel it is that the lottery next period will result in a loss?" [sliding scale: 0 = extremely unlikely to 10 = extremely likely]. Analysis of this variable available upon request.

[2] In the estimates that follow, the variance-covariance matrix of the estimates is modified to account for heteroscedasticity under general conditions with a degrees-of-freedom correction (referred to as the HC1 method, MacKinnon and White 1985). In this case, because the number of respondents and observations is relatively large, using HC1 standard errors (instead of the random-effects least-squares standard errors) and the choice of HC1 modification does not have a noticeable effect on inference and the qualitative results.

by the constant term. Coefficients capture deviations from this level. Figure 1 displays results for Studies 1-4 (panels a-d). Each panel displays the proportion of participants selecting the sure loss across all 15 rounds of the experiment for the no-loss and early-loss conditions.[3] Study 1 results can be found in panel a.

Broad bracket effect (H1). As predicted by H1, we found that those who were given the 30-play cumulative loss probability in the pure loss gamble were more likely to select the sure loss than those who learned the 1-play probability. The Table 1, column 1 coefficient on the broad bracket condition indicator ("30-play prob.") displays that participants in the broad bracket condition were 11.7 percentage points more likely to select the sure loss in the first round and prior to experiencing a loss compared to those in the narrow bracket condition. This effect holds when controlling for sex, age, education, income, and risk tolerance (Table A1, column 1).

Interaction with experience (H2). To determine whether the broad bracket effect changed across rounds and was influenced by witnessing or experiencing a loss (H2), we included three additional variables in the regression ("round," "round after loss," and "time since loss") and tested whether they interacted with the variable representing the broad bracket effect. The "round" variable is a continuous variable that takes on values of the rounds (i.e., 2-15), with round one as the reference. Its coefficient describes the effect of time passing on selection of the sure loss. In order to capture further elements of the recency effect and gambler's fallacy for people who witnessed a loss, we included variables to separately capture behavior change in the round immediately after a loss and behavior change over time after the loss. "Round af.ter loss" is an indicator variable equal to 1 for choices of early-loss participants in round five and for choices of late-loss participants in round twelve (and equal to 0 otherwise). "Time since loss" is a continuous variable that is always equal to 0 for participants in the no-loss conditions, but that is equal to values between 1 to 11 for the early-loss and late-loss conditions depending on how many rounds it has been since the gamble resulted in a loss. For instance, for the early loss group, the value of that variable would be 1 for round five, 2 for round six, and so on.

The "round" variable shows a slight recency effect: Over time more people switched from the sure loss to the gamble (1 percentage point per round as shown in row 2). However, this round effect did not differ for people given the cumulative loss probability ("30-play prob. x round"), so the broad bracket effect was neither eliminated nor did it change in size for the no-loss conditions across rounds, or for the pre-loss rounds of the loss conditions.

The delayed effect of a loss ("time since loss") was the same for the early- and late-loss conditions and was consistent with elimination of the recency effect discussed above: After a loss, participants were no longer likely to switch to the gamble as time passed. The immediate effect of a loss ("round after loss" and "round after loss x late loss") was different for the early- and late-loss groups, with people in the former group being less likely to choose the sure loss in the round immediately after a loss and the latter group being more likely. While this may be an interesting difference driven by the different timings of the loss, exploring it is beyond the scope of this investigation. While witnessing a loss did cause significant changes in choice that were both immediate ("round after loss" and "round after loss x late loss") and delayed ("time since loss"), none of these changes interacted with the broad bracket. Thus, the effect of a loss did not undo or even mitigate the impact the broad bracket on selection of the sure loss. These results hold with the inclusion of controls (Table A1, column 1).

[3] The conditions with a late loss shock are included in the regression analysis, but the figures exclude them for ease of visual comparison with later studies that only include an early loss shock.

TABLE 1. OLS panel regressions for Studies 1-4. Predicted variable is choice of sure loss (flood insurance) over pure loss gamble (no flood insurance).

	Study 1	**Study 2**	**Study 3**		**Study 4**
	Includes all 6 conditions	Includes all 6 conditions	Includes 4 main conditions	Includes 4 no-loss conditions with low & high premiums	Includes 4 main conditions
	(1)	(2)	(3)	(4)	(5)
30-play prob.	0.117*** (0.025)	0.152*** (0.018)	0.125*** (0.026)	0.131*** (0.037)	0.057* (0.023)
Round	-0.010*** (0.002)	0.002* (0.001)	0.001 (0.002)	0.001 (0.002)	-0.005** (0.002)
Round after loss	-0.068* (0.027)	-0.067*** (0.017)	-0.044* (0.021)		-0.003 (0.023)
Time since loss	0.010*** (0.003)	0.009*** (0.002)	0.009*** (0.003)		0.009** (0.003)
Round after loss x late loss	0.127*** (0.037)	0.063** (0.022)			
Time since loss x late loss	0.005 (0.006)	-0.011* (0.004)			
30-play prob. x round	0.0002 (0.002)	-0.003^{+} (0.002)	-0.001 (0.003)	-0.001 (0.003)	-0.002 (0.003)
30-play prob. x round after loss	-0.024 (0.039)	-0.041^{+} (0.024)	-0.049 (0.031)		-0.077* (0.033)
30-play prob. x time since	-0.001 (0.004)	-0.001 (0.003)	-0.002 (0.004)		-0.0002 (0.004)
30-play prob. x round after loss x late loss	-0.012 (0.056)	-0.015 (0.032)			
30-play prob. x time since x late loss	0.008 (0.009)	0.004 (0.006)			
High price				-0.097* (0.039)	
30-play prob. x high price				0.008 (0.053)	
High price x round				-0.003 (0.003)	
30-play prob. x high price x round				-0.006 (0.004)	
Constant	0.584*** (0.018)	0.622*** (0.014)	0.555*** (0.020)	0.530*** (0.028)	0.720*** (0.017)
Independent rounds	yes	yes	no	no	yes
Floodplain sample	no	no	no	no	yes
Participants	1,076	2,076	1,051	1,062	1,019
Observations	16,140	31,140	15,765	15,930	15,285
R^2	0.012	0.013	0.011	0.007	0.005
Adjusted R^2	0.011	0.012	0.010	0.006	0.005

Note: Reference group is narrow bracket condition with no loss in round one. Heteroskedasticity-consistent (HC1) standard errors in parentheses. Dependent variable is binary choice of certain loss (insurance) over loss lottery (no insurance). $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

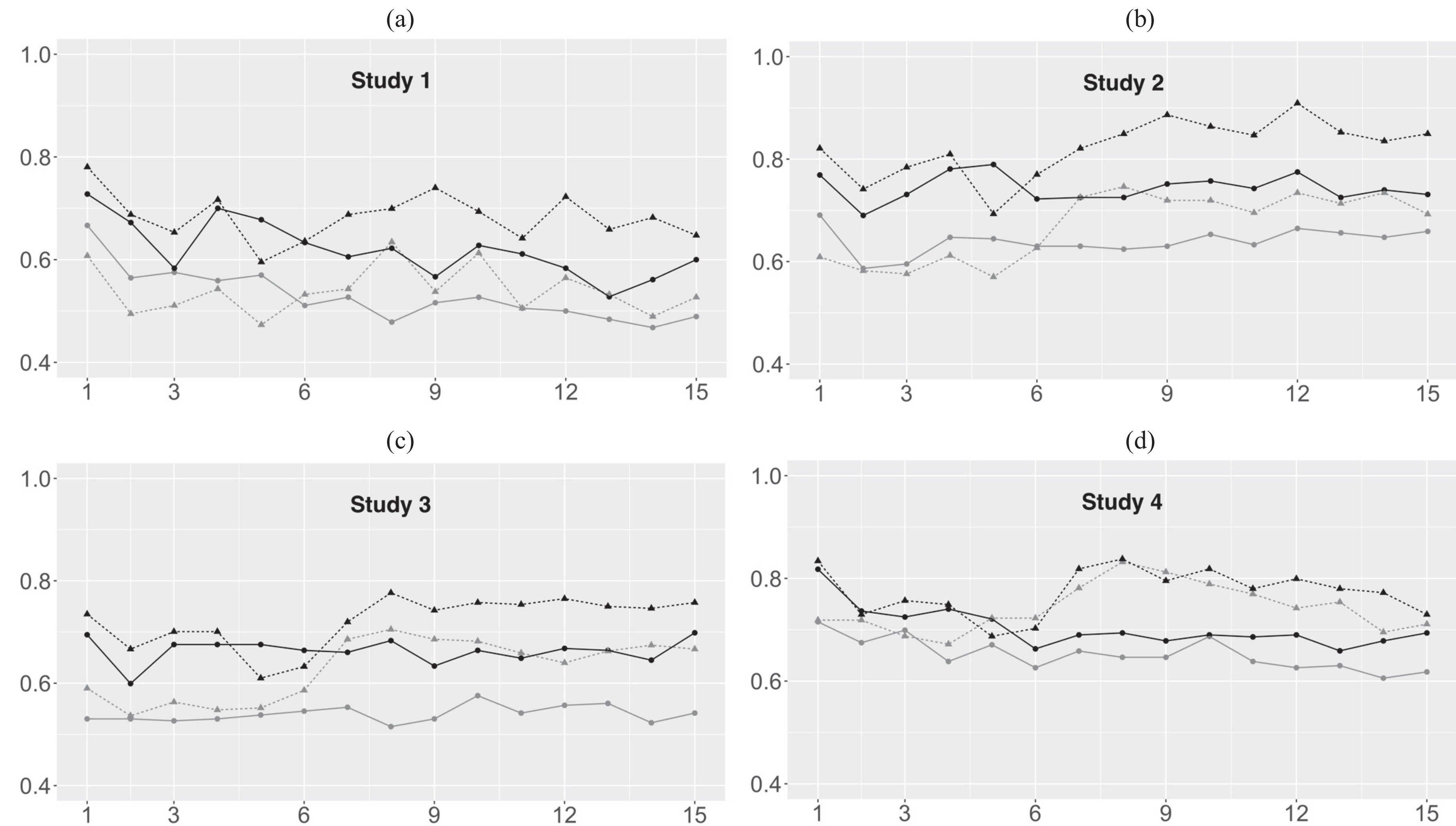

FIGURE 1. Proportion of participants selecting the sure loss (purchasing flood insurance) in each round for Studies 1-4 (panels a-d). The x-axis indicates the round number. The same four conditions are shown for each study: narrow-bracket no-loss (solid grey with circle markers —●—), broad-bracket no-loss (solid black with circle markers —●—), narrow-bracket early-loss (dashed gray with triangle markers - -▲- -), and broad-bracket early-loss (dashed black with triangle markers - -▲- -). The broad bracket conditions are represented by black lines, while the narrow bracket conditions are represented by gray lines. Solid lines with circle markers indicate conditions in which no loss occurred, while dashed lines with triangle markers represent conditions in which a loss occurred at the end of round 4.

5. Study 2: Flood Insurance Context

Study 2 enriched the experimental setup to reflect a risk communication environment, as recent research suggests that participants exhibit behavior that differs in relevant and important ways when choices are framed neutrally rather than in a particular context (see Jaspersen, 2016 for a review). For example, participants show more risk aversion when making decisions about insurance than when making context-free decisions for the same risk (Hershey & Schoemaker, 1980; Lypny, 1993). We chose a setting in which participants decide whether or not to purchase flood insurance by paying a premium so as to map onto the structure of Study 1.

5.1 Methods

Procedure. The procedure for this experiment was identical to that of Study 1, except that the scenario now incorporated a flood insurance context with endowments expressed in the fictional taler currency. Participants were told in each round that they needed to decide if they should buy flood insurance at a cost of 450 talers to protect their house from a flood that could cause 45,000 talers in damage. They would learn at the end of every year whether or not a flood occurred and see a summary of their assets given their insurance decision.

Design. The experimental design was the same as in Study 1: It was a 2 (bracket size: 1-year, 30-year) x 3 (loss occurrence: no loss, early loss, late loss) between-subjects design so that participants were randomly assigned to one of six conditions.[4] In this study, the narrow bracket conditions displayed the annual flood probability, while the broad bracket conditions displayed the likelihood of experiencing at least one flood over a 30-year time horizon.

Measures. As in Study 1, we included comprehension check questions, two methods to assess risk preferences, and four subjective probability assessments. We also included questions eliciting experience with flooding and purchasing flood insurance to use as control variables in our regressions. The exact wording of the scenario and these measures are detailed in the appendix.

Participants. A total of 2,076 participants completed the study and passed the comprehension check (45% female, M_{age} = 35.9 years, SD =11.1).

Payment. Participants were paid on average $1.09 (range: $0.51-$1.75), which included a bonus based on their answer to the Gneezy-Potters investment task. Additionally, 18 participants were randomly chosen to be paid based on their choices—15 of them received $94.55, while the other three received $95.

5.2 Results and discussion

Table 1, column 2 displays the results of Study 2, where the predicted variable is the percentage of people purchasing insurance. The reference condition in the regression is the no-flood 1-year condition in round one, and the average proportion purchasing insurance in this group is represented by the constant term. Figure 1b displays the proportion of participants purchasing insurance across all 15 rounds of the experiment for the conditions with no flood damage and those with an early flood loss.

Broad bracket effect (H1). As in Study 1, broad bracketing increased the proportion of participants purchasing flood insurance, thus supporting H1. Participants shown the cumulative loss probability over 30 years

[4] We also included a manipulation that tested the effect of a policy relevant feature of real-world flood insurance decisions: the designation of a person's house as being located within a FEMA-named Special Flood Hazard Area, generally considered higher risk zones. We expected this dichotomous naming convention would have an impact on decisions. This line of inquiry was pursued for its policy relevance but was not germane to our investigation of broad bracketing. Importantly, there was no detectable impact of this information, so we collapsed across those conditions in our investigation of the hypotheses at hand.

were 15.2 percentage points more likely to purchase insurance in the first round and prior to experiencing a loss than those who saw the 1-year probability of a flood. This effect holds when controlling for real-life flood experience as well as sex, age, education, income, and risk tolerance (Table A1, column 2).

Interaction with experience (H2). We used the same variables to examine the effect of experience as we did for Study 1 ("round," "round after loss," "time since loss", "round after loss x late loss", "time since x late loss"). The broad bracket ("30-year prob.") did not interact with any of these experience variables, thus supporting H2. These results hold with the inclusion of controls (Table A1, column 2).

6. Study 3: Non-independent decisions

One limitation of Studies 1 and 2 is that each of the rounds was independent from previous ones so that participants were shielded from having a choice in an early round permanently ruin their chances to earn a large bonus in the experiment. This treatment may have reduced the potential for experience-based effects and is not as reflective of real-world decisions, where a past outcome impacts future choices. An additional consideration is that, in Studies 1 and 2, the insurance premium was set equal to the expected value of the loss, but in real life, the price of insurance is usually much higher than the risk neutral price, often worth 1-2% of a person's income, due to considerations like loading costs. Thus, Study 3 extends our investigation by making rounds *non-independent*, that is, by structuring the experiment so that earnings carry over across all 15 rounds, and by testing whether price interacts with the broad bracket effect.

6.1 Methods

Procedure. The procedure for this experiment was similar to Study 2, except that earnings carried across rounds (i.e., the endowment did not reset with each round in the event of an uninsured loss), and we altered the monetary amounts so that total assets remained the same and earnings could be cumulated across rounds: The house value was reduced to 80,000 talers, and the savings account was increased to 15,000 talers. The potential damage from a flood was changed to 35,000 talers with an annual probability of 1%. At the end of each round, participants were provided with the value of their total assets based on their insurance decision and whether or not a flood had occurred during the round. Participants' total earnings were summarized at the end of the experiment.

Design. The experimental design was a 2 (bracket size: 1-year, 30-year) x 2 (loss occurrence: no-loss, early-loss) between-subjects design. The cost of insurance in these conditions was the actuarially fair premium (350 talers per year). To examine the impact of the insurance premium on behavior, we included two additional no-loss conditions in which the premium was much higher (1,000 talers per year): 1-year high price and 30-year high price. Participants were randomly assigned to one of these six conditions.

Measures. We included the same questions as in Study 2 and additional measures designed to assess participants' ability to calculate probabilities across different time periods, the perceived relevance/usefulness of the cumulative probability information, and the preferred time period over which each participant would like to know the cumulative probability. The details of these measures can be found in the appendix.

Participants. A total of 1,591 participants completed the study and passed the comprehension check (45% female, M_{age} = 35.9 years, SD =11.9).

Payment. Participants were paid on average \$1.95 (range: \$1-\$2). In addition, 20 participants were randomly chosen to receive payment based on their choices in the experiment, receiving between \$80 and \$90.

6.2 Results and discussion

Broad bracket effect (H1). For the main analyses testing H1 and H2 (Table 1, column 3), we include only the four main conditions in which the price of insurance was set to the risk neutral price of 350 talers (N=1,051). Figure 1c displays the proportion of participants purchasing insurance in these four conditions. We find a significantly positive impact of the broad bracket ("30-play prob.") on choice in the direction predicted: Participants were 12.5 percentage points more likely to purchase insurance in the first round and prior to experiencing a loss than those who saw the 1-year probability. This effect holds when controlling for real-life flood experience as well as sex, age, education, income, and risk tolerance (Table A1, column 3).

Interaction with experience (H2). The broad bracket did not interact with any of the experience variables ("round," "round after loss," "time since loss"). These results hold with the inclusion of controls (Table A1, column 3).

Price effects. To examine whether price interacted with the broad bracket, we conducted an analysis that compared the 1-year and 30-year no-loss conditions from the main analysis with the two additional conditions in which the price of insurance was set to 1,000 talers (N=1,062; Table 1, column 4). Though we found that the increased price led to an overall drop in insurance purchasing by about 9.7 percentage points ("High price"), we found no interaction between price and the broad bracket ("30-play prob. X high price"). We also did not find any interaction between price and either round ("High price X round) or round and the broad bracket ("30-play prob. X high price X round").

Thus, we found that allowing earnings to carry across rounds did not qualitatively or quantitatively affect the main broad bracketing effect nor did it lead to an interaction of the broad bracket with experience, offering confirming evidence for H1 and H2. These effects hold when controlling for real-life flood experience as well as sex, age, education, income, and risk tolerance (Table A1, column 4).

7. Study 4: Disaster-prone sample

In Study 4, we extend our investigation in two ways. First, we collect a sample of participants living in highly flood-prone areas. We target US counties where people are likely to have more experience with both flooding and flood insurance. Second, we test whether giving participants both the narrow and broad brackets eliminates the effect of broad bracketing. One concern with the broad bracket is that it operates by skewing people's preferences, making them behave more risk averse than is consistent with their underlying risk preferences. This suggests that decision makers primarily care about the narrow bracket, and only use the broad bracket to make inferences about the narrow bracket, which they do imperfectly. If this is true, then presenting both versions would completely eliminate the broad bracket effect because decision makers would ignore the broad bracket. However, if the foregoing explanation does not entirely account for the effect—i.e., if people still find the broad bracket informative—then the effect will not be eliminated entirely. In cases where people anticipate making multiple decisions over an extended time period, we expect that decisions makers will still find the broadly bracketed information useful and respond to it.

7.1 Methods

Procedure. The procedures for this experiment were identical to that of Study 2.

Design. The design was a 2 (bracket size: 1-year, 30-year) x 2 (loss occurrence: no-loss, early-loss) between-subjects design. We also included an additional no-loss condition: the 1-and-30 condition.[5] Participants in this condition learned *both* the annual flood probability and the probability of at least one flood over 30 years. Participants were randomly assigned to one of these five conditions.

---

[5] For two exploratory conditions not central to our investigation, we recruited 468 additional participants. Those conditions involved presenting participants with both the 1-year probability as well as a cumulative probability over an extremely long time horizon. In one condition, that time horizon was 100 years, and in the other it was 200 years. Results for those two conditions are available upon request.

Measures. We included the same measures as in Study 3.

Participants. We recruited participants from an online panel through which participants could be identified by zip code. We selected participants from twelve US counties in which at least 50% of the flood insurance policies were owned by people in areas where flood insurance was not a requirement for getting a mortgage loan (i.e., outside the Special Flood Hazard Areas). We made this designation to ensure we were selecting counties in which most of the people were at risk of flood, most of the people were aware of this risk, and many people were likely to have experienced a flood and/or purchased flood insurance previously. The list of counties we sampled from can be found in the appendix. A total of 1,260 participants completed the study and passed at least one of the three comprehension check questions (70% female, M_{age} = 43.3 years, SD =15.9).

Payment. Participants were offered a payment equivalent to those in the previous three studies and were told there was a chance they would earn a bonus based on their decisions.[6] In addition, 14 participants were randomly selected to be paid based on their decisions; ten participants received $94.55, and four received $95.

7.2 Results and discussion

Our sampling procedure was successful in increasing the proportion of people in the sample with flood experience and/or experience purchasing flood insurance. Thirty seven percent indicated experiencing a flood disaster at least one time compared to only 9% and 18.5% in Studies 2 and 3; 55% indicated that they had ever purchased flood insurance while only 11.4% and 24.2% ever had this coverage in Studies 2 and 3 respectively.

Broad bracket effect (H1). For testing H1 and H2 (Table 1, column 5), we include only the four main conditions in which participants received information for only one of the two brackets (N=1,019). Figure 1d displays the proportion of participants purchasing insurance in these four conditions. We find a significantly positive impact of the broad bracket on choice in the direction predicted. Participants were 5.7 percentage points more likely to purchase insurance in the first round and prior to experiencing a loss than those who saw the 1-year probability, though this was a somewhat smaller increase than in Studies 2 and 3. One possible explanation for this difference is that people in this sample were already much more likely to purchase flood insurance in any given round. The baseline rate of insurance purchasing for people with the narrow bracket was 72%, which is 10 percentage points higher than in the previous two studies. This lowered the proportion of decisions (i.e., choices to not purchase insurance) that could be influenced by broad bracketing. This effect holds when controlling for real-life flood experience as well as sex, age, education, income, and risk tolerance (Table A1, column 5).

Interaction with Experience (H2). The broad bracket ("30-year prob.") did not interact with either the "round" or the "time since loss" variables (H2A supported), but it did interact with the "round after loss" variable:

---

[6] Personal communication from the panel administrators regarding exact pay received by participants was as follows: "Our panelists select a number of different ways they would like to be incentivized. Some choose cash, travel miles, gift cards, points to gift card websites etc. Also, we often have to incentivize the last 10% of the respondents more in an effort to complete the survey. What we use as a good rule of thumb is that you can assume that ~25-35% of the total cost per complete goes to the respondent." Since we were charged $7.50 per participant, this means that participants received the equivalent of $1.88 to $2.63 in whatever medium they chose to receive their incentive.

Participants in the 30-year conditions who experienced a flood loss in round 4 were 2 percentage points less likely (summing the coefficients on “30-year prob.” and “30-year prob. x round after loss”) to purchase flood insurance in the round right after the loss than participants who also experienced a loss in round 4 but who were in the 1-year condition. This effectively eliminated the broad bracket effect in round 5 for participants who witnessed the loss (H2B not supported). These effects hold when including control variables (Table A1, column 5).

Based on additional analyses (available upon request from the authors), we offer one possibility that could have led to the different result regarding H2B in this study than in Studies 1, 2, and 3. In Study 4, participants who experienced an early flood but were *uninsured* at the time behaved in a way that was opposite to participants who experience an early flood but were *insured*. In earlier studies, only the insured loss experiencers displayed an immediate reaction to loss (i.e., to drop insurance). In this case, the uninsured loss experiencers were much more likely to take up insurance in the subsequent round than were similar participants in Studies 1-3. In the previous studies, people who were uninsured did not differ in their behavior in round 5, whether or not they experienced a flood loss. This may be a function of Study 4 participants being real-life residents of high-risk floodplains and being more affected by the in-experiment floods because of it. Because the 1-year condition had a more even mix of insured and uninsured who experienced the loss shock, and because the main regression did not control for insured status, the counteracting effects of the insured and uninsured likely canceled each other out and led to a coefficient on “round after loss” that was not different from zero. However, because the broad bracket conditions had higher proportions of insured participants overall than in the narrow bracket conditions, the average effect detected is that of the insured participants—i.e., to drop one’s insurance immediately after the flood shock. This observation demonstrates that the fact that the broad bracketing could backfire in the case of a flood, at least in the immediate aftermath, if it has led to a significantly larger proportion of people being insured and if those insured people respond to a flood by dropping insurance (and uninsured people respond to a flood by taking up insurance).

Simultaneous presentation of the narrow and broad brackets. To test the impact of providing both the narrow and broad brackets, we conducted a regression that included only participants in the three no-loss conditions: 1-year, 30-year, and 1-and-30 year (N=745; Table 2). For this analysis, the 1-and-30 condition served as the reference condition. When controlling for round effects, we observed no statistically significant difference between any of the conditions. The broad bracket was associated with a 5.5 percentage point higher take up of insurance in the first round relative to the 1-and-30 condition, but this was only marginally significant. As there was no interaction between round and condition, we ran a regression where we provided more observations by not distinguishing between rounds so as to obtain more statistical power for differentiating between smaller effect sizes (Table 2, column 2). We found the 1-and-30 condition led to an average 2.2 percentage point *decrease* in insurance purchasing compared to the 30-year condition— some choices are indeed skewed by the broad bracket—but was associated with a 3 percentage point *increase* in purchasing insurance relative to the 1-year condition—suggesting that not all choices are skewed but rather informed by the broad bracket. The proportion of people purchasing insurance in each of the 15 rounds for these three conditions is visualized in Figure 2. (When controls are included in the regression collapsed across rounds, the 1-and-30 condition does not appear different from the broad bracket condition. See Table A2.)

TABLE 2. OLS regressions for Study 4. Predicted variable is purchase of flood insurance.

	Panel regression (1)	Simple OLS collapsed across rounds (2)
1-play prob.	-0.006 (0.035)	-0.030** (0.011)
30-play prob.	0.055+ (0.033)	0.022* (0.011)
Round	-0.002 (0.002)	
1-play prob. x round	-0.003 (0.003)	
30-play prob. x round	-0.004 (0.003)	
Constant	0.701*** (0.024)	0.682*** (0.008)
Participants	745	745
Observations	11,175	11,175
R^2	0.005	0.002
Adjusted R^2	0.005	0.002

Note: Reference group is 1-and-30 condition in round one for column one and collapsed across rounds for column two. Heteroskedasticity-consistent (HC1) standard errors in parentheses. Dependent variable is binary choice to purchase insurance. +p<0.1; *p<0.05; **p<0.01; ***p<0.001

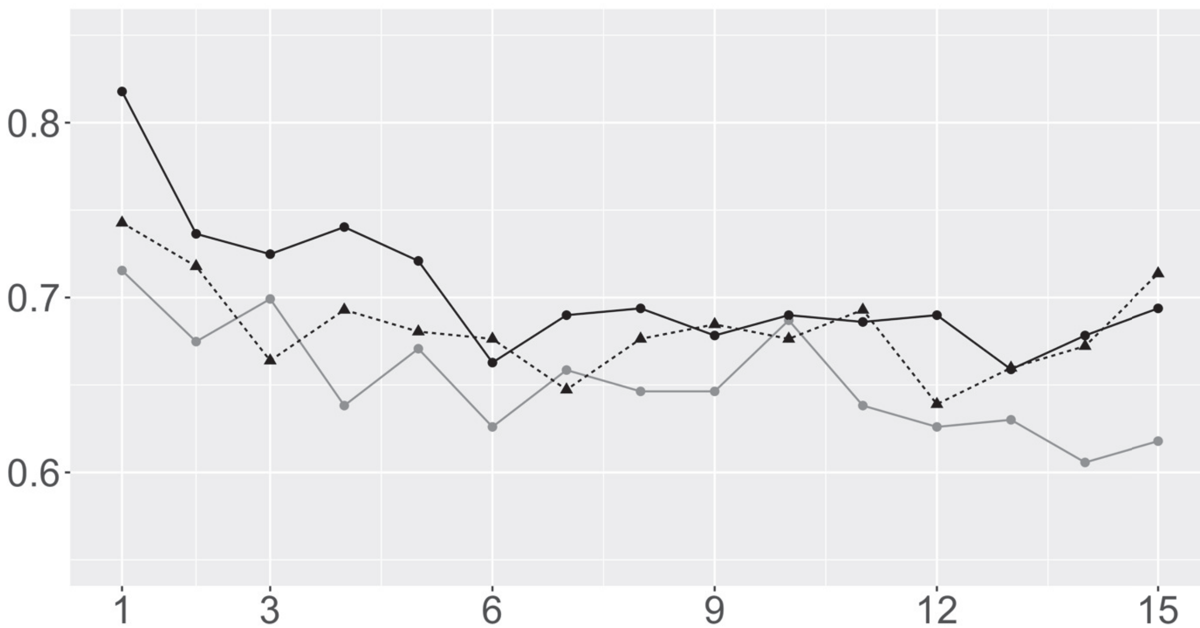

FIGURE 2. Proportion of participants purchasing flood insurance in each round for Study 4. The x-axis indicates the round number. Only the three no-loss conditions are displayed: narrow-bracket no-loss (solid grey with circle markers —●—), broad-bracket no-loss (solid black with circle markers —●—), 1-and-30 condition no-loss (dashed black with triangle markers - -▲- -).

8. Study 5: Changing the length of the time horizon

In Study 5, we extend our investigation in two ways. First, we test whether the broad bracket effect generalizes to other time horizons. Second, we begin to investigate the mechanism and examine whether the broad bracket effect depends on the magnitude of the cumulative probability such that longer time horizons, which are associated with larger cumulative probabilities, lead more people to purchase insurance (H3).

8.1 Methods

Procedure. The procedure for this experiment was the same as for Study 3, where rounds were non-independent, i.e., earnings carried across rounds.

Design. Participants were randomly assigned to one of five, between-subjects conditions that only differed by length of the time horizon over which the cumulative probability information was presented: 1 year, 5 years, 10 years, 30 years, or 200 years. No conditions included loss occurrences.

Measures. We included the same measures and in the same format as in Study 3.

Participants. A total of 1,081 participants completed the survey and passed the comprehension check (55% female, M_{age} = 38.6 years, SD =12.2).

Payment. Participants were paid on average $1.96 (range: $1-$2). In addition, five participants were randomly chosen to receive payment based on their choices in the experiment; these participants received between $89.80 to $93.30.

8.2 Results and discussion

Broad bracket effect (H1). Figure 3 displays the proportion of participants purchasing insurance in each of the five conditions. We included all five conditions for the main analysis, with the 1-year condition in round one as the reference group (Table 3, column 1). We find a significantly positive impact of all of the broad-bracket conditions on flood insurance take up in the first round compared to the 1-year condition: The 5-year bracket led to a 14.1 percentage point increase; the 10-year led to a 16 percentage point increase; the 30-year led to a 17.3 percentage point increase; and the 200-year led to a 16.8 percentage point increase. The magnitude of these effects are a few percentage points smaller but still significant when controlling for real-life flood experience as well as sex, age, education, income, and risk tolerance (Table A3, column 1).

Interaction with experience (H2). There was no average change in purchasing across rounds ("round"), nor was there any interaction of the round variable with any of the broad bracket conditions. These results hold with the inclusion of controls (Table A3, column 1).

Broad bracket size (H3). To examine whether the length of the time horizon among the broad bracket conditions was positively associated with purchasing insurance (H3), we ran two regressions that excluded the 1-year condition and used the 5-year condition as the reference condition (N=861; Table 3, columns 2 and 3). In column 2, we ran a regression with indicator variables comparing the 10-year, 30-year, and 200-year conditions to the 5-year reference condition. In column 3, we defined a continuous variable that had four different values corresponding to the number of years associated with each of the different time horizon lengths (i.e., 5, 10, 30, and 200). We found no difference among the broad bracket conditions in either regression (or when including controls as in Table A3, columns 2 and 3), thus, rejecting H3. The size of the probability is, therefore, not the only driving force behind the broad bracket effect. One potential explanation for this pattern is that probability size is positively related to protective behavior, *but* there is also a counteracting time horizon effect, as proposed by H4. We examine this in the next experiment.

TABLE 3. OLS panel regressions for Study 5. Predicted variable is purchase of flood insurance.

	All time horizons (1-year as reference)	Excluding 1-year condition (5-year as reference)	
		Time horizons coded as indicator variables	Time horizon coded as continuous variable
	(1)	(2)	(3)
5-play prob.	0.141*** (0.041)		
10-play prob.	0.160*** (0.040)	0.019 (0.036)	
30-play prob.	0.173*** (0.040)	0.032 (0.037)	
200-play prob.	0.168*** (0.041)	0.027 (0.038)	
Length of time horizon			0.0001 (0.0002)
Round	-0.004 (0.002)	-0.001 (0.002)	-0.001 (0.001)
5-play prob. x round	0.002 (0.003)		
10-play prob. x round	0.005+ (0.003)	0.003 (0.003)	
30-play prob. x round	0.001 (0.003)	-0.001 (0.003)	
200-play prob. x round	0.002 (0.003)	-0.001 (0.003)	
Length of time horizon x round			-0.00001 (0.00001)
Constant	0.556*** (0.031)	0.697*** (0.027)	0.712*** (0.016)
Participants	1,081	861	861
Observations	16,215	12,915	12,915
R^2	0.004	0.001	0.0003
Adjusted R^2	0.003	0.0001	0.00004

Note: Earnings carried over across rounds in Study 5 (non-independent rounds). For column one, the reference group is the 1-year condition in round one. For columns two and three, the reference group is the 5-year condition in round one. In column three, the “length of time horizon” variable is a continuous variable with values equal to the length of the time horizon corresponding to the condition {1, 5, 10, 30, 200}. Heteroskedasticity-consistent (HC1) standard errors in parentheses. Dependent variable is binary choice to purchase insurance. +p<0.1; ***p<0.001

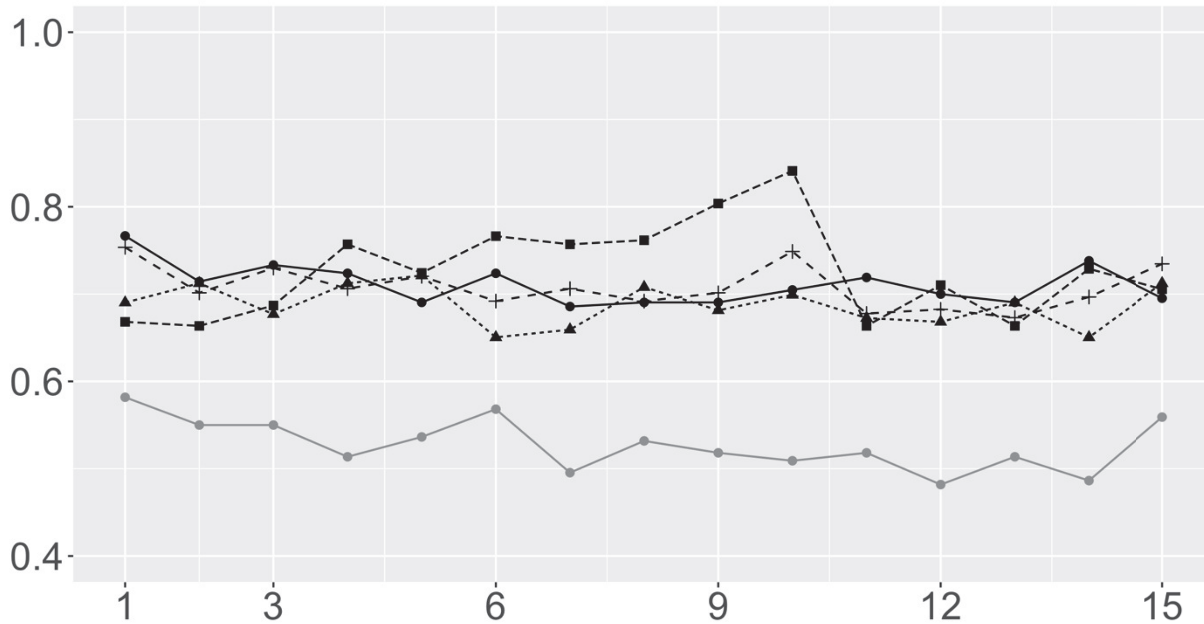

FIGURE 3. Proportion of participants purchasing flood insurance in each round for Study 5. The x-axis indicates the round number. All five no-loss conditions are displayed: 1-year bracket (solid grey with circle markers —●—), 30-year bracket (solid black with circle markers —●—), 5-year bracket (dashed black with triangle markers - -▲- -), 10-year bracket (dashed black with square markers - -■- -), and 200-year bracket (dashed black with plus-sign markers - -+- -). All the broad bracket conditions are represented in black.

9. Study 6: Probability size versus time horizon length

Study 6 examines the roles played by probability size and time horizon length in creating the broad bracket effect. H4 proposes that decision makers are sensitive to both probability size and time horizon length but in the opposite direction: The greater the cumulative probability, the *more* people will take protective action, but the longer the time horizon, the *less* people will take protective action.

9.1 Methods

Procedure. The procedure for this experiment was the same as for Study 5, except that participants made only one decision (i.e., one round). However, participants were told that they would not be told the number of rounds they would face ahead of time, and that they could potentially play up to 25 rounds. The talers conversion was changed to 10,000 talers per $1 so that the maximum possible earnings from one's decisions in the experiment was $9.50.

Design. The design was a 2(time horizon length: 10 years, 200 years) x 5(cumulative probability size: 1%, 10%, 26%, 63%, 87%) between-subjects design, and participants were randomly assigned to one of these ten conditions. We chose cumulative probabilities to span the range of values between 0 and 100% and included those used in our previous studies. Note that these probability-time horizon combinations all reflect risks with different underlying annual probabilities.

Measures. In addition to the measures used in the earlier studies, we asked each participant to guess the one-year probability for their event ("What do you think (make your best guess) is the probability (in percent) of at least one flood over the next 1 year?" [free response number between 0 to 100 with up to two decimal places]. We also included a short, 3-item subjective numeracy scale (McNaughton, Cavanaugh, Kripalani, Rothman, & Wallston, 2015).

Participants. A total of 2,303 participants completed the survey and passed the comprehension check (57% female, M_{age} = 37.3 years, SD = 11.6).

Payment. Participants were paid on average $0.70 (range: $0.50-$0.75). In addition, 21 participants were randomly chosen to receive payment based on their choices in the experiment; 16 participants received $9.40, while five received $9.50.

9.2 Results and discussion

Table 4 displays the main results of Study 6, where the predicted variable is the percentage of people purchasing insurance. Figure 4 displays the predicted values (lines) and observed values (points) for the proportion of participants purchasing insurance in each condition. The cumulative probability associated with each condition is indicated on the x-axis and the length of the time horizon is represented by the line type and point shape. The 95% confidence bands for the predicted values are shown.

Probability size versus time horizon length (H4). To examine the relative contribution of probability size and time horizon length on behavior, we ran an OLS regression with purchase decision as the predicted variable, probability size (continuous), time horizon length (10 years as the reference), and the interaction of those two variables as the predictor variables (Table 4). As expected, we found a positive association between cumulative probability size ("Probability") and purchasing: When the time horizon was 10 years, every one percentage point increase in the cumulative probability shown was associated with a 0.4 percentage point increase in insurance take up. This effect was the same size regardless of whether the interaction term was included in the regression or not. Thus, the model predicts that going from a cumulative probability of 1% to one of 87% should result in an increase in insurance purchasing of about 34 percentage points. We also found that, when an interaction term was included, there was no main effect of increasing the time horizon to 200 years, but there was an interaction: The longer time horizon did not have as much impact for smaller cumulative loss probabilities as it did for larger cumulative loss probabilities. When the time horizon was 200 years, every one percentage point increase in the cumulative probability was associated with a 0.2 percentage point increase in insurance take up, an effect which is half the size of that for the 10-year time horizon. This means the model predicts only a 17.2 percentage point increase in purchasing going from a cumulative probability of 1% to one of 87%. These results hold with control variables for real-life flood experience as well as sex, age, education, income, and risk tolerance (Table A4).

TABLE 4. OLS regressions for Study 6. Predicted variable is purchase of flood insurance.

	Main effects only	Interaction
	(1)	(2)
Probability	0.004^{***} (0.0003)	0.004^{***} (0.0004)
200-year prob.	-0.096^{***} (0.020)	-0.030 (0.030)
Probability x 200-year prob.		-0.002^{**} (0.001)
Constant	0.503^{***} (0.018)	0.470^{***} (0.021)
Observations	2,303	2,303
R^2	0.067	0.070
Adjusted R^2	0.066	0.069
Note: **p<0.01; ***p<0.001		

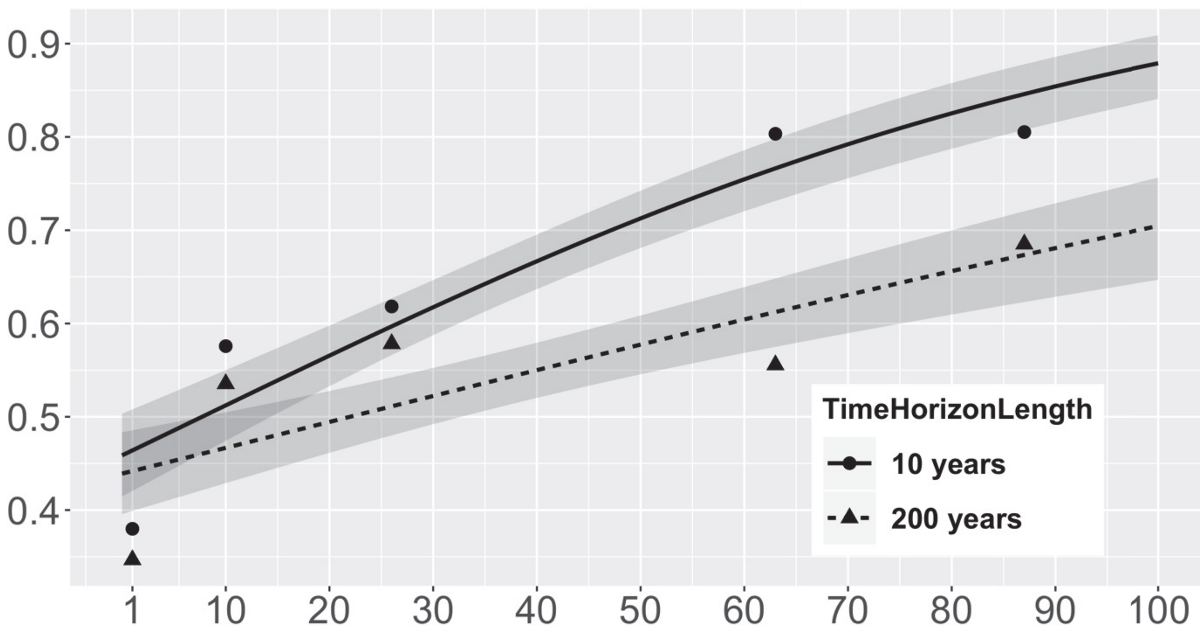

FIGURE 4. Probability of purchasing flood insurance in each condition for Study 6. The x-axis indicates the cumulative probability displayed to participants in a given condition. The points represent observed values for participants in the 10-year conditions (circles ●) and the 200-year conditions (triangles ▲). The lines represent the predicted values from the regression with the interaction term (Table 6, column 2) for the 10-year conditions (solid line —) and the 200-year conditions (dashed line - - -).

10. General discussion

We investigated whether the effect of broad bracketing is present when using a simpler, more scalable presentation format (presenting cumulative loss probabilities), and whether the effect persists over time when choices have to be made repeatedly and in the face of feedback. Furthermore, we examined the mechanism behind broad bracketing by isolating the relative importance of probability size and bracket size to the effect. Examining these questions not only advances theory but also helps assess whether broad bracketing can be used as a risk communication tool to motivate people to protect themselves from catastrophic losses. For this investigation, we focused on the example of flooding.

The findings from our six studies have established that broad bracketing can be robust to repetition and feedback, both of which are key features of the decision environment that people face when making protective decisions. Specifically, we find that providing the cumulative probability of experiencing at least one negative event over multiple periods compared to the probability of experiencing the event during one period leads more people to take protective action against a risk. Importantly, we find that this effect lasts across 15 rounds of decisions with feedback after each decision whether or not one experiences a loss. Study 1 demonstrates that this effect occurs in an abstract setting and Studies 2-5 show that it also occurs in the richer, more ecologically relevant situation of insuring one's house and assets against a catastrophic flood. We found that all effects hold even after controlling for real-life personal experience with floods.

We find that these effects are also robust to other features of the decision environment. In Study 3, we find that allowing earnings and losses to carry forward across rounds (like they do in real life) does not change the main broad bracketing result. In the same study, we find that, although increasing the cost of the protective action (i.e., the price of flood insurance) lowers the proportion of people taking the protective action across all conditions, as one would expect, the higher cost does not decrease the size of the broad bracket effect. In Study 4, we find that the broad bracket increases protective behavior even among people who live in a floodplain and have experienced the flood risk in real life. We also discover that presenting the narrow and broad brackets simultaneously reduces the broad bracket effect, but does not eliminate it.

Our investigation also provides insights into the mechanism behind broad bracketing. The combined findings from Studies 5 and 6 demonstrate that the broad bracket effect is *not* solely driven by the size of the probabilities shown: While a larger cumulative probability leads to more people taking protective action, this effect is dampened for longer time horizons, and this dampening effect is larger for larger probabilities. These results indicate that both elements of broad brackets inform decision makers' judgments.

10.1 Theoretical implicati\ons

Our findings have a number of important theoretical implications. First, we find that a more scalable, simple form of broad bracketing that displays only the probability of loss (i.e., extending the time horizon) can have the same effect in increasing risk averse behavior as previously demonstrated in the loss domain for the standard form of broad bracketing (i.e., presenting a full distribution of outcomes; Webb & Shu, 2017). Second, we find that the broad bracket effect is robust to repetition and feedback, which has not been previously demonstrated. In particular, the effect is not only not eliminated in the face of experience, but it largely remains the same size over time.

Third, with this latter finding, our work also contributes to the literature on decisions from description versus decisions from experience. Past research found that decisions made in the presence of both description and experience converged with the pattern of decisions made solely from experience, suggesting that people eventually ignore description and place more weight on experience (Jessup et al., 2008; Lejarraga & Gonzalez, 2011; Newell et al., 2016; Rakow et al., 2008; Yechiam & Busemeyer, 2006). This provides reasonable justification to believe the broad bracket effect will disappear over time, or at least decrease in size. However, across all our studies, we found this not to be the case: Even when participants have the exact same sequence of experiences, their choices are significantly influenced by the description they receive, that is, participants with the broadly bracketed probability description make different choices than those with the narrowly bracketed probability description. Furthermore, we observed almost no change in the size of this effect with the exception of one (Study 4) of our five studies examining this question. Thus, we suggest that while experience may indeed influence choices when participants are presented with both description and experience, so, too, does description. Future research is needed to rectify these diverging observations.

Fourth, our findings advance our understanding of the mechanism underlying broad bracketing. We show that broad bracketing is not solely the result of error-prone inference of the one-period probability, or the skewing of risk preferences. Even when given both the one-period and cumulative probabilities, participants are still influenced in their choices by the presence of the cumulative probability, suggesting they derive additional value from that information. Furthermore, the size of the cumulative probability of the loss is an important driver of the effect, but it is not the only driver. Participants discount a given cumulative probability more when it is presented using a longer time horizon, reflecting an understanding that it represents an event with a lower underlying probability of occurring.

10.2 Policy implications

Presenting the cumulative loss probability can be used by risk communicators as a powerful way to motivate protective behavior against rare catastrophic events. However, the broad bracket will likely only change behavior around rare events if it involves a cumulative probability that is large enough that people will not ignore it. For instance, extending the time horizon over 10 years might be effective for an event with a 1% annual probability (which translates to about a 10% probability of one or more floods over 10 years), but might not be effective for an event with a 0.1% annual probability (which translates to a 1% probability of one or more floods over 10 years). Indeed, Study 6 demonstrates that the “10% over 10 years” leads to more than a 55% take-up of flood insurance, but the “1% over 10 years” leads to less than 40% take-up of flood insurance—more than a 15 percentage point difference. This finding suggests that when an event is really rare, it may be worthwhile for risk communicators to use longer time horizons. A 0.1% annual chance might be presented as an 18% chance of one or more floods over 200 years. If communicators are worried that doing so may bias decision makers too far away from their natural preferences, results from Study 4 show that communicators can simultaneously present both the cumulative and one-period probabilities without entirely eliminating a potential broad bracketing effect.

However, we advise the use of caution in providing overly long time horizons. Though we did not find any evidence of backfiring from especially long time horizons—in Study 6, among people who received cumulative probabilities extended over 200 years, there was still a positive trend associated with increasing the size of the cumulative probability—we did observe a dampening effect due to longer time horizons in both Studies 5 and 6. Further research is needed to fully confirm whether there might be some danger that participants reduce attention to or ignore the broad bracket if it is considered unreasonably long.

Based on our findings, for events with an annual risk of 1% or more, there is little need to use time horizons longer than five or ten years. Study 5 revealed that extending the time horizon to five years substantially increased protective behavior compared to presenting the one-year probability of loss; however, extending the time horizon even further (10 years, 30 years, 200 years) did not result in any additional increase in protective behavior. People also seem to *prefer* shorter time periods. Exploratory questions in Studies 3-5 indicated that the majority of people preferred a time horizon of 10 years or less in specifying the likelihood of a flood when making decisions on purchasing insurance. The ideal time horizon will likely vary for events of different underlying probabilities of occurring.

10.3 Future research directions

Future research is needed to deepen our understanding of the dynamics of broad bracketing and its application to risk communication. It is currently unclear whether different types of broad bracketing presentations will have different impacts on behavior. We tested an alternative type of broad bracketing (cumulative loss probabilities) in which we presented only the cumulative probability of one or more losses and the timeframe over which that probability was calculated. This differs from the standard broad bracketing presentation of the full distribution of outcomes. Though we found similar impacts to previous work, it is unclear whether there could be unique benefits or risks to each format.

Utilizing the cumulative loss probability format, we were able to demonstrate for the first time that the length of the time horizon impacts the effectiveness of a broad bracketing framing. Though we did not focus on this in our investigation, in Study 6 we found the dampening of the longer time horizon to be concentrated among cumulative probabilities greater than 50%. Cumulative probabilities lower than 50% largely yielded similar behavior regardless of whether they were calculated over 10 or 200 years. Furthermore, though we found a positive relationship between cumulative probability size and the likelihood of taking protective action even for the longer time horizon of 200 years, it is unclear whether there is a possibility that a time horizon can be so long that it backfires. Study of this potentially non-linear relationship between attention to the broad bracket and the length of the time horizon could provide further insights into how people respond to the risk of rare events.

Further work is also needed to examine whether the length of the time horizon interacts with the number of decision periods a person expects to have. For instance, in Study 5, we noticed a substantial dip in protective action for participants in the 5-year and 10-year conditions in the round immediately following these specific rounds. Some participants who received the 5-year probability of at least one flood (5%) and did not experience a loss during the first five rounds, seemed to believe they were "in the clear." Thus, whether a risk communicator provides a broad bracket that is larger or smaller than the number of decisions a decision maker will be making may influence whether such dips in protective behavior are observed.

The extent to which the broad-bracketing effect appears in the real world, when people face real opportunity costs, more demands on their attention, as well as real consequences to their decisions, is left to future research. Though we observed a broad bracketing effect among people living in a floodplain, the effect was smaller than among participants who did not live in a floodplain. The base rate of purchasing insurance was much higher within this floodplain sample than among participants in the other studies, and based on this, one possibility is that these individuals were convinced that they should purchase insurance whether the probability of a future flood was presented for next year or over a 30 year period.

Another concern is that probabilities of a loss in the real world change over time, as is the case with respect to climate change and its impact on flood-related damage in future years. If people believe that real-life event probabilities are dynamic and change over time, they may place less weight on descriptive probability information. Field research is needed to examine these questions.

Future work could benefit by examining variation across individuals with respect to whether and to what extent broad bracketing impacts behavior. In particular, individual differences in numeracy and intuitive math operations may be influential in the broad bracket effect. In Study 4, we observed that providing both the 1-year and 30-year probabilities did result in a decrease in the size of the broad bracketing effect, suggesting that this experimental condition was helping some people correct error-prone inferences they would have made if they were only given the broad bracket. Thus, the size of the broad bracket impact on a population may depend on the proportion of people who make such inferences in the absence of full information.

References

Ayton, P., & Fischer, I. (2004). The hot hand fallacy and the gambler's fallacy: Two faces of subjective randomness? *Memory & Cognition*, *32*(8), 1369–1378. https://doi.org/10.3758/BF03206327

Barron, G., & Erev, I. (2003). Small Feedback-based Decisions and Their Limited Correspondence to Description-based Decisions. *Journal of Behavioral Decision Making*, *16*(3), 215–233. https://doi.org/10.1002/bdm.443

Barron, G., & Yechiam, E. (2009). The coexistence of overestimation and underweighting of rare events and the contingent recency effect. *Judgment and Decision Making*, *4*(6), 447–460. https://doi.org/http://www.doaj.org/doaj?func=openurl&genre=article&issn=19302975&date=2009&volume=4&issue=6&spage=447

Benartzi, S., & Thaler, R. H. (1999). Risk Aversion or Myopia? Choices in Repeated Gambles and Retirement Investments. *Management Science*, *45*(3), 364–381. https://doi.org/10.1287/mnsc.45.3.364

Charness, G., Gneezy, U., & Halladay, B. (2016). Experimental methods: Pay one or pay all. *Journal of Economic Behavior & Organization*, *131*, 141–150. https://doi.org/10.1016/j.jebo.2016.08.010

Clot, S., Grolleau, G., & Ibanez, L. (2018). Shall we pay all? An experimental test of Random Incentivized Systems. *Journal of Behavioral and Experimental Economics*, *73*(February), 93–98. https://doi.org/10.1016/j.socec.2018.01.004

Croson, R., & Sundali, J. (2005). The Gambler' s Fallacy and the Hot Hand: Empirical Data from Casinos. *Journal of Risk and Uncertainty*, *30*(3), 195–209.

De La Maza, C., Davis, A., Gonzalez, C., & Azevedo, I. (2019). Understanding Cumulative Risk Perception from Judgments and Choices: An Application to Flood Risks. *Risk Analysis*, *39*(2), 488–504. https://doi.org/10.1111/risa.13206

Dohmen, T., Falk, A., Huffman, D., Sunde, U., Schupp, J., & Wagner, G. G. (2011). Individual Risk Attitudes: Measurement, determinants, and behavioral consequences. *Journal of the European Economic Association*, *9*(3), 522–550. https://doi.org/10.1111/j.1542-4774.2011.01015.x

Doyle, J. K. (1997). Judging Cumulative Risk. *Journal of Applied Social Psychology*, *27*(6), 500–524. https://doi.org/10.1111/j.1559-1816.1997.tb00644.x

FEMA. (2018). *Fact Sheet: FEMA assistance is a supplement to private recovery efforts — not a substitute*. Retrieved from https://www.fema.gov/news-release/2018/06/14/fact-sheet-fema-assistance-supplement-private-recovery-efforts-not

Fuller, R., Dudley, N., & Blacktop, J. (2004). Older people's understanding of cumulative risks when provided with annual stroke risk information. *Postgraduate Medical Journal*, *80*(949), 677–678. https://doi.org/10.1136/pgmj.2004.019489

Gneezy, U., & Potters, J. (1997). An experiment on risk taking and evaluation periods. *The Quarterly Journal of Economics*, *112*(2), 631–645. Retrieved from http://qje.oxfordjournals.org/content/112/2/631.short

Hardin, A. M., & Looney, C. A. (2012). Myopic loss aversion: Demystifying the key factors influencing decision problem framing. *Organizational Behavior and Human Decision Processes*, *117*(2), 311–331. https://doi.org/10.1016/j.obhdp.2011.11.005

Hershey, J. C., & Schoemaker, P. J. H. (1980). Risk Taking and Problem Context in the Domain of Losses : An Expected Utility Analysis. *Journal of Risk and Insurance*, *47*(1), 111–132.

Hertwig, R., Barron, G., Weber, E. U., & Erev, I. (2004). Decisions from Experience and the Effect of Rare Events in Risky Choice. *Psychological Science*, *15*(8), 534–539. https://doi.org/10.1111/j.0956-7976.2004.00715.x

Hogarth, R. M., & Einhorn, H. J. (1992). Order effects in belief updating: The belief-adjustment model. *Cognitive Psychology*, *24*(1), 1–55. https://doi.org/10.1016/0010-0285(92)90002-J

Jarvik, M. E. (1951). Probability learning and a negative recency effect in the serial anticipation of alternative symbols. *Journal of Experimental Psychology*, *41*(4), 291–297. https://doi.org/10.1037/h0056878

Jaspersen, J. G. (2016). Hypothetical surveys and experimental studies of insurance demand: A review. *Journal of Risk and Insurance*, *83*(1), 217–255. https://doi.org/10.1111/jori.12100

Jessup, R. K., Bishara, A. J., & Busemeyer, J. R. (2008). Feedback produces divergence from prospect theory in descriptive choice. *Psychological Science*, *19*(10), 1015–1022. https://doi.org/10.1111/j.1467-9280.2008.02193.x

Kahneman, D., & Lovallo, D. (1993). Timid Choices and Bold Forecasts: A Cognitive Perspective on Risk Taking. *Management Science*, *39*(1), 17–31. https://doi.org/10.1287/mnsc.39.1.17

Kahneman, D., & Tversky, A. (1972). Subjective probability: A judgment of representativeness. *Cognitive Psychology*, *3*(3), 430–454. https://doi.org/10.1016/0010-0285(72)90016-3

Kahneman, D., & Tversky, A. (1979). Prospect Theory: An Analysis of Decision under Risk. *Econometrica*, *47*(2), 263–292.

Keller, C., Siegrist, M., & Gutscher, H. (2006). The role of the affect and availability heuristics in risk communication. *Risk Analysis*, *26*(3), 631–639. https://doi.org/10.1111/j.1539-6924.2006.00773.x

Knäuper, B., Kornik, R., Atkinson, K., Guberman, C., & Aydin, C. (2005). Motivation Influences the Underestimation of Cumulative Risk. *Personality and Social Psychology Bulletin*, *31*(11), 1511–1523. https://doi.org/10.1177/0146167205276864

Kunreuther, H. (1996). Mitigating disaster losses through insurance. *Journal of Risk and Uncertainty*, *12*(2–3), 171–187. https://doi.org/10.1007/BF00055792

Kunreuther, H., Ginsberg, R., Miller, L., Sagi, P., Slovic, P., Borkan, B., & Katz, N. (1978). *Disaster Insurance Protection: Public Policy Lessons*. John WIley and Sons, Inc.

Kunreuther, H., & Michel-Kerjan, E. (2015). Demand for fixed-price multi-year contracts: Experimental evidence from insurance decisions. *Journal of Risk and Uncertainty*, *51*(2), 171–194. https://doi.org/10.1007/s11166-015-9225-4

Lejarraga, T., & Gonzalez, C. (2011). Effects of feedback and complexity on repeated decisions from description. *Organizational Behavior and Human Decision Processes*, *116*(2), 286–295. https://doi.org/10.1016/j.obhdp.2011.05.001

Linville, P. W., Fischer, G. W., & Fischhoff, B. (2015). AIDS risk perceptions and decision biases. In J. B. Pryor & G. D. Reeder (Eds.), *The Social Psychology of HIV Infection* (Vol. 24, pp. 1–38). Hillsdale NJ: Lawrence Erlbaum. https://doi.org/10.4324/9781315722924

Long, H. (2017, August 29). Where Harvey is hitting hardest, 80 percent lack flood insurance. *The Washington Post*. Washington, DC. Retrieved from https://www.washingtonpost.com/news/wonk/wp/2017/08/29/where-harvey-is-hitting-hardest-four-out-of-five-homeowners-lack-flood-insurance/?utm_term=.62cee8661b99

Looney, C. A., & Hardin, A. M. (2009). Decision Support for Retirement Portfolio Management: Overcoming Myopic Loss Aversion via Technology Design. *Management Science*, *55*(10), 1688–1703. https://doi.org/10.1287/mnsc.1090.1052

Lypny, G. J. (1993). An Experimental Study of Managerial Pay and Firm Hedging Decisions. *Journal of Risk and Insurance*, *60*(2), 208–229.

MacKinnon, J. G., & White, H. (1985). Some heteroskedasticity consistent covariance matrix estimators with improved finite sample properties. *Journal of Econometrics*, *29*(3), 305–325. https://doi.org/10.1016/0304-4076(85)90158-7

McClelland, G. H., Schulze, W. D., & Coursey, D. L. (1993). Insurance for low-probability hazards: A bimodal response to unlikely events. *Journal of Risk and Uncertainty*, *7*(1), 95–116. https://doi.org/10.1007/BF01065317

McNaughton, C. D., Cavanaugh, K. L., Kripalani, S., Rothman, R. L., & Wallston, K. A. (2015). Validation of a Short, 3-Item Version of the Subjective Numeracy Scale. *Medical Decision Making*, *35*(8), 932–936. https://doi.org/10.1177/0272989X15581800

Meyer, R., & Kunreuther, H. (2017). *The Ostrich Paradox: Why We Underprepare for Disasters*. Wharton Digital Press.

Newell, B. R., Rakow, T., Yechiam, E., & Sambur, M. (2016). Rare disaster information can increase risk-taking. *Nature Climate Change*, *6*(2), 158–161. https://doi.org/10.1038/nclimate2822

Plonsky, O., Teodorescu, K., & Erev, I. (2015). Reliance on small samples, the wavy recency effect, and similarity-based learning. *Psychological Review*, *122*(4), 621–647. https://doi.org/10.1037/a0039413

Rakow, T., Demes, K. A., & Newell, B. R. (2008). Biased samples not mode of presentation: Re-examining the apparent underweighting of rare events in experience-based choice. *Organizational Behavior and Human Decision Processes*, *106*(2), 168–179. https://doi.org/10.1016/j.obhdp.2008.02.001

Read, D., Loewenstein, G. F., & Rabin, M. (1999). Choice Bracketing. *Journal of Risk and Uncertainty*, *19*(3), 171–197. https://doi.org/10.1023/A:1007879411489

Redelmeier, D. A., & Tversky, A. (1992). On the framing of multiple prospects. *Psychological Science*, *3*(3), 191–193. https://doi.org/10.1111/j.1467-9280.1992.tb00025.x

Robinson, P. J., & Wouter Botzen, W. J. (2018). The impact of regret and worry on the threshold level of concern for flood insurance demand: Evidence from Dutch homeowners. *Judgment and Decision Making*, *13*(2), 237–245. Retrieved from http://www.sjdm.org/journal/18/18201a/jdm18201a.html

Samuelson, P. A. (1963). Risk and Uncertainty: A Fallacy of Large Numbers. *Scientia*, *98*, 108–113. Retrieved from http://www.casact.org/pubs/forum/94sforum/94sf049.pdf

Shaklee, H., & Fischhoff, B. (1990). The Psychology of Contraceptive Surprises: Cumulative Risk and Contraceptive Effectiveness. *Journal of Applied Social Psychology*, *20*(5), 385–403.

https://doi.org/10.1111/j.1559-1816.1990.tb00418.x

Slovic, P., Fischhoff, B., & Lichtenstein, S. (1978). Accident probabilities and seat belt usage: A psychological perspective. *Accident Analysis and Prevention*, *10*(4), 281–285. https://doi.org/10.1016/0001-4575(78)90030-1

Slovic, P., Fischhoff, B., Lichtenstein, S., Corrigan, B., Slovic, P., Fischhoff, B., & Lichtrenstin, S. (1977). Preference for Insuring against Probable Small Losses : Insurance Implications. *The Journal of Risk and Insurance*, *44*(2), 237–258.

Thaler, R. H., Tversky, A., Kahneman, D., & Schwartz, A. (1997). The Effect of Myopia and Loss Aversion on Risk Taking: An Experimental Test. *The Quarterly Journal of Economics*, *112*(2), 647–661. https://doi.org/10.1162/003355397555226

Tversky, A., & Kahneman, D. (1973). Availability: A heuristic for judging frequency and probability. *Cognitive Psychology*, *5*(2), 207–232. https://doi.org/0.1016/0010-0285(73)90033-9

Visschers, V. H. M., Meertens, R. M., Passchier, W. W. F., & de Vries, N. N. K. (2009). Probability Information in Risk Communication: A Review of the Research Literature. *Risk Analysis*, *29*(2), 267–287. https://doi.org/10.1111/j.1539-6924.2008.01137.x

Webb, E. C., & Shu, S. B. (2017). Is broad bracketing always better? How broad decision framing leads to more optimal preferences over repeated gambles. *Judgment and Decision Making*, *12*(4), 382–395.

Weber, E. U., Shafir, S., & Blais, A.-R. (2004). Predicting Risk Sensitivity in Humans and Lower Animals: Risk as Variance or Coefficient of Variation. *Psychological Review*, *111*(2), 430–445. https://doi.org/10.1037/0033-295X.111.2.430

Yechiam, E., & Busemeyer, J. R. (2006). The effect of foregone payoffs on underweighting small probability events. *Journal of Behavioral Decision Making*, *19*(1), 1–16. https://doi.org/10.1002/bdm.509

Yin, H., Chen, J., Kunreuther, H., & Michel-kerjan, E. (2017). *Availability Heuristic and Gambler's Fallacy over Time in a Natural Disaster Insurance Choice Setting*.

Appendix

TABLE A0. Power analyses for Studies 1-6. Minimum detectible effects (MDE) are shown for main effects and interactions of interest.

Study	Planned N	Design	Rounds	Main effect MDE (percentage points)	Additional effects MDE (percentage points)
1	1,000	2 (brackets) x 3 (no loss, early loss, late loss)	15	BB = 8.5	BB x round = 0.4 BB x round-after-loss = 4.0 BB x time-since-loss = 0.7
2	2,000	2 (brackets) x 3 (no loss, early loss, late loss)	15	BB = 6.0	BB x round = 0.2 BB x round-after-loss = 2.5 BB x time-since-loss = 0.5
3	1,000	2 (brackets) x 2 (no loss, loss)	15	BB = 9.0	BB x round = 0.4 BB x round-after-loss = 4.0 BB x time-since-loss = 0.7
4	1,000	2 (brackets) x 2 (no loss, loss)	15	BB = 9.0	BB x round = 0.4 BB x round-after-loss = 4.0 BB x time-since-loss = 0.7
5	1,000	5 conditions	15	Time horizon = 13.5	Time horizon x round = 0.4
6	2,300	2 (time horizon) x 5 (probabilities)	1	Time horizon = 8.5 Probability = 0.1	Time horizon x probability = 0.2

TABLE A1. OLS panel regressions for Studies 1-4 with demographic and control variables. Predicted variable is choice of sure loss (flood insurance) over pure loss gamble (no flood insurance).

	Study 1 Includes all 6 conditions (1)	**Study 2** Includes all 6 conditions (2)	**Study 3** Includes 4 main conditions (3)	**Study 3** Includes 4 no-loss conditions with low & high premiums (4)	**Study 4** Includes 4 main conditions (5)
30-play prob.	0.121*** (0.025)	0.153*** (0.018)	0.115*** (0.026)	0.122*** (0.036)	0.056* (0.023)
Round	-0.010*** (0.002)	0.002* (0.001)	0.001 (0.002)	0.001 (0.002)	-0.005** (0.002)
Round after loss	-0.067* (0.027)	-0.067*** (0.017)	-0.044* (0.021)		-0.004 (0.023)
Time since loss	0.010*** (0.003)	0.009*** (0.002)	0.009*** (0.003)		0.009** (0.003)
Round after loss x late loss	0.126*** (0.037)	0.063** (0.022)			
Time since loss x late loss	0.005 (0.006)	-0.011* (0.004)			
30-play prob. x round	0.0004 (0.002)	-0.003+ (0.002)	-0.001 (0.003)	-0.001 (0.003)	-0.002 (0.003)
30-play prob. x round after loss	-0.026 (0.039)	-0.041+ (0.024)	-0.049 (0.031)		-0.077* (0.033)
30-play prob. x time since	-0.002 (0.004)	-0.001 (0.003)	-0.002 (0.004)		-0.0001 (0.004)
30-play prob. x round after loss x late loss	-0.011 (0.056)	-0.015 (0.032)			
30-play prob. x time since x late loss	0.008 (0.009)	0.004 (0.006)			
High price				-0.091* (0.039)	
30-play prob. x high price				0.004 (0.051)	
High price x round				-0.003 (0.003)	
30-play prob. x high price x round				-0.006 (0.004)	
Sex: female	-0.056* (0.023)	0.008 (0.016)	-0.046* (0.022)	-0.022 (0.022)	-0.009 (0.022)
Sex: other/no answer	-0.045 (0.154)	0.142+ (0.076)	-0.347+ (0.210)	-0.442*** (0.086)	0.125 (0.085)
Age	-0.0002 (0.001)	0.001 (0.001)	-0.002+ (0.001)	-0.0002 (0.001)	0.001 (0.001)
Educ: HS diploma/GED	0.101 (0.160)	-0.150 (0.105)	0.219 (0.203)	0.150 (0.323)	-0.017 (0.080)
Educ: some college	0.144 (0.157)	-0.141 (0.103)	0.271 (0.203)	0.193 (0.322)	0.065 (0.079)
Educ: 2-year coll. deg.	0.112 (0.158)	-0.140 (0.104)	0.219 (0.203)	0.171 (0.323)	-0.009 (0.082)
Educ: 4-year coll. deg.	0.136 (0.156)	-0.132 (0.103)	0.269 (0.202)	0.238 (0.322)	0.080 (0.079)
Educ: Master's deg.	0.173 (0.159)	-0.125 (0.105)	0.304 (0.204)	0.240 (0.323)	0.058 (0.082)
Educ: doctrate deg.	0.376* (0.177)	0.011 (0.112)	0.223 (0.222)	0.102 (0.341)	-0.039 (0.113)
Educ: professional deg.	0.223 (0.174)	-0.071 (0.113)	0.316 (0.225)	0.286 (0.338)	0.069 (0.101)
Income	-0.0004 (0.0003)	-0.0004+ (0.0002)	-0.0002 (0.0003)	-0.0005 (0.0003)	0.0005+ (0.0002)
Self-assessed risk tolerance	-0.031*** (0.004)	-0.023*** (0.003)	-0.044*** (0.004)	-0.040*** (0.004)	-0.039*** (0.004)
Real-life Flood Experience		0.031 (0.026)	0.075** (0.028)	0.092** (0.029)	0.077*** (0.019)

Constant	0.650*** (0.160)	0.832*** (0.107)	0.572** (0.206)	0.527 (0.324)	0.769*** (0.090)
Independent rounds	yes	yes	no	no	yes
Floodplain sample	no	no	no	no	yes
Participants	1,076	2,076	1,051	1,062	1,019
Observations	16,140	31,140	15,765	15,930	15,285
R^2	0.016	0.015	0.018	0.014	0.015
Adjusted R^2	0.015	0.015	0.017	0.013	0.014

Note: Reference group is narrow bracket condition with no loss in round one. Heteroskedasticity-consistent (HC1) standard errors in parentheses. Dependent variable is binary choice of certain loss (insurance) over loss lottery (no insurance). $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

TABLE A2. OLS regressions for Study 4. Predicted variable is purchase of flood insurance.

	Panel regression (1)	Simple OLS collapsed across rounds (2)
1-play prob.	-0.006 (0.033)	-0.030^{**} (0.010)
30-play prob.	0.047 (0.031)	0.014 (0.010)
Round	-0.002 (0.002)	
1-play prob. x round	-0.003 (0.003)	
30-play prob. x round	-0.004 (0.003)	
Sex: female	0.005 (0.027)	0.005 (0.009)
Sex: other/no answer	0.247^{***} (0.048)	0.247^{*} (0.115)
Age	0.001^{+} (0.001)	0.001^{***} (0.0003)
Educ: HS diploma/GED	0.299^{**} (0.100)	0.299^{***} (0.052)
Educ: some college	0.327^{**} (0.100)	0.327^{***} (0.052)
Educ: 2-year coll. deg.	0.316^{**} (0.103)	0.316^{***} (0.053)
Educ: 4-year coll. deg.	0.374^{***} (0.099)	0.374^{***} (0.052)
Educ: Master's deg.	0.379^{***} (0.105)	0.379^{***} (0.053)
Educ: doctrate deg.	0.349^{**} (0.121)	0.349^{***} (0.059)
Educ: professional deg.	0.422^{***} (0.119)	0.422^{***} (0.057)
Income	0.001^{*} (0.0003)	0.001^{***} (0.0001)
Self-assessed risk tolerance	-0.048^{***} (0.005)	-0.048^{***} (0.002)
Real-life Flood Experience	0.090^{***} (0.023)	0.090^{***} (0.009)
Constant	0.444^{***} (0.103)	0.426^{***} (0.054)
Participants	745	745
Observations	11,175	11,175
R^2	0.019	0.103
Adjusted R^2	0.017	0.102

Note: Reference group is 1-and-30 condition in round one for column one and collapsed across rounds for column two. Heteroskedasticity-consistent (HC1) standard errors in parentheses. Dependent variable is binary choice to purchase insurance. $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

TABLE A3. OLS panel regressions for Study 5. Predicted variable is purchase of flood insurance.

	All time horizons (1-year as reference)	Excluding 1-year condition (5-year as reference)	
		Time horizons coded as indicator variables	Time horizon coded as continuous variable
	(1)	(2)	(3)
5-play prob.	0.109^{**} (0.039)		
10-play prob.	0.139^{***} (0.038)	0.029 (0.035)	
30-play prob.	0.160^{***} (0.038)	0.051 (0.035)	
200-play prob.	0.132^{***} (0.039)	0.024 (0.036)	
Length of time horizon (continuous)			0.00001 (0.0002)
Round	-0.004 (0.002)	-0.001 (0.002)	-0.001 (0.001)
5-play prob. x round	0.002 (0.003)		
10-play prob. x round	0.005^{+} (0.003)	0.003 (0.003)	
30-play prob. x round	0.001 (0.003)	-0.001 (0.003)	
200-play prob. x round	0.002 (0.003)	-0.001 (0.003)	
Length of time horizon (continuous) x round			-0.00001 (0.00001)
Sex: female	-0.052^{*} (0.020)	-0.065^{**} (0.021)	-0.065^{**} (0.021)
Sex: other/no answer	0.112^{**} (0.041)	0.111^{*} (0.045)	0.109^{*} (0.047)
Age	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)
Educ: HS diploma/GED	0.209^{+} (0.110)	0.196^{+} (0.112)	0.187^{+} (0.107)
Educ: some college	0.177 (0.108)	0.188^{+} (0.110)	0.183^{+} (0.104)
Educ: 2-year coll. deg.	0.188^{+} (0.110)	0.170 (0.112)	0.167 (0.107)
Educ: 4-year coll. deg.	0.199^{+} (0.108)	0.208^{+} (0.109)	0.205^{*} (0.103)
Educ: Master's deg.	0.202^{+} (0.111)	0.193^{+} (0.112)	0.189^{+} (0.107)
Educ: doctrate deg.	0.332^{**} (0.125)	0.417^{***} (0.113)	0.408^{***} (0.108)
Educ: professional deg.	0.295^{*} (0.128)	0.329^{*} (0.130)	0.325^{**} (0.125)
Income	-0.0002 (0.0003)	-0.0003 (0.0003)	-0.0003 (0.0003)
Self-assessed risk tolerance	-0.052^{***} (0.004)	-0.051^{***} (0.004)	-0.051^{***} (0.004)
Real-life Flood Experience	0.072^{**} (0.028)	0.086^{**} (0.029)	0.086^{**} (0.029)
Constant	0.599^{***} (0.118)	0.686^{***} (0.118)	0.716^{***} (0.110)
Participants	1,081	861	861
Observations	16,215	12,915	12,915
R^2	0.016	0.014	0.014
Adjusted R^2	0.015	0.013	0.012

Note: Earnings carried over across rounds in Study 5 (non-independent rounds). For column one, the reference group is the 1-year condition in round one. For columns two and three, the reference group is the 5-year condition in round one. In column three, the "length of time horizon" variable is a continuous variable with values equal to the length of the time horizon corresponding to the condition {1, 5, 10, 30, 200}. Heteroskedasticity-consistent (HC1) standard errors in parentheses. Dependent variable is binary choice to purchase insurance. $^{+}p<0.1$; $^{*}p<0.05$; $^{**}p<0.01$; $^{***}p<0.001$

TABLE A4. OLS regressions for Study 6. Predicted variable is purchase of flood insurance.

	Main effects only (1)	Interaction (2)
Probability	0.003*** (0.0003)	0.004*** (0.0004)
200-year prob.	-0.080*** (0.019)	-0.022 (0.029)
Probability x 200-year prob.		-0.002** (0.001)
Sex: female	0.066*** (0.020)	0.066*** (0.020)
Sex: other/no answer	0.207 (0.146)	0.205 (0.146)
Age	-0.001 (0.001)	-0.001 (0.001)
Educ: HS diploma/GED	-0.057 (0.189)	-0.057 (0.189)
Educ: some college	-0.017 (0.188)	-0.020 (0.188)
Educ: 2-year coll. deg.	-0.029 (0.189)	-0.033 (0.189)
Educ: 4-year coll. deg.	-0.073 (0.187)	-0.076 (0.187)
Educ: Master's deg.	-0.007 (0.189)	-0.010 (0.188)
Educ: doctrate deg.	-0.224 (0.207)	-0.226 (0.207)
Educ: professional deg.	-0.087 (0.203)	-0.085 (0.203)
Income	-0.0004 (0.0003)	-0.0004 (0.0003)
Self-assessed risk tolerance	-0.045*** (0.004)	-0.045*** (0.004)
Real-life Flood Experience	0.155*** (0.027)	0.156*** (0.027)
Constant	0.770*** (0.191)	0.737*** (0.191)
Observations	2,303	2,303
R^2	0.143	0.146
Adjusted R^2	0.137	0.140

Note: **p<0.01; ***p<0.001

Scenario for Study 1

You have been given $95. You have two options:

1) *CERTAIN LOSS: A certain loss of $0.45.*

or

2) *LOTTERY: A lottery with a chance of losing $45. The chance of losing money* ***if you play this lottery one time is 1% [at least once if you play this lottery 30 times is 26%].***

Regardless of whether you select the CERTAIN LOSS or the LOTTERY, you will learn the outcome of the lottery at the end of each round. Each round involves a new decision with $95. That is, at the end of each round, your earnings will be reset. You will play multiple rounds (but fewer than 25 rounds).

Scenario for Study 2

Imagine you own a house that is worth 90,000 talers and have a savings account containing 5,000 talers. There is a chance that a major flood will occur during the year in which case you will suffer severe damage and the value of the house will decrease by 45,000 talers. If you have purchased insurance for the year and a major flood occurs, you will be given a 45,000 talers claims payment by the insurer to compensate you for the loss suffered. The chance of experiencing a flood over ***the next year is 1% [the next 30 years for your home is 26%]****.*

Each round involves a new decision on a house that is worth 90,000 talers with a savings account containing 5,000 talers. That is, at the end of each round, all your assets and wealth will be reset.

Scenario for Study 3

You own a house that is currently worth <u>80,000</u> talers and have a savings account currently containing <u>15,000</u> talers. There is a chance that a major flood will occur during the year in which case you will suffer severe damage and the value of the house will decrease by 35,000 talers (to a value of <u>45,000</u> talers). If you have purchased insurance for the year (at a cost of 350 talers) and a major flood occurs, the insurer will cover all your repair costs, and your house value will be fully restored to its current value of <u>80,000</u> talers. ***The chance of experiencing a flood over the next 1 year for your home is 1%. [The chance of experiencing at least one flood over the next 30 years for your home is 26%.]***

[Note: Underlined amounts updated each round based on remaining wealth from decisions.]

Comprehension check questions for Study 1

The first question was “Imagine that you chose the LOTTERY and it turned out that you lost money. How much money would you have at the end of the round?” [options: $95, $50, $25, $10]. The second question was, “Imagine you are selected to win the bonus and in the round that was chosen you had selected the CERTAIN LOSS. How much would the bonus for that round be worth?” [options: $95, $94.55, $50, $49.55]. The third question depended on which probability frame was presented. For the 1-play frame, participants answered “What is the probability of losing money at least once if you play the LOTTERY one time?” [options: 0.01%, 0.1%, 1%, 10%]. Those with the 30-play frame saw the following question: “What is the probability of losing money at least once if you play the LOTTERY 30 times?” [options: 20%, 24%, 26%, 30%]. Participants had to get all three questions correct before being allowed to proceed to the experiment. They were given two chances to correctly answer the questions. If they failed to answer at least one question correctly the first time, they were sent to the beginning of the instructions and asked to re-read them again before being allowed to answer the questions a second time.

Comprehension check questions for Study 2

The first question was “Imagine that there is a flood and you did not purchase insurance. How many talers is your house worth at the end of the round?” [options: 90,000 talers, 45,000 talers, 25,000 talers, 10,000 talers]. The second question was, “Imagine you are selected to win the bonus and in the round that was chosen you had purchased insurance at a premium of 450 talers and there was a flood. How much would the bonus be worth?” [options: $95, $94.55, $45, $44.55]. The third question depended on which probability frame was presented. For the 1-year frame, participants answered “What is the probability of experiencing a flood over the next year?” [options: 0.01%, 0.1%, 1%, 10%]. Those with the 30-year frame saw the following question: “What is the probability of experiencing a flood over the next 30 years?” [options: 20%, 24%, 26%, 30%]. Like in Study 1, participants had to get all three questions correct before being allowed to proceed to the experiment, and they were given two chances to correctly answer the questions.

Comprehension check questions for Study 3

The first question was “If a flood occurs, how much damage will it cause to your house? (That is, by how much will your house value be reduced?)” [options: 80,000 talers, 35,000 talers, 20,000 talers, 15,000 talers]. The second question was, “What is the cost of one year of flood insurance?” [options: [premium] talers, 100 talers, [premium + 300] talers, 200 talers]. The third question depended on which probability frame was presented. For the 1-year frame, participants answered “What is the probability of experiencing a flood over the next year?” [options: 0.01%, 0.1%, 1%, 10%]. Those with the 30-year frame saw the following question: “What is the probability of experiencing a flood over the next 30 years?” [options: 20%, 24%, 26%, 30%]. They were given two chances to correctly answer the questions. Participants were allowed to proceed to the experiment even if they failed the comprehension check, but only after they were informed of the answers to the questions they answered incorrectly. Only those who passed the comprehension check were included in the analysis.

Comprehension check questions for Study 4

The comprehension check questions were the same as for Study 2 except that participants in the 1-and-30 year condition anwered both versions of the third question. They were given two chances to correctly answer the questions. Participants were allowed to proceed to the experiment even if they failed the comprehension check, but only after they were informed of the answers to the questions they answered incorrectly. Only those who passed the comprehension check were included in the analysis.

Comprehension check questions for Study 5

The comprehension check questions were the same as for Study 3 except that the third question, which depended on condition, had three additional variations: For the 5-year frame, participants answered “What is the probability of experiencing at least one flood over the next 5 years?” [options: 0.05%, 0.5%, 5%, 15%]; for the 10-year frame, participants answered “What is the probability of experiencing at least one flood over the next 10 years?” [options: 1%, 5%, 10%, 15%]; and for the 200-year frame, participants answered “What is the probability of experiencing at least one flood over the next 200 years?” [options: 26%, 63%, 87%, 99%]. They were given two chances to correctly answer the questions. Participants were allowed to proceed to the experiment even if they failed the comprehension check, but only after they were informed of the answers to the questions they answered incorrectly. Only those who passed the comprehension check were included in the analysis.

Comprehension check questions for Study 6

The first two comprehension check questions were the same as for Study 3 and 5. The third question was “What is the probability of experiencing a flood over the next [length of assigned time horizon]?”, with options varying depending on the probability associated with the assigned time horizon [options: [probability + 5 percentage points]%, [probability]/10 %, [probability]%, [probability – probability/4]%]. They were given two chances to correctly answer the questions. Participants were allowed to proceed to the experiment even if they failed the comprehension check, but only after they were informed of the answers to the questions they answered incorrectly. Only those who passed the comprehension check were included in the analysis.

Additional measures for Study 2

We assessed experience with flood insurance by asking the following question: "How experienced are you in buying flood insurance?" The options were: (1) I have never purchased flood insurance and do not know anything about it; (2) I have never purchased flood insurance but am familiar with it; (3) I have purchased flood insurance several times before; (4) I purchase flood insurance regularly. We also assessed whether people had experienced flood damage previously by asking the following question: "Have you had prior experience in suffering flood-related losses from hurricanes or floods?" The options were: (1) I do not know anyone who has suffered a flood-related loss from a hurricane or flood; (2) I know someone (such as a friend, family member, etc.) who suffered a flood-related loss from a hurricane or flood but I have not suffered a loss myself; (3) I suffered a flood-related loss from a hurricane or flood once; (4) I suffered a flood-related loss from a hurricane or flood several times.

Additional measures for Study 3

We assessed understanding of cumulative probabilities with the following question: "Please answer the following questions based on your own knowledge. For each one, enter a number between 0-100. Round to the nearest whole number (do not include decimals or percent (%) signs). (1) If the answer to the question was explicitly provided to you in the scenario, type in that number. (2) If it was not provided to you, please just use your own intuition to make an educated guess. In the scenario, what was the percent chance of: (a) a flood occurring in the next 1 year? (b) at least one flood occurring of the next ten years? (c) at least one flood occurring over the next 30 years? (d) at least one flood occurring over the next 100 years? (e) at least one flood occurring over the next 200 years?" [All responses were entered into text boxes that limited responses to be between 0-100.]

We assessed preference over time horizons with the following question: "In the box below, type in the number that best completes the following sentence for you: 'I would prefer to know the probability of flood over the next _______ year(s).' (Type only numbers.)" [Text box entry allowed only numbers between 1 and 1000.]

List of counties used for Study 4

State	County
Florida	Walton County
Florida	Broward County
Florida	Indian River County
Louisiana	St. Bernard Parish
Louisiana	Orleans Parish
Louisiana	St. Tammany Parish
Louisiana	St. John the Baptist Parish
Louisiana	Plaquemines Parish
Louisiana	Terrebonne Parish
Maryland	Worcester County
New Jersey	Somerset County
Texas	Galveston County
Texas	Brazoria County
Virginia	Virginia Beach city

Data on insurance take-up in each county can be found on the OSF page at: https://osf.io/mejf5/?view_only=9b8760dca9714e85bc178f290140125e