Mis-Remembrance of Things Past: Memory and Adaptation to Climate Change

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Abstract

Human memory is imperfect: we often fail to recall true events and invent false experiences. I examine the economic consequences of these cognitive limits for adaptation to climate change in rural Bangladesh. I elicit environmental memories, beliefs, and adaptation decisions from 2,279 farmers across three survey waves. Using both natural variation in daily weather and a series of experiments embedded in the survey design, I test the predictions of a recall-driven belief formation model featuring endogenous memory revision. I find evidence that the contemporaneous context causes memory reconstruction, distorting remembered features of past experiences. Memories impact both expectations about future environmental conditions and economic decisions pertaining to climate change adaptation.

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> "And it never failed that during the dry years the people forgot about the rich years, and during the wet years they lost all memory of the dry years. It was always that way."
>
> —*East of Eden*, John Steinbeck

1 Introduction

Our perceptions of the world around us drive our decision-making. These beliefs especially impact the well-being of households living in low- and middle-income countries, where significant information frictions limit access to the ground truth. From school quality (Andrabi et al., 2017) to prices (Jensen, 2007), medicine (Sievert, 2024) to migration (Baseler, 2023), a rich body of work documents the critical role expectations play in important economic choices. In the absence of reliable information on the true state of the world, what underpins these beliefs?

I examine one potential channel: memories. Our own experiences provide a natural source of data for learning about the unknown. Yet in contrast to a computer database seamlessly logging and retrieving information, human memory exhibits cognitive limits that systematically alter the ways in which we store past events in our mind and remember them during moments of need. We often fail to recall true aspects of our experiences and invent details that never happened, and what's more, commit these memory failures in predictable ways (Schacter, 1999; Kahana, 2012).

This paper examines the economic consequences of human memory. I begin by incorporating the cognitive regularities highlighted by psychologists into an economic model of belief formation and decision-making. In particular, I allow for *constructive* memory (Bartlett, 1932): the imperfect encoding of details about past experiences that our minds endogenously revise, thereby creating false perceptions of the past. The model describes a clear, causal chain: contemporaneous context—features of the setting in which the decision-maker forms expectations—shape both which experiences come to mind and the content of those recalled memories, which in turn impact the beliefs that ultimately drive economic behavior. I then take the predictions of this framework to the high-stakes setting of rural small-holder farmers learning about and adapting to environmental threats. Equipped with data I collect on memories, beliefs, and decisions for 2,279 farmers across 250 villages in Bangladesh, I use a collage of experiments embedded in the survey design and naturally occurring variation in weather to test the predictions of the model. I find evidence that memories endogenously respond to stimuli in line with the psychology literature, systematically shaping both beliefs and important economic decisions—in this case, the adoption of technology designed to address global warming's most severe threats.

The simple conceptual framework at the heart of this story illustrates the cognitive pitfalls on the path from initial experience to retrieved memory as an input to expectations and ultimately behavior. Closely following related work in economics (Bordalo et al., 2023), an assumption about similarity serves as the key engine driving the model, whereby our minds more easily consider notions more similar to those already top of mind. Consider a farmer choosing whether or not to purchase flood insurance. Their beliefs about the likelihood of severe flooding enter as a key primitive in that decision-making process. Farmers form these beliefs amid a contemporary context which shapes the set of memories which come to mind. In my framework, this contextual environment not only changes which memories come to mind but also the *content* of those recollections, differing from previous economic models of memory by incorporating constructive memory into the cognitive process such that the mind endogenously revises features of past experiences. The similarity of the context and set of potential features modulates how our brain edits past experiences, closely related to the simulation mechanism of Bordalo et al. (2024). For instance, a farmer may not store the exact number of days a past flood remained on their land, and thus if prompted to think about devastation and destruction from long-lasting inundation by a motivated insurance salesperson, the farmer may simulate the missing feature of duration as a particularly lengthy flood event. I also allow the context to have direct association effects on beliefs independently of their role in shaping memories to account for distinct forces such as taste-based projection bias (Loewenstein et al., 2003).

The model predicts a causal chain from the contemporaneous context faced by the decision-maker to the ultimate economic choice. Under constructive memory, the setting of the moment of recollection alters not only which experiences the decision-maker remembers but also the content of those memories. The framework suggests three key steps: (1) context impact the nature of recalled events, (2) memories shape expectations, and (3) beliefs alter decisions.

To test the assumptions and predictions of this model, I collect three waves of surveys with rice farmers in rural Bangladesh, combining several survey experiments and a quasi-experiment arising from weather fluctuations. All of the treatments I consider echo the natural variation in context that likely shape expectations in farmers' day-to-day lives. The villages in my sample span a wide area, generating natural variation in climate experiences and weather that allow me to isolate the impact of context and memories in shaping farmers' beliefs about future environmental conditions and adaptation decisions. I focus on three dimensions of the local environment that directly shape agricultural production decisions: flooding, soil salinity, and monsoon intensity. For flooding and soil salinity, I speak to the economic consequences of memory by eliciting willingness-to-pay in an incentive-compatible manner for two climate adaptation goods directly downstream of beliefs: salinity-tolerant rice seed varieties and a flood insurance contract that I offer to farmers.

I do not have a single design that captures all aspects of the model I aim to test; rather I present a collage of evidence using many natural and survey experiments. I therefore structure the empirical results around each of these three environmental settings—flooding, soil salinity, and then rainfall—in order to keep the discussion of related data compact. I now preview the results of each.

I first use repeated elicitation of flood memories for the same farmer to test the model's predictions about feature reconstruction. Farmers' recollection demonstrate substantial inconsistency over time: across several different methods of identifying the same flood, farmers remember a given experience again less than half of the time. For instance, comparing two survey waves when the same farmer recalls every flood they can, a respondent only reports a flood in the same year as their previous answer about a quarter of the time. Pairing experiences based on other memory features such as duration and damage yield similarly low match rates. Consistent with the theory, experiences with more salient features prompt more consistent recall. For instance, farmers are at least 50% more likely to consistently remember a flood that caused property damage, all else equal. Salient features are also more consistently remembered even conditional on the same overall flood experience being recalled twice, consistent with stronger encoding and storage of especially distinctive characteristics.

I next show that the contemporaneous context alters memories in line with the framework's predictions about endogenous revision. First, I use variation in whether it happens to rain on the day a farmer answers survey questions as a natural experiment providing quasi-random shocks to context. The model predicts that a decision-maker recounting memories on two days with the same context—in this case, rainfall status—will recount more similar memories on those days. In line with that theoretical result, farmers interviewed across days with matching rainfall status have at least 10 percent more successful flood memory matches. These results are driven by two rainy days as opposed to two dry ones, consistent with rainy days being more rare and therefore a stronger contextual cue.

Contemporaneous rain also alters both which memories come to mind and the content of those memories, as predicted by the model's associative recall and reconstruction predictions. Conditional on a farmer fixed effect, rainy day interview produce flood memories with longer inundation and greater harvest damage, suggesting farmers view rainfall as more similar to harmful floods and lingering water. The total number of recalled floods falls, consistent with an interference effect in which these more destructive floods crowd out less damaging ones. As a complementary exercise to examine feature reconstruction, I examine how the first flood recounted by farmers shapes the second and subsequent memories, treating the telling of the first memory to the enumerator as the context. To isolate variation in that context, I randomize the order in which farmers recount floods (oldest vs. most recent first) and, conditional on a farmer fixed effect, examine how the answers of neighbors facing the same answer order shape a respondents' subsequent flood memories. In line with the previous evidence, I find reconstruction: for instance, a one-day longer flood reported by neighbors who recalled memories in the same order leads to remembering a 0.57-day longer second flood. A given feature of the first remembered flood also impacts other features of subsequent floods, and consistent with the model, similarity mediates these spillovers. I find that features more similar to another as measured by their covariance among first-reported floods are also more impacted by contextual deviations in that feature.

To conclude the floods section of the paper, I present results from an information experiment designed to test the causal impact of contextual shocks to memories on behavior. I randomly provide farmers with information about floods elsewhere in the country and their damages, either before I elicit flooding memories or after. A pure control group receives no information. Farmers receiving information after they recount floods exhibit no different incentivized demand for a flood insurance contract than those in the control group. Farmers told only about the existence of floods in another region prior to their recall show lower demand, perhaps because the context reminds them of their ability to cope with floods without insurance previously. Adding in details about floods' destructive power more than offsets this impact, however. Consistent with the information treatments only impacting willingness-to-pay when provided before memory elicitation, I find evidence that damage information alters the floods people remember, increasing the amount of crop damage per memory by 14.8% and the mean duration by nearly two days.

I next turn to the case of soil salinity as a further test of the causal impact of memory on beliefs and climate adaptation decisions. I elicit both farmers' recollections about salinity trends over the past decade and their forecasts about the ensuing one. I randomize the order in which I ask these questions: in each wave, half of respondents answer the past then the future, while the other half answer about the future first then the past. Analyzing this survey experiment, I find evidence of memories impacting expectations. Using a difference-in-differences design comparing farmers (1) predicted to remember past salinity declines vs. not and (2) asked about the past first vs. second, I find that recounting salinity trends over the last 10 years first when those memories are predicted to show decreases makes farmers significantly more likely to expect decreases in the future—by 10.1% to 27.9% depending on the specification. These results hold after including farmer and village-by-survey wave fixed effects and under several different ways of predicting past memories. Later on in the survey, I measure incentivized demand for a salinity tolerant seed. The treatment effects of memory persist, causing a decline of 6.8% to 29.9% among farmers asked about the past first in past-declining villages, depending on the specification. The reverse experiment—being asked about the future before the past—shows null results, consistent with successful encoding and storage of memories reducing scope for simulation of the past.

Finally, I examine monsoon intensity to study the model's prediction that contemporaneous context will play a stronger reconstructive role when a memory's features have weaker encoding and storage. I compare memories of more recent vs. older monsoon seasons to capture variation in the strength of feature retention. Again using natural variation from rain on the day of the interview with the farmer, I show that rain on the survey date increases the amount of rain one remembers in the past by more when the remembered year occurred longer ago, conditional on a farmer fixed effect. The contemporaneous rain impact is 19.9% higher for a 10-year older memory. This memory revision process provides a complementary channel through which weather can impact decisions to projection bias and salience as in Busse et al. (2015).

Together, these empirical results describe a belief formation and decision-making process shaped by memories and subject to the cognitive limitations plaguing human recall, even in this high-stakes setting where economic livelihoods directly depend on farmers' ability to accurately perceive the environmental conditions they face.

Related Literature This paper relates most closely to two strands of research. First, I build on a set of papers examining the role of memory in economic decisions. Following a long literature in psychology, economists have increasingly focused on memory as an important determinant of beliefs (Mullainathan, 2002; Gennaioli and Shleifer, 2010; Bordalo et al., 2020, 2023; Fudenberg et al., 2024). The model in this paper incorporates the psychological notion of constructive memory formally into the expectation formation process, applying the principle of similarity-based simulation from Bordalo et al. (2024) to the content of memories themselves, and thereby proposing an additional channel through which contextual cues shape beliefs above and beyond the contextual association of the experience itself (Conlon and Kwon, 2025; Wachter and Kahana, 2024). I then provide a series of empirical tests of this memory process. Experiments from lab settings (e.g., Enke et al. 2023) have been recently complemented with an expanding body of field evidence illustrating the importance of memory in a range of settings, including financial markets (Jiang et al., 2025), schools (Miserocchi, 2023), health (Das et al., 2012), privacy (Hnilo and Bauer, 2026), fertility (Müller, 2022), retail consumption (Bordalo et al., 2026), inflation (Taubinsky et al., Forthcoming), and the labor force (Pires, 2022). This paper relates most closely to work illustrating the economic consequences of cognitive features of human recall. For instance, failures in retrieving information about irregular expenditures bias Zambian farmers' predictions about their budget, reducing consumption smoothing (Augenblick et al., 2024). In another example, trauma-induced memory distortions inhibit refugees in Ethiopia and post-conflict and displaced persons in Colombia from effectively simulating the future and making corresponding investments (Ashraf et al., 2025). In a similar setting to this paper, Naso (2025) studies recall about rainfall, agricultural output, and crop prices in Burundi, and presents evidence of interference: political events unrelated to agriculture increase recall error for maize and bean prices. My paper directly tests for the causal impact of memory in a setting where I can directly elicit what farmers remember and use both natural quasi-random variation in the weather and survey experiments to test the predictions of a recall model. Incentivized measures of climate adaptation allow me to establish the economic stakes involved.

Second, this paper provides evidence on the underlying determinants of adaptation to climate change (see Carleton et al. 2024 for a review). Existing work has examined a host of important frictions to individual investment in adaptation, including credit constraints (Lane, 2022), information asymmetries (Beaman et al., 2014; Mahadevan et al., 2023), financial market imperfections (Karlan et al., 2014), supply-side frictions (Emerick et al., 2016), and market structures (Bhandari et al., 2022). A growing set of evidence has established the importance of beliefs (Kala, 2019; Zappalà, 2024; Burlig et al., 2026; Patel, 2026a), and this paper highlights the role of memory in shaping these expectations about important environmental threats.

2 Belief Formation Model with Constructive Memory

To guide the empirical analysis that follows, this section presents a simple framework of belief formation featuring imperfect recall and the constructive nature of memory. This model is designed to present a formal mathematical description of the psychological mechanisms I wish to study, rather than present a new set of theoretical results. I highlight two key forces in the framework. First, the contemporaneous context shifts which past experiences come to mind, biasing recall toward context-consistent experiences. Second, I allow the individual to potentially omit details when encoding past experiences and in turn use the context of belief formation to guide the reconstruction of those missing features. As a result, context changes not only which experiences become successfully recalled but also the content of those memories. This latter mechanism marks the key departure from other models focusing primarily on selective recall such as Bordalo et al. (2023): I endogenize the content of retrieved experiences such that the same experience recalled in two different contexts can have different reconstructed features and therefore distinct impacts on beliefs. Figure 1 presents the overall structure of the model, which contains two main parts. First, the individual accumulates and stores experiences (perhaps imperfectly) in memory. Second, amid the context of making a decision, they retrieve these memories to form beliefs.

2.1 Set-Up

Decision Problem A decision maker (DM) makes a binary choice $d \in \{0,1\}$, such as whether to purchase flood insurance. Utility from this decision hinges on the state of the world $\omega \in \{0,1\}$, such as whether a devastating flood will strike in the coming season. Denote the DM's utility function by $U(d,\omega)$. I impose no Pareto dominance such that utility satisfies both of the following conditions: $U(1,1) > U(0,1)$—the DM would rather have insurance amid a flood than not have insurance, and $U(0,0) > U(1,0)$—the DM would rather not have insurance than have insurance in the absence of a flood.

Role of Beliefs Let θ denote the true probability that $\omega = 1$, such as the true chance of a flood next year. The DM cannot perfectly observe the likelihood of this event, and instead forms a belief $\widehat{\theta}$ which captures their perceived probability of $\omega = 1$. The DM maximizes expected utility given their beliefs. Ruling out cases in which either $d = 0$ or $d = 1$ dominates in all states of the world, the optimal decision $d^\star$ hinges on whether beliefs exceed a threshold $\theta^\star$ so that $d^\star(\widehat{\theta}) = \mathbb{1}\{\widehat{\theta} \geq \theta^\star\}$.[1] In the running example, a DM will choose to purchase flood insurance when they perceive a sufficiently high risk of flooding.

Features A set $\mathcal{F} = \{1,\ldots,F\}$ of $F > 1$ features describes each state of the world, such as the length of time water sits on the land and the amount of crop damage. Let $\overline{\mathcal{X}} \equiv \{0,1\}^F$ denote the unrestricted binary feature space. I allow the environment to rule out logically inconsistent feature vectors, such as a week-long flood with no standing water. Let $\mathcal{X} \subseteq \overline{\mathcal{X}}$ denote the feasible feature space. A generic feasible object $x \in \mathcal{X}$ has coordinates $x = (x_1,\ldots,x_F)$ where $x_f \in \{0,1\}$.

Three key objects—hypotheses, experiences, and contexts—each lie in the same feature space $\mathcal{X}$.

Hypotheses Let $H \in \mathcal{X}$ denote the feature representation of the hypothesis that $\omega = 1$. In the flood-insurance example, H represents the feature configuration associated with a

[1]Specifically, this threshold is $\theta^\star = [U(0,0) - U(1,0)]/([U(1,1) - U(0,1)] + [U(0,0) - U(1,0)]) \in (0,1)$.

damaging flood. This hypothesis has features describing the state of the world $\omega = 1$, such as an indicator for standing water lasting more than one week during a bad flood.

Experiences Suppose each DM has $N \geq 2$ past experiences, such as all of the monsoon seasons they have previously experienced on their land. Each experience $e_n \in \mathcal{X}$ contains the realized features of an event, such as the nature of flooding on the DM's land during the rains. Let $E = (e_1, \ldots, e_N) \in \mathcal{X}^N$ denote the database of all experiences.

Contexts Each DM forms beliefs $\widehat{\theta}$ at the moment of decision-making amid a contemporaneous context $c \in \mathcal{X}$, such as the contextual setting of the day they make their prediction about flood risk and decide whether to purchase insurance. Contexts share the same structure as past experiences and hypotheses, also consisting of a set of binary features such as rain on the day of the survey.

2.2 Memory and Belief Formation

A DM endowed with experiences E facing context c forms belief $\widehat{\theta}$ about hypothesis H to inform their decision d. I now incorporate cognitive features of human memory into this belief formation process to embed three stages: encoding and storage, retrieval and reconstruction, and aggregation. The processes I describe below may have evolutionary underpinnings that may even make them lead to more efficient outcomes—I take no normative stance on this framework.

Encoding and Storage I first introduce encoding—the process by which an experience becomes initially stored into memory—and storage—maintaining that information over time. I allow for forgotten details as the DM imperfectly records and stores past experiences, such that feature values can now contain the empty set $\emptyset$. For example, a flood experienced by the DM includes the timing, duration, water height, and damages from the inundation. Yet the DM may not store the height of the flood waters in their memory. Define the space of memory traces by $\mathcal{T} = \{0, 1, \emptyset\}^F$. After experience e_n occurs, the DM encodes and stores a memory trace $m_n \in \mathcal{T}$. This trace reflects a potentially incomplete subset of features from the original experience.

For each experience n and feature f, let σ_{nf} denote the salience or emotional intensity of feature f in experience n. Let Z_{nf} collect other determinants of storage, such as attention, time since encoding, or measurement conditions. I model retention as a probabilistic event: $R_{nf} \mid \sigma_{nf}, Z_{nf} \sim \text{Bernoulli}\big(\rho(\sigma_{nf}, Z_{nf})\big)$, where $\partial\rho(\sigma_{nf}, Z_{nf})/\partial\sigma_{nf} > 0$. Thus more salient or emotionally intense features survive encoding and storage with higher probability.

Given the retention indicators, the stored memory trace $m_n \in \mathcal{T}$ satisfies Equation (1).

$$m_{nf} = \begin{cases} e_{nf}, & \text{if } R_{nf} = 1, \\ \emptyset, & \text{if } R_{nf} = 0. \end{cases} \tag{1}$$

I distinguish feature-level salience, which governs storage, from event-level salience, which governs baseline accessibility at retrieval. Let $\sigma_n \equiv (\sigma_{n1}, \ldots, \sigma_{nF})$ denote the salience profile of experience n. Let $A_n = A(\sigma_n, W_n)$ denote event-level salience, where W_n collects event-level determinants of accessibility such as recency, frequency, or emotional intensity. I assume that A_n weakly increases in each feature-level salience coordinate $\partial A_n / \partial \sigma_{nf} \geq 0$.

Similarity Similarity serves as the key engine driving the model. For any two feature vectors $x, y \in \mathcal{T}$, I define similarity to capture the representational proximity between objects x and y in context c.[2] An extensive literature in cognitive science has studied this similarity function.[3] This body of work has documented a context-dependent nature of similarity in which objects relate to one another based on how their features fall in an underlying psychological space rather than based on literal overlap in observed attributes.

To operationalize that idea in my model, I define a context-dependent embedding function $\psi : \mathcal{T} \times \mathcal{X} \to \mathbb{R}^K$, where $x \in \mathcal{X}$, $\psi(x, c)$ gives the location of x in a K-dimensional latent conceptual space under context c. This function captures a reduced-form representation of the underlying cognitive process.[4] Note that even when two objects share no literal common attributes, they may still be psychologically close if they map to nearby points in latent space—a relationship which critically depends on the context in which that object is observed. As one piece of evidence supporting this conceptualization of similarity in my empirical context as opposed to a feature matching one, I find that 24.62% of farmers in my sample believe that spraying sugar on their plots helps to combat soil salinity. Despite the absence of any agronomic benefit of this practice, the semantic link between sugar as a means to neutralize salt illustrates the relevance of this kind of similarity function in this setting. This also allows similarity to operate even with missing features in a memory trace by operating only on the non-missing coordinates yet still allowing for conceptual closeness. In the running example, a rainy day and a past flood may be close because they lie near one another in latent conceptual space, such as dimensions related to water exposure or

[2] I define similarity over $\mathcal{T}$ to allow similarity to operate on memory traces, but note that $\mathcal{X} \subset \mathcal{T}$.

[3] See, for instance, Tversky (1977), Nosofsky (1986), Medin et al. (1993), Richie and Bhatia (2021), and Kaushik and Thompson (2026).

[4] In the empirical tests that follow, I explicitly define how the data map into a notion of similarity I can measure.

inundation risk. A DM may conceive of a rainy day in the middle of the monsoon season differently than one amid a drought.

I therefore define the similarity between any two objects $x, y \in \mathcal{T}$ conditional on context c by Equation (2), where the parameter $\lambda > 0$ governs how quickly similarity decays with distance in the context-dependent latent conceptual space. The sign predictions I take to the data do not require the exact exponential kernel in Equation (2), only that recall and reconstruction are increasing in contextual similarity.

$$S_c(x, y) = \exp\bigl(-\lambda \left\| \left[\psi(x, c) - \psi(y, c)\right] \right\|_2\bigr) \qquad \lambda > 0 \tag{2}$$

Retrieval At the moment of belief formation about amid context c, the DM turns to their memories to calculate their expectation. Retrieval consists of two key components: recall and reconstruction.

First, the DM recalls experiences from their experience database with a probability based on two objects: a baseline accessibility of trace m_n denoted by $\kappa_n > 0$, and the similarity of the trace to the context $S_c(m_n, c)$. I link baseline accessibility to event-level salience by assuming $\kappa_n = K(A_n, W_n)$ where $\partial K(A_n, W_n)/\partial A_n > 0$. The vector W_n may include recency, frequency, or other determinants of accessibility. Equation (3) captures this probability, where $\alpha \geq 0$ governs the sensitivity of recall to contextual similarity.

$$r_n(c) = \frac{\kappa_n \exp\bigl\{\alpha S_c(m_n, c)\bigr\}}{\sum_{j=1}^{N} \kappa_j \exp\bigl\{\alpha S_c(m_j, c)\bigr\}} \tag{3}$$

Since $\sum_{n=1}^{N} r_n(c) = 1$, $r_n(c)$ denotes the probability that a single recall draw selects trace m_n, or equivalently the recall share allocated to trace m_n.

In the running example, this expression predicts, for instance, that all else being equal, the DM will recall a recent flood more often than one longer ago due to a higher κ. Given two experiences with comparable baseline accessibility, the one more similar to the context in which the DM forms beliefs will more easily come to mind. Note the denominator of Equation (3) captures the well-documented psychological feature of interference (Kahana, 2012), as the ease of recalling experience i crowds out the recall of experience j.

Second, during reconstruction, the DM simulates any missing features of the trace based on their similarity to the context, "filling in the gaps" of their memory. For example, a farmer may not have retained precise details on the duration of a past flood. After listening to a pushy insurance salesperson, the farmer recalls their previous experience and simulates a flood length. Because the salesperson has emphasized damaging inundation events, the farmer endogenously revises the length to be long because longer floods lie are more similar to the belief formation context induced by the salesperson.

Formally, for any trace $m \in \mathcal{T}$, let $\mathcal{O}(m) \equiv \{f \in \mathcal{F} : m_f \neq \emptyset\}$ denote the set of retained coordinates. Define the set of feasible completions of m by $\mathcal{C}(m) \equiv \{x \in \mathcal{X} : x_f = m_f \text{ for all } f \in \mathcal{O}(m)\}$.[5] I assume $\mathcal{C}(m_n) \neq \varnothing$ for every trace that arises in the model. The DM replaces each missing feature $\mathcal{F} \setminus \mathcal{O}_n$ with a value $\{0, 1\}$. I model retrieval as a constructive process similar to simulating the future to reflect recent evidence from neuroimaging studies suggesting a close link between the two (Schacter and Addis, 2007). Let $\tilde{e} \in \mathcal{C}(m_n)$ denote a reconstructed experience, and let $\nu(\tilde{e}|m_n) \geq 0$ capture a baseline propensity to reconstruct $\tilde{e}$ given the non-missing features in m_n. I impose $\operatorname{supp} \nu(\cdot \mid m) \subseteq \mathcal{C}(m)$. The object $\nu(\cdot \mid m)$ captures semantic, logical, or experiential priors over feasible completions. I assume $\sum_{\tilde{e} \in \mathcal{C}(m_n)} \nu(\tilde{e}|m_n) = 1$. Conditional on recalling trace m_n in context c, Equation (4) gives the probability that the DM reconstructs experience $\tilde{e} \in \mathcal{C}(m_n)$, where $\tau \geq 0$ indexes the extent to which the DM distorts reconstruction toward context-congruent completions. This similarity-based simulation closely follows the psychological channel through which people imagine to generate predictions about the future from Bordalo et al. (2024).[6]

$$g(\tilde{e} \mid m_n, c) = \frac{\nu(\tilde{e}|m_n) \exp\big\{\tau S_c(\tilde{e}, c)\big\}}{\sum_{x \in \mathcal{C}(m_n)} \nu(x|m_n) \exp\big\{\tau S_c(x, c)\big\}} \qquad \tau \geq 0 \tag{4}$$

Belief Formation Putting these pieces together, belief formation takes place in three core steps, fueled by the context c in which the DM forms beliefs $\widehat{\theta}$. First, the context shapes which memory traces the DM retrieves, distorting recall towards more similar experiences. Second, conditional on retrieval, the DM simulates missing features, again favoring those that make the reconstructed memory more similar to the context. Third, the DM aggregates reconstructed memories to assess support for the hypothesis.

Equation (5) presents this process. The DM draws repeated memories from their experience bank based on the context-driven recall and reconstruction probabilities. I assume that the DM assigns greater probability to H based on the similarity between the retrieved memories and the hypothesis. I also allow for the contemporaneous context c to have a direct effect on beliefs where $\gamma \in [0, 1]$ governs the weight on this channel relative to memory. This can be viewed as a reduced form object capturing the informational content of the current experience, as well as a host of other channels such as preference-based projection bias or

[5]This helps to rule out internally inconsistent memories like week-long floods with no standing water.

[6]For incomplete traces $m \in \mathcal{T}$, I extend the embedding in the similarity function by the baseline average over feasible completions $\psi(m, c) = \sum_{x \in \mathcal{C}(m)} \nu(x \mid m)\psi(x, c)$. This extension lets similarity operate on incomplete traces while ensuring that missing entries enter through the feasible completions of the trace.

priming (Loewenstein et al., 2003).

$$\widehat{\theta}(c) = \gamma S_c(c, H) + (1-\gamma) \sum_{n=1}^{N} r_n(c) \sum_{\tilde{e} \in \mathcal{C}(m_n)} g(\tilde{e} \mid m_n, c)\, S_c(\tilde{e}, H) \tag{5}$$

To summarize, the contemporaneous context in this model can influence beliefs via three channels: which memories the DM retrieves, the content of those recalled memories via reconstruction, and a non-memory direct effect.

2.3 Empirical Tests

This section presents the main assumptions and predictions of the model that I take to the data in the next section. All proofs (even those that follow trivially from the definitions above) can be found in the appendix.

Prediction 1: Experience salience increases recall. All else equal, greater event salience A_n increases probability the DM recalls that experience.

Prediction 2: Salience increases feature-level storage. All else equal, a more salient feature f in an experience n will be encoded and stored with greater likelihood.

Prediction 3: Contextual shocks increase recall of similar events, crowding out dissimilar. All else equal, a context that makes experience i more similar to the moment of belief formation increases the probability the DM recalls that experience, crowding out less similar memories.

Prediction 4: Constructive reconstruction moves memories toward the context. All else equal, conditional on recalling an incomplete trace m_n, the reconstructed feasible completion $\tilde{e} \in \mathcal{C}(m_n)$ will be more similar to the context of belief formation than a completion drawn from the baseline reconstruction distribution.

Prediction 5: Weaker storage amplifies contextual reconstruction. All else equal, conditional on recalling trace m_n, context has a larger effect on reported memory content when feature f is less likely to have been successfully stored.

Prediction 6: Memory can shift beliefs. All else equal, context-induced changes in recall and reconstruction shift beliefs whenever they change the memory-based support for the hypothesis H.

Prediction 7: Memory can shape decision-making. Memory can shape decision-making when DMs are sufficiently close to their decision thresholds.

3 Empirical Tests of Constructive Memory

I now take the predictions of this simple model to the data using a series of natural and survey experiments with farmers in Bangladesh on the front lines of climate change. I focus on contextual shocks that mimic those facing farmers in their day-to-day lives: simple survey questions about what they remember, news they might hear about in conversations, and the weather. I begin by describing the sample and then turn to a collage of tests of the framework's predictions across three environmental domains: flooding, soil salinity, and monsoon intensity. I organize the results by setting to keep data descriptions compact. Appendix Section D presents robustness checks for the results that follow.

Sample The results that follow stem from a three-wave panel of in-person household surveys and accompanying experiments I conducted with 2,279 dry season rice farmers across 250 villages in the Khulna Division of Bangladesh. Appendix Table A.1 presents summary statistics on farmer demographics. Notably, the median years spent farming in my sample is two decades; these farmers have extensive decision-making experience in the domains I examine. In the estimating equations that follow, let i index farmers, t index survey round, and $v(i)$ capture the village of farmer i. These villages span a large area amounting to approximately 15% of the surface area of Bangladesh, providing substantial natural variation in experiences and climate shocks across farmers. For more details on the survey sampling, see Appendix Section B.1. See Appendix Section B.2 for the full text of the survey questions used in the analyses below.

3.1 Flooding

I begin by examining memory and floods. Floods provide an especially appealing context for studying constructive memory for two main reasons. First, floods—the world's most common natural disaster—naturally exhibit rich variation across important features: for example, inundation events vary in length and the scope of damage they cause. Second, they present a first-order concern for farmers. Amid a lack of access to official estimates of flood risk, households must rely largely on their own experiences to form beliefs and make important climate adaptation decisions.

Flood Memory Data For each farmer in my sample during each wave of the survey, I elicit every flood they have experienced. Prior to the recall exercise, I explicitly define a flood to respondents: each enumerator says, "By flood I mean unexpected and unwanted water enters your land or house and covers the ground. I only want you to think of a flood happening to you if it covers at least half of one of your plots, or the water is touching your house." Appendix Table A.8 shows the distribution of the number of flood memories recalled by farmers in each survey wave. Farmers remember at least one flood about 60 percent of the time. This exercise yields a total of 6,663 interviews in which I capture flood memories. For each experienced flood, I capture five features: when the flood occurred (year), the length of inundation (Likert scale of different lengths), whether the flood harmed the respondent's crops (and if so, the share of total harvest lost to flooding as a Likert scale), and whether the flood damaged the respondent's house (and if so, the amount of property damage in BDT). To reduce survey time, a random half of respondents in the first wave did not answer questions about harm for their recalled floods. These survey activities produce a final sample of 6,491 unique flood memories, 5,345 of which I have data on all five features. Appendix Table A.9 presents summary statistics on these features. The average recalled flood lasts nearly three weeks. Recalled floods cause tremendous damage: almost all harm agricultural production with an average harvest loss of 78%. Four out of 10 floods damage farmers' homes, with an average house damage per flood (unconditional) of nearly 12,000 BDT (about 100 USD).

Salience and Imperfect Recall I first examine Prediction 1, which posits stronger recall for memories with more salient features. I leverage the repeated elicitation of flood memories *within* farmer to overcome the lack of accurate ground truth data on individual exposure.[7] I identify which floods come to mind most consistently for the same farmer across rounds, then examine which features predict consistent recall. I estimate Equation (6), where $RecalledAgain_{i,j(t)}$ indicates whether farmer i recalled memory j from time t again during a later survey round, $X^f_{i,j(t)}$ indicates the value of feature f for that memory the first time it was recalled, and λ_i and ϕ_t denote farmer and survey wave fixed effects, respectively. I cluster standard errors at the farmer level.

$$RecalledAgain_{i,j(t)} = \sum \beta_f X^f_{i,j(t)} + \lambda_i + \phi_t + \varepsilon_{i,j} \tag{6}$$

The constructive nature of memory presents an important challenge for identifying the "same" memory over time: two memories elicited at different times may differ either because they capture distinct underlying flooding experiences or because they capture the same experience yet have reconstructed features that diverge. To address this, I construct six different definitions of "same flood" and examine predictors of recall across all of them. I classify a flood from an earlier survey round as matched if it satisfies the criteria with any

[7] Flood detection methods based on remote sensing such as the one I develop in Patel (2026b) cannot capture historical flooding at a household level well.

flood recalled in a later round. First, an "exact year" match requires the reported flood year to be identical across rounds, regardless of other features. Second and third, "year $\pm$ 1" and "year $\pm$ 2" matches allow the reported year to differ by up to one or two years, respectively. Fourth, a "weak feature" requires agreement on the indicators of whether the flood harmed crops and harmed the respondent's house, approximate agreement on the share of crops damaged (within 25%), and approximate agreement on flood duration (within 3 days). Fifth, a "strong feature" match requires exact agreement on whether crops and the house were harmed, exact agreement on the share of crops damaged and on flood duration, and agreement on the value of house damage within 25% of the larger reported value. Finally, a combined "year $\pm$1" and "strong feature" match require both criteria to be jointly satisfied.[8] I restrict the sample to floods whose reported year predates the survey year of the earlier round so that each flood in principle could be matched.

The same farmer may report different floods across rounds not because of fundamental features of memory but because of differential survey fatigue. To account for this, I report consistency statistics for three samples: all floods; those recalled by respondents who reported at least one matching-eligible flood in both the earlier and later round (ruling out cases where a respondent reported floods in one round but not the other); and those recalled by respondents who reported the same number of matching-eligible floods in both rounds.

Farmers' recollection of floods shows substantial inconsistency over time. Table 4 presents match rates across each definition of "same experience", separately by subsets of farmers to address potential survey fatigue. Under no definition do farmers recall more than half of their experiences multiple times. Farmers report a flood in the same year as their previous answer only about a quarter of the time. Feature-wise matches show strikingly low consistency rates—well below 10 percent for the strong feature match—suggesting substantial scope for memory reconstruction.

Consistent with the theory, experiences with more salient features prompt more consistent recall. Table 5 presents the results of estimating Equation (6). I omit features used to construct the matches—which would mechanically be linked—and therefore omit match definitions using all available features. In general, longer floods come to mind more easily and repeatedly. Crop damage, by contrast, does not predict consistent recall. This finding highlights the specific notion of salience at play in the model: because nearly all floods cause agricultural loss, the presence of crop damage—though destructive—does not make any individual flood stand out (Bordalo et al., 2022). Property damage, however, which has

[8] Year-based match definitions are computed only over floods with a reported flood year, excluding memories in which the farmer answered "I don't know". Feature-based match definitions are computed over the subset of floods for which harm questions were asked in both the earlier and later round.

a much lower base rate, predicts consistent recall much more strongly. These magnitudes are large: all else equal, a flood memory with property damage is between 50% and 100% more likely to be recalled again, depending on the match definition and sample.

Salience and Feature Encoding and Storage I next examine the related but distinct Prediction 2, which posits that among matched floods, more salient features will be more consistently recalled because they exhibit stronger encoding and storage. To conduct this analysis, I first use a greedy one-to-one assignment algorithm to match each flood—when possible—to a single corresponding flood in a subsequent round, based on the matching definitions described above. A pair meeting both the year (± 1) and strong feature criteria jointly receives the highest score of 10. A pair satisfying only the strong feature match (without meeting any year-based or weak feature criterion) receives a score of 4, as does a pair that matches on the exact year alone (without meeting any looser year criterion or any feature criterion). A pair matching on year ± 1 only (without exact year or year ± 2) receives a score of 3, and a pair matching on ± 2 only (without any tighter year match) receives a score of 2. A pair satisfying only the weak feature match (without the strong feature match) receives the lowest score of 1. These scores are mutually exclusive by construction: each pair receives credit for only its single strongest match criterion. I drop pairs that satisfy no match criterion. Among pairs with the same score, I break ties first by smaller absolute year difference and then by smaller absolute difference in flood duration.

Given this set of best matches, I assess which features show the highest consistency within pairs. Because features exhibit varying base rates, common characteristics (such as an indicator for crop damage) may be consistently recalled just by chance. To adjust for this, I report Cohen's κ statistics, defined in Equation (7), where I calculate chance rates by randomly constructing pairs across 500 permutations.

$$\kappa = \frac{\text{Observed Agreement} - \text{Chance Agreement}}{1 - \text{Chance Agreement}} \tag{7}$$

Table 6 presents the results. I divide the table into two sections: *features*, which shows the dimensions of features in common within the pair, and *feature values*, which shows which specific values of those features disproportionately appear in successful matches. As before, salient features stand out: crop damage share and house damage agree across matched pairs far more often than chance would predict. Among specific values, top-quartile house damage and top-quartile crop damage are about twice as likely as bottom-quartile values to appear in best matched memories, suggesting extreme damage is encoded and stored more reliably. Older floods also appear disproportionately more often than recent ones, consistent with selection from extreme and famous inundation events such as the 1988 and 1998 floods in

Bangladesh—the latter of which covered nearly 70 percent of the country and killed more than 1,000 people, making it one of the deadliest floods in the country's history.

Context Similarity and Memory Consistency I next test Prediction 3 by asking whether more similar contemporaneous contexts yield more consistent recall. I use natural variation in whether rain happens to fall on the day of the interview as an exogenous shock to context. The model predicts that farmers recounting floods on two days with the same rainfall status will report more similar memories than those recounting floods on days with different rainfall status. I estimate Equation (8) at the flood memory level, where $Match_{i,j}$ denotes whether flood j recalled by respondent i has a match in a later round under a given match definition, $SameRain_{i,j}$ captures whether those two rounds have the same rainfall status, and $\phi_{v(i)}$ and $\psi_{t(j)}$ capture village and survey wave fixed effects, respectively. I cluster standard errors at the respondent level and report robustness to clustering at the village level. I measure rainfall using enumerator reports to avoid the poor data quality of remote-sensing precipitation products in Bangladesh, described further in the monsoon section below.

$$Match_{i,j} = \beta SameRain_{i,j} + \phi_{v(i)} + \psi_{t(j)} + \varepsilon_{i,j} \tag{8}$$

Consistent with the model, Table 7 reports results of Equation (8) showing that context similarity drives recall similarity. Panel A shows that matching rainfall status increases the likelihood of a successful memory match by 10 to 20 percent depending on the match definition, with statistically significant treatment effects in half of cases. Panels B and C re-estimate Equation (8) excluding two-dry-day pairs (Panel B) and two-rainy-day pairs (Panel C) from the same-rainfall category. These results show that recalling memories on two rainy days produces especially consistent memory, as expected given that rainy days are rarer and therefore more salient, serving as a stronger contextual cue. Appendix Table D.16 confirms these results are robust to clustering standard errors at the village level.

Feature Reconstruction Natural Experiment To test Prediction 4, I use this same natural variation in daily weather as an exogenous shock to the contemporaneous environment in which the decision-maker accesses memory, shedding light on how context alters memory content. I estimate Equation (9), where $Y^f_{i,j}$ denotes the value of feature f recalled by respondent i for memory j, $Raining_{i,d(j)}$ indicates whether it rained on the day $d(j)$ when farmer i recalled memory j, and λ_i and $\phi_{t(j)}$ denote farmer and survey wave fixed effects, respectively. This design holds constant anything affecting an individual farmer's memories systematically, leveraging only variation in the weather on one day the farmer reports memories versus another. For example, this accounts for any concerns that specific types of farmers may be more or less willing to be surveyed on rainy days. I cluster standard errors at the village level.

$$Y_{i,j}^f = \beta Raining_{i,d(j)} + \lambda_i + \phi_t(j) + \varepsilon_{i,j} \tag{9}$$

Table 2 reports results from Equation (9), showing that contemporaneous rain on the day of memory elicitation causes a substantial shift in the content of recalled flooding events. Panel A shows that rain makes recalled floods last longer, more likely to damage crops, and more damaging to harvest. There are no significant impacts on property damage, consistent with rain having the potential to damage harvest even independent of flooding—making crop harm a more similar feature. The total number of floods recalled falls while the average reported year rises, consistent with interference: floods more similar to the context came to mind, crowding out other experiences from recall. This raises the possibility that contextual similarity impacts memory primarily through selection of which memories surface rather than reconstruction of omitted features. Panel B re-estimates Equation (9) including the "bad" control of flood year: results remain very similar, suggesting reconstruction does play a substantial role. These results are robust across alternative specifications, including different fixed effects—village and round, village and survey date, and round and farmer—and standard errors clustered at either the village or farmer level.[9] Unlike the information experiment that follows, this natural experiment does not allow me to disentangle the causal effect of context on beliefs and behavior through memory from a direct effect (moderated by γ in the model). Appendix Table A.16 estimates Equation (9) using beliefs about future flooding and demand for flood insurance, generally finding negative effects, though these results are not robust across specifications.[10]

Feature Reconstruction Survey Experiment To provide experimental evidence on memory reconstruction and Prediction 4, I randomize whether farmers recall floods from oldest to most recent or in the reverse order. The core idea is that the first flood memory farmers recount serves as a context in and of itself, potentially shaping subsequent memories.

[9]See Appendix Table D.3 for robustness using the outcome of the number of recalled floods; Appendix Table D.4 for flood year; Appendix Table D.5 for flood duration; Appendix Table D.6 for any crop damage; Appendix Table D.7 for share of harvest damaged; Appendix Table D.8 for any property damage; and Appendix Table D.9 for the amount of property damage.

[10]In survey waves one and two, the insurance demand elicitation was hypothetical rather than incentivized (and described to participants as such). In wave three, I offered actual insurance contracts and properly incentivized the mechanism, as described further below. To examine whether the hypothetical framing altered responses, I conducted an additional survey on a separate sample of 49 households from the same area who had not participated in the main survey, offering both the incentivized and hypothetical versions. Appendix Figure D.3 compares the two measures: they are strongly positively correlated.

Randomizing whether the oldest or most recent flood comes first provides exogenous variation in the nature of that initial context. As with the salinity experiment described below, I use neighbors' answers to predict the context for a specific feature, calculating the leave-out-mean characteristic of the first flood remembered by other farmers in the village asked to recount floods in that same order.

Formally, I estimate the reduced-form specification in Equation (10). Let $Y_{i,j,f,t}$ denote the value of feature f for the j^{th} memory (with $j > 1$) recalled by farmer i in survey round t, and let $\widetilde{Y}_{v(i),f,t}$ denote the leave-out-mean for feature f for all farmers other than i in village $v(i)$ interviewed in survey round t who answered questions in the same order. I include farmer fixed effects λ_i and flood-year fixed effects $\phi_{y(j)}$, and I cluster standard errors at the farmer level.

$$Y_{i,j,f,t} = \beta\widetilde{Y}_{v(i),f,t} + \lambda_i + \phi_{y(j)} + \varepsilon_{i,j,f,t} \quad \forall j > 1 \tag{10}$$

The intuition behind this design is that after including a farmer fixed effect and year of flood fixed effect, the only way in which the content of the first memory should impact the features of subsequently recalled events is by serving as a context. To address concerns that even compared to another of their memory elicitations, a farmer in any given survey wave might be primed for other reasons to discuss floods of a certain characteristic, I use their neighbors' responses instead of their own, generating variation based on the randomized order of recall. In an instrumental variables sense, the exclusion restriction assumption underpinning these results requires that conditional on a farmer and flood year fixed effect, the only way one's neighbors' answers to the first flood they remembered conditional on being asked to recount floods in the same order impacts a respondents' subsequently recalled inundation events is through the respondent's own recounting of their first flood memory.

Table 3 presents the main results. Panel A reports the first-stage: the leave-out-mean of responses from others in the same village asked to recall memories in the same randomized order predicts farmers' own responses across all five features. Panel B reports the main reduced-form result from Equation (10). Being in a village where others asked in the same order report a first flood one day longer increases the length of subsequent floods the farmer reports by .57 days. The share of harvest damaged and the likelihood of home damage both increase by more than 25 percentage points, shifting subsequent flood features substantially—again evidence of reconstruction. The only feature without treatment effects in these reduced form results is the indicator for crop damage, which exhibits a high base rate and therefore low marginal predictive power. Appendix Table D.10 confirms robustness to clustering standard errors at the village level. Panel C reports the instrumental-variable versions of these coefficients, scaling the reduced form results in Panel B by the first stage in Panel A. Appendix Table A.10 shows no impact of oldest-first order on the total number of memories, suggesting these effects operate primarily through reconstruction rather than differential recall of experiences.

A corollary of Prediction 4 is across-feature spillovers via similarity: for instance, if longer floods are more similar to more agriculturally destructive floods, then a first memory of a longer flood will increase not only the length of subsequent floods recalled but also the amount of crop damage. I calculate each set of spillovers in Appendix Tables A.11 through A.15, with clustered standard errors at the village level in Appendix Tables D.11 through D.15. Figure 3 combines the main coefficients to systematically examine these patterns, plotting the relationship between cross-feature spillover coefficients and a proxy for similarity: the underlying correlation between flood features among first flood memories. Each point represents a pair of flood features—one whose leave-out-mean is used as the regressor and the other as the spillover outcome. The horizontal axis shows the pairwise correlation between the main and spillover features in the first-mentioned flood sample. The vertical axis shows the standardized reduced-form coefficient from regressing the spillover outcome feature on the main feature's leave-out-mean among later-mentioned floods (Panel B of the spillover tables), standardized by the ratio of the regressor's standard deviation the outcome's. Although the number of total observations is low, the results show a positive and marginally statistically significant relationship, suggesting that the cross-feature spillover effects are stronger among features closer in similarity space.

Flood Information Experiment To experimentally identify the role of memory in economic behavior and test Prediction 7, I run a tailored information experiment linking context, memory, and demand for flood insurance. The core intuition is that comparing the differential treatment effects of information provided *before* memory elicitation to those of the same information provided *after* allows me to separate the memory-induced impact of context on beliefs and behavior from impacts through other channels (governed by γ in the model).

The experiment involves five arms; Figure 4 presents this design visually. All respondents report their flood memories following the same elicitation procedure described above. A random third of respondents are assigned to a pure control arm. In the remaining four arms, I randomly provide information. Enumerators delivered two pieces of truthful information: "flood" information and "damage" information. For the flood information treatment, enumerators read: "Major floods struck some parts of Bangladesh in June, August, and October of this year." For the damage information treatment, they read both the flood information and a second sentence: "The government estimates that 1.1 million metric tons of rice have been destroyed by floods in Bangladesh this year." To aid interpretation, I contacted a small number of control-group respondents after the experiment to measure their prior knowledge about the information disseminated in the treatment arms. The flood information was very commonly known, while essentially no farmers knew of the government's crop loss estimates from the damage arm. I randomize whether I administer only the flood information or also provide the damage information, cross-randomized with whether I provide that information before the memory elicitation or after the elicitation (but still well before the flood insurance WTP elicitation). Because information provided after the explicit elicitation may still affect implicit recall when respondents answer later questions, I interpret these effects as a lower bound on the causal impact of memory.

I focus on demand for flood insurance as the main climate adaptation outcome of interest. As detailed in Appendix Section B.2, I first explain flood insurance and then elicit willingness-to-pay for a contract that pays 10,000 BDT in the event of a flood. I measure demand for insurance using an ascending price list version of the Becker-DeGroot-Marschak (BDM) mechanism. Starting at a low price, I ask farmers if they would be willing to purchase the contract at that price.[11] If they say yes, I increase the price and ask again, repeating until they say no. I then randomly select a price, and if the respondent was willing to purchase at that price, the transaction takes place.[12]

I also measure impacts on beliefs about future floods as a test of Prediction 6. To capture quantitative expectations in this low-numeracy population, I develop a visual elicitation method in which respondents place beans across pictures of mutually exclusive scenarios describing the number of days of flooding they might experience. I elicit this for both one year ahead and over the next five years. I also elicit expected crop damage by flood length to compute expected agricultural loss from floods. Appendix Section B.2 provides further details.

I estimate Equation (11) to identify how contemporaneous context impacts beliefs and economic behavior through memory. The variable $FloodBeforeRecall_i \in \{0, 1\}$ denotes whether enumerators read the flood information to respondent i prior to their memory elicitation (equal to one for all respondents who received any information prior to the memory elicitation), $DamageBeforeRecall_i \in \{0, 1\}$ indicates the incremental affect of whether the damage information was read aloud, and $FloodAfterRecall_i \in \{0, 1\}$ and $DamageAfterRecall_i \in \{0, 1\}$ signify the corresponding information read after the memory elicitation.

[11] During piloting, norms against accepting gifts for free distorted the BDM when beginning at 0 BDT. To circumvent this issue, I begin the price list at a small positive value of 10 BDT, and only ask about 0 BDT if the respondent declines the initial price.

[12] I pre-registered winsorizing willingness-to-pay at 200 BDT.

$$Y_i = \alpha + \beta_1 FloodBeforeRecall_i + \beta_2 DamageBeforeRecall_i + \\ \beta_3 FloodAfterRecall_i + \beta_4 DamageAfterRecall_i + \varepsilon_i \quad (11)$$

Table 8 presents the main impacts of the information experiment on demand for flood insurance. I pre-registered restricting the sample to places where a machine learning algorithm's predictions based on geographic features suggest high flood risk.[13] My preferred sample is therefore shown in column (4), restricting to respondents in the top two quintiles of flood risk, though I report results among alternate samples for robustness. Among respondents shown information *after* after they recalled their memories, I find small and statistically insignificant impacts on willingness-to-pay. Information provided *before* the memory elicitation tells a different story. Informing farmers about floods elsewhere in the country lowers flood insurance demand 18.4% (though not statistically significant), while the impact of additionally providing the damage information as well more than compensates. Importantly, note that this captures the incremental impact of listening to the damage sentence. The net impact of the combined damage and flooding information (the sum of β_1 and β_2), while positive, is small and not statistically significant. One interpretation of this set of results is that flood information alone serves as a reminder of how frequently floods occur, and given the lack of baseline flood insurance, farmers may be reminded that they have consistently coped with past floods without insurance—thereby lowering demand. Adding the damage information, however, shifts this context: farmers think specifically about the destruction caused by past floods, raising willingness-to-pay for insurance.

Consistent with the information provided before memory elicitation impacting insurance demand but the same information provided after having no effect, Table 9 provides evidence that the contextual effect operates through memory. I estimate Equation (11) using the average feature value across recalled floods as the outcome, coding those who do not report a memory as having a feature value of 0. As expected, information provided after recall has no impact on the content of memories. Damage information provided before recall has an incremental effect of increasing the likelihood that respondents remember a flood that damaged their homes by 22.1%, that damaged their crops by 10.2% (though not statistically significant), the amount of crop damage per memory by 14.8%, and the duration of flooding by nearly two days. Combining these into an index following Kling et al. (2007), I find a marginally statistically significant 0.22 standard deviation increase in the severity of flood memories among those provided with the damage information prior to memory elicitation.

[13] See Patel (2026b) for further details on the construction of this flood risk measure.

Table 10 provides evidence on the next step in the causal chain, showing whether this shift in memories appears to translate into respondents' beliefs. Across measures of expectations for both future flood frequency and flood damage, I find reasonably large and positive effects from the damage arm prior to recall, though the estimates are not statistically significant at standard levels.[14] Although this could simply reflect a lack of statistical power, another interpretation is that memories shape decision-making through a separate channel than the beliefs I measure. Instead of (only) impacting the perceived likelihood of flooding and expected flood damages, memories may shape preferences or beliefs about other non-flood risk parameters that nevertheless impact demand for flood insurance, such as the channel described in Bordalo et al. (2025).

3.2 Soil Salinity

I next turn to the domain of soil salinity to test Predictions 6 and 7 by isolating the causal link from farmer's memories to their beliefs and subsequently to adaptation behavior.

Soil salinity provides an ideal setting for this question for three main reasons. First, the saltiness of one's soil plays a critical role in dry-season rice production (in which all of the farmers in my sample engage). This challenge extends beyond Bangladesh, with soil salinity threatening approximately 30 percent of irrigated land worldwide (Hopmans et al., 2021). Scientists forecast that global warming will exacerbate this problem through several channels including rising sea levels, increased evaporation, droughts, and floods (Mukhopadhyay et al., 2021), significantly reducing agricultural productivity (Clarke et al., 2015; Dasgupta et al., 2015, 2018). Second, farmers lack access to formal measurements of their soil, forcing them to largely rely on their own experiences to form their beliefs (Patel, 2026a). Third, farmers face a high-stakes adaptation decision that hinges on these expectations: the choice of whether or not to plant a salinity-tolerant rice variety. Farmers' beliefs about the salt content of their soil play a key role in this seed decision because the salinity-tolerant varieties do not Pareto dominate other choices: at low salt levels, farmers may be better off planting another variety which tastes better, protects against pests, or earns a higher market price, for instance, while at high salt levels, the salinity tolerant seeds still produce high yield while other varieties do not.[15]

[14] Appendix Tables D.17 and D.18 present these results for memories and beliefs, respectively, expanding to all respondents, irrespective of flood risk (contrary to the pre-registration).

[15] In Patel (2026a), I use two randomized controlled trials to show that beliefs play a causal role in this technology adoption and that seed choice has a large impact on farmers' agricultural profits.

Salinity Memory, Belief, and Behavior Data Across all three waves of the survey, I elicit farmers' memories about whether "the amount of salt in the soil in the typical plot" in their village increases, decreased, or stayed the same over the last decade, and their forecasts about the same over the coming 10 years. Appendix Table A.2 presents the distribution of answers to these salinity trend memories and forecasts, documenting substantial variation: for instance, about 39.9% of farmers think salinity has increased in the past, while 42.0% think salt levels have decreased. Because the modal farmer reported decreasing past salinity, I focus on whether the respondent recalls decreasing salinity as the main dimension of heterogeneity in the regressions that follow. This disagreement reflects across village, within village, and even within farmer over time variation, as shown in Appendix Table A.3. During the first two waves, I also elicit farmers' willingness-to-pay for a salinity tolerant seed variety using the same BDM mechanism as described above. All else equal, I expect farmers who forecast higher salinity in the future to pay more for the packet of BRRI 67 seeds, a variety which has been explicitly designed to grow well in high salinity soil. This demand measurement took place an average of 39.44 minutes after the salinity memory and belief questions in the first survey wave and 5.29 minutes later in the second round.

Empirical Strategy To identify the causal impact of memories on beliefs and behavior, I randomize which question I ask first at the farmer-by-survey wave level: either farmer's memories of past salinity or their prediction about future salt levels. I then ask whether those randomly assigned to answer about memory before providing their expectations report different beliefs. For behavior, the experiment tests whether beliefs formed after a past-memory prompt translate into differential product valuation in comparison to beliefs formed before, even when all respondents explicitly report memories before demand elicitation. Note that the model describes an implicit memory recall at the belief formation stage, and thus even those respondents asked about future salinity changes first likely draw from their own memory database about past salinity when providing their forecast. However, by explicitly asking about past changes, I more directly bring memories about soil salinity top of mind for the respondent. When considering impacts on climate adaptation behavior, that elicitation takes place after elicited memories even for the group asked to recall second, and therefore I only capture the marginal effect of recall on beliefs when asked first. These results likely present a lower bound estimate of the potential scope for memory in expectation formation and behavior.

The act of asking about past salinity first may impact beliefs about the future and subsequent climate adaptation behavior in ways other than the memory mechanism at the heart of the model, for instance, by making respondents think longer about salinity. To account for any of these other channels, I implement a difference-in-differences design combining this question order randomization with climatic variation in salinity across places and over time. I ask whether the specific *content* of memories impacts expectations and willingness-to-pay, conditional on the act of first reflecting on the past itself. To overcome the fundamental challenge that I do not observe the content of counterfactual memories that would have been recalled first among those asked about their forecasts first, I use predicted salinity memories as a proxy for the content of each respondent's recall. I estimate Equation (12), where $PastFirst_{i,t}$ indicates whether respondent i answered about past salinity first in survey wave t, $\widehat{PastDecrease}_{i,t}$ captures the predicted likelihood of respondent i recalling past salinity in period t, λ_i denotes a farmer fixed effect, and $\psi_{v(i),t}$ denotes a village-by-survey wave fixed effect.[16] These fixed effects account for any time-invariant characteristics of a farmer as well as any local shocks in that village and year. I also report specifications without fixed effects and with separate village and survey wave fixed effects.

$$Y_{i,t} = \alpha + \beta_1 PastFirst_{i,t} + \beta_2 \widehat{PastDecrease}_{i,t} + \beta_3 PastFirst_{i,t} \times \widehat{PastDecrease}_{i,t} + \lambda_i + \psi_{v(i),t} + \varepsilon_{i,t} \tag{12}$$

This regression tests for the causal role of memory by examining whether farmers predicted to recall decreasing salinity alter their beliefs and climate adaptation behavior when asked to explicitly recall these memories before their predictions, compared to those with different predicted memories and those with the same predicted memories asked after their beliefs.

I consider three ways to predict salinity memories $\widehat{PastDecrease}_{i,t}$ and find similar results with all three. First, I calculate the village-by-survey wave leave-out-mean using the responses of all other farmers in that same village during that round. Second, I convert this into a binary variable indicating whether the majority of other farmers report decreasing salinity in the past. Finally, I use a simple random forest algorithm to predict whether or not a respondent remembers declining salinity, and calculate the gap in predicted likelihood of a decrease vs. not decrease to use as $\widehat{PastDecrease}_{i,t}$. I train this algorithm on a random 30 percent sub-sample of respondents. The algorithm achieves an area under the ROC curve of .74 in the remaining 70 percent, as shown in Appendix Figure A.1.[17]

[16] Following Patel (2026a), I exclude respondents who fail basic survey comprehension checks. Appendix Table D.2 presents the main analysis including all respondents.

[17] I include as predictors the village leave-out-mean, soil salinity on the farmer's plot as measured during wave 1, household size, gender, age, an indicator for anyone over the age of 60 in the house, an indicator for a male over the age of 60, years of schooling of the respondent as well as indicators for different levels of schooling, an indicator for past migration, log household earnings, the number of plots, and the years of experience cultivating Boro season rice.

Results This experiment shows consistent evidence that memory impacts beliefs and climate adaptation behavior. Figure 2 presents the simplest way to see these effects by plotting the raw means by treatment status and the binarized prediction of respondents' memories. The bar charts in the first row show that farmers predicted to remember declines in salt levels are substantially more likely to predict future decreases when asked about their memories first. This change in beliefs translates to changes in incentivized demand for technology to combat salinity, shown by the substantial decreases in willingness-to-pay among those farmers in the second row. Appendix Figure D.2 demonstrates similar results for the continuous predictors of leave-out-mean and random forest prediction showing graphs with linear functions between the outcome (forecasts and seed demand) and the predictor, separately by randomized question order.

Table 1 presents these results in regression form by estimating Equation (12) and reporting the main interaction coefficient of interest β_3.[18] Columns vary based on the predictor used and the set of fixed effects included in the specification. The results remain broadly consistent across different predictions of recalled salinity trends, though the leave-out-mean systematically yields the largest magnitudes. Adding increasingly demanding fixed effects has little impact on these treatment effects, though standard errors become slightly larger with both household and village-by-wave fixed effects that render the forecast treatment effects (but not the seed demand estimates) no longer statistically significant at traditional levels. The experiment shows large impacts of memory in this setting: beliefs about declining future salinity trends increase between 10.1% and 27.9% depending on the specification; seed demand falls by 6.8% to 29.9%. Appendix Figure D.1 visualizes the coefficient of interest of Equation (12) through a margins plot. Appendix Table D.1 shows these results remain robust to clustering standard errors at the village level. Together, these results indicate a substantial role for memories to shape climate adaptation behavior through their impact on beliefs in this setting.

Placebo Checks As reassurance for the memory interpretation of the results in Table 1, I next examine two placebo tests where a memory explanation would predict null results. First, I re-estimate Equation (12) using farmers' willingness-to-pay for a plastic plate instead of a salinity tolerant seed. Appendix Table A.6 shows consistently small and insignificant impacts on demand for this plate, as expected. Second, I consider the reverse experiment: does being asked about the future first change what people remember about the past? Under constructive memory, the answer to this question could be yes if farmers do not encode and

[18] Appendix Table A.4 and Table A.5 present the full set of difference-in-difference coefficients from Equation (12) for forecasts and seed willingness-to-pay, respectively.

store information about past trends; any features stored in the mind should attenuate this effect. The model therefore predicts this reverse experiment should have smaller impacts. Appendix Table A.7 shows the results of this reverse experiment in which I estimate Equation (12), replacing each instance of the past with the future.[19] Consistent with the theory, I find small and insignificant impacts of answering about future predictions first on memory.

3.3 Monsoon Intensity

Finally, I turn to monsoon intensity to shed light on Prediction 5: contemporaneous context will play a stronger reconstructive role when a memory's features have weaker encoding and storage. I use older monsoon seasons to proxy for amenability to reconstruction and examine whether rain during the farmer's interview alters older memories more than more recent ones.

Rainfall presents an ideal setting for testing this prediction for four reasons. First, monsoon intensity constitutes an important input into farmers' decisions about the type of and timing of agriculture, not to mention meaningfully relates to flood risk. Second, examining monsoon intensity farther ago vs. more recently provides natural variation in the strength of encoding and storage while maintaining a high-degree of homogeneity across other aspects of the memories. Third, contemporaneous rainfall at the time of the survey provides naturalistic variation in the decision-maker's context. Fourth, farmers necessarily observe rainfall when it occurs, allowing me to abstract from uncertainty about whether people actually experienced an underlying event when analyzing memories.

Monsoon Intensity Data I directly elicit farmers memories about past monsoon seasons using a visual elicitation method to capture percentages in this low-numeracy population. I ask farmers to place 10 buttons across mutually exclusive pictures that capture the number of days of rain across a typical two week period during the monsoon (see Appendix Section B.2 for details on this measurement). Barring survey attrition, I capture rainfall memories twice for each farmer: once chosen at random during either the first or second wave, and among all farmers in the third wave. Each time I measure memories, I elicit recollections of the most recent season (the same calendar year for waves one and three and the previous calendar year for wave two) and for either five or 10 years ago, chosen at random. I randomize the order in which I elicit memories.

To capture the contemporaneous context, I use enumerator reports to the question, "was it raining at any time during the day in this area?" asked at the end of each survey. These data

[19] I also re-calculate each of the predictions accordingly; Appendix Figure A.2 presents the random forest performance using forecasts as the outcome.

provide reliable rainfall measurement in rural Bangladesh, where sparse coverage of rainfall gauges yields poor data on precipitation and substantial disagreement across different remote sensing products.

Empirical Strategy To test Prediction 5, I adopt a difference-in-differences design, leveraging quasi-random variation in the weather in combination with random variation in recall period embedded in the survey. I ask whether contemporaneous rain differentially impacts memory reconstruction for more temporally distant monsoons (with therefore weaker feature storage). I include farmer fixed effects and village-by-round fixed effects, thereby holding constant any particular farmer's tendency to differentially answer about longer ago monsoon seasons and the characteristics of any particular village in a given year, capturing only day-to-day variation in the weather. I estimate Equation (13), where $Y_{i,j,t}$ denotes the monsoon memory of farmer i for recall period j in year t, $Raining_{i,t}$ denotes whether the enumerator interviewing i in year t reported rain in that area as of i's survey, $RecallPeriod_j$ equals the number of years since the monsoon season in question, λ_i denotes a farmer fixed effect, $\psi_{v(i),t}$ denotes a village-by-round fixed effect, and $\phi_{o(i,j,t)}$ denotes a fixed effect for the order in which farmer i recounted rainfall memory j in year t among the set of monsoon memories. I cluster standard errors at the village level.

$$Y_{i,j,t} = \beta_1 Raining_{i,t} + \beta_2 RecallPeriod_j + \beta_3 Raining_{i,t} \times RecallPeriod_j + \lambda_i + \psi_{v(i),t} + \phi_{o(i,j,t)} + \varepsilon_{i,j,t} \tag{13}$$

Results Consistent with the model's prediction, I find evidence that contemporaneous rainfall rewrites older memories more than more recent ones. Table 11 presents the main results. Column (4) presents the most conservative specification in Equation (13), while Columns(1), (2), and (3) show results remain robust to less stringent fixed effects. Rain on the survey date increases recalled monsoon intensity by 19.9 percent for 10-year older memory. In other words, contemporaneous context has a smaller impact on memories with better-stored features. Appendix Figure A.3 visualizes this interaction effect as a margins plot. Interestingly, for the most recent monsoon seasons, rainfall on the interview date has a negative impact on recalled precipitation (a phenomenon also reflected in the negative β_1 coefficients in Table 11). One interpretation of this pattern is a contrast effect. To investigate this hypothesis, I ask farmers about their beliefs about the changes in rainfall over time: 96 percent of farmers believe low rain years have become more common (see Appendix Table

A.17 for the full distribution of responses).[20] Under that mental model, contemporaneous rainfall during the survey may serve as a reference point to sharpen the perceived gap, even among well-preserved recent memories. Consistent with that interpretation, I find negative impacts of contemporaneous rain on future expectations about monsoon intensity. Unlike in the case of soil salinity, however, I do not find evidence with my pre-registered hypothesis that future predictions about monsoon intensity change based on respondents first recalling the past, perhaps because this contrast effect dominates. Appendix Table A.18 presents these results.

4 Discussion

This paper provides evidence that memories play a fundamental role in the belief formation process and economic decision-making of farmers on the front lines of climate change. Contemporaneous context shape the content of memories, highlighting their instability. What farmers remembers plays a causal role in their expectations about the future environmental conditions they will face and has a corresponding impact on their demand for climate adaptation technology.

Although beyond the scope of this paper, these results have suggestive implications for policy. In general, the model of memory I have presented here does not have signed predictions for overall climate adaptation without further assumptions. However, the memory patterns I have documented point to particular moments and places when policy interventions might be post effective. For example, in the immediate aftermath of a natural disaster, the context may have an outsized impact on beliefs about the likelihood of future catastrophe via reconstruction of past memories, consistent with the spike in flooding beliefs I observe in Patel (2026b). Overall, these results underscore that lowering information frictions to alleviate the cognitive demands for complex forecasting such as those facing farmers learning about their local environment could substantially increase welfare.

[20] Climate scientists have also documented shifting patterns in the South Asian monsoon (Mohan and Rajeevan, 2017).

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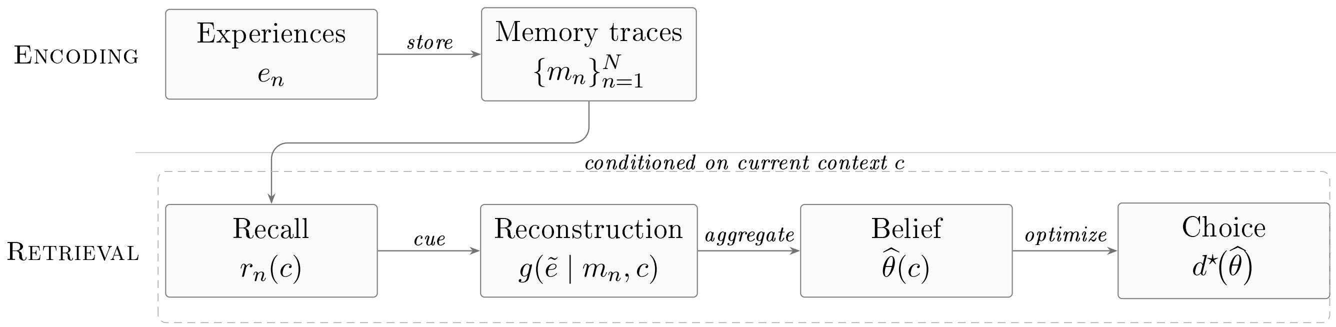
5 Figures and Tables

Figure 1: Timeline of Belief Formation



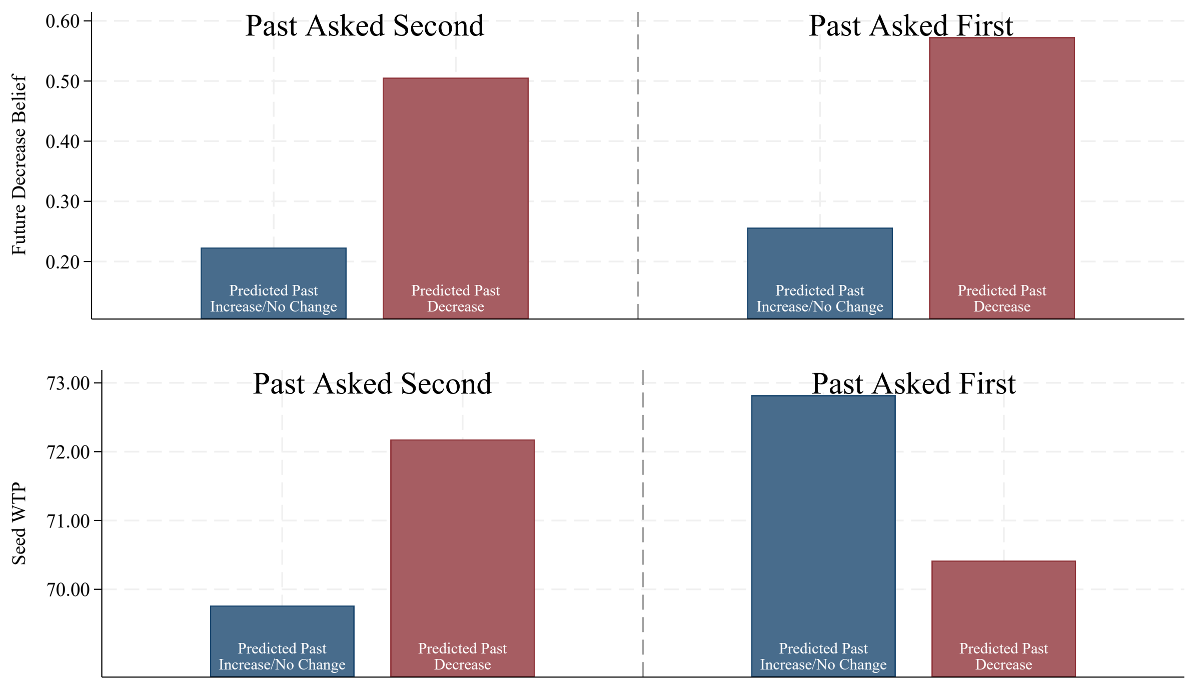
Note: Figure 1 presents a timeline of the model. The DM encodes and stores experiences as noisy memory traces (top row). At decision time, the DM recalls traces and reconstructs them with context-dependent probability and aggregates memories into a belief that determines their decision (bottom row).

Table 1: Salinity Question Order Experiment Results

	No FEs			Village + Round FEs			HHID + Village × Round FEs		
	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)
Panel A. Outcome: Future village salinity decrease									
Asked Past First × Past Decrease (Prediction)	0.067*	0.034	0.053**	0.088**	0.053**	0.066***	0.066	0.032	0.042
	(0.039)	(0.026)	(0.026)	(0.038)	(0.025)	(0.024)	(0.041)	(0.029)	(0.030)
Observations	6,141	6,141	6,141	6,141	6,141	6,141	6,079	6,079	6,079
Unique farmers	2,266	2,266	2,266	2,266	2,266	2,266	2,204	2,204	2,204
Control mean	0.315	0.315	0.315	0.315	0.315	0.315	0.316	0.316	0.316
Panel B. Outcome: Seed WTP									
Asked Past First × Past Decrease (Prediction)	-16.800**	-4.818	-11.996**	-17.222**	-7.782	-12.380**	-21.330**	-16.585**	-17.283**
	(8.127)	(4.871)	(5.114)	(7.979)	(4.764)	(4.952)	(10.095)	(6.560)	(7.017)
Observations	4,116	4,116	4,116	4,116	4,116	4,116	3,740	3,740	3,740
Unique farmers	2,246	2,246	2,246	2,246	2,246	2,246	1,870	1,870	1,870
Control mean	70.498	70.498	70.498	70.498	70.498	70.498	71.126	71.126	71.126

Note: Table 1 presents the main interaction term of a difference-in-differences regression of being asked about past salinity trends first, the prediction of the respondent answering that in the past, salinity declined, and the interaction of those two. The prediction of a past decrease in the first, fourth, and seventh columns corresponds to the share of the respondents' neighbors in their village in that same survey wave who answered decrease (the leave-out-mean). In the second, fifth, and eighth columns, the prediction is a binarized version of that variable equal to one if the majority of neighbors answered a decrease and zero otherwise. In the third, sixth, and ninth columns, the prediction comes from a random forest model trained on the leave-out-mean, soil salinity on a randomly selected plot of the respondent as measured during the baseline survey, household size, gender, age, indicators for having a household member over 60 and a male household member over 60, years of schooling, indicators for different levels of schooling, indicator for having migrated in the past, log household earnings, total number of plots, and years cultivating Boro season rice. The first three columns do not include any fixed effects; the next three include village and survey round fixed effects; the last three include household and village-by-round fixed effects. All standard errors are clustered at the household level, and the sample excludes those who failed survey comprehension checks. Panel A shows impacts on whether the respondent predicted that salinity will decrease in the future. Panel B shows impacts on the Becker-DeGroot-Marschak elicitation of willingness-to-pay for a salinity tolerant seed. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" The future question is, "Think about the next 10 years. Do you think the amount of salt in the soil in the typical plot in your village will increase, decrease, or stay the same from now until then?" Standard errors are reported in parentheses, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Figure 2: Salinity Question Order Experiment: Raw Means



Note: Figure 2 plots means of the raw data of respondents' beliefs about future village salinity decline and seed willingness-to-pay by predicted past salinity decline, separately by whether respondents first answered about about past salinity ("Past Asked First") or not ("Past Asked Second"). Predicted past decline is measured using whether more than half of the respondents' neighbors answered that salinity decreased in the past. The sample excludes those who failed comprehension checks.

Table 2: The Impact of Contemporaneous Rainfall on Recalled Floods

	# Floods	Flood Year	Length (Days)	Harm Crops	Share Crop Damage	Harm House	Value House Damage (BDT)
Panel A. Farmer and round fixed effects							
Rain on Survey Day	-0.175***	1.386**	3.250***	0.046***	0.063***	-0.022	624.978
	(0.067)	(0.701)	(0.645)	(0.015)	(0.022)	(0.029)	(989.718)
Control mean	0.999	2,006.593	17.526	0.911	0.756	0.448	11,749.649
Observations	6,487	5,927	5,948	4,706	4,691	4,721	4,619
Panel B. Farmer, round, and flood year fixed effects							
Rain on Survey Day			3.237***	0.045***	0.060***	-0.009	162.006
			(0.622)	(0.016)	(0.023)	(0.029)	(1,055.172)
Control mean			17.477	0.911	0.756	0.448	11,810.367
Observations			5,896	4,667	4,652	4,681	4,580

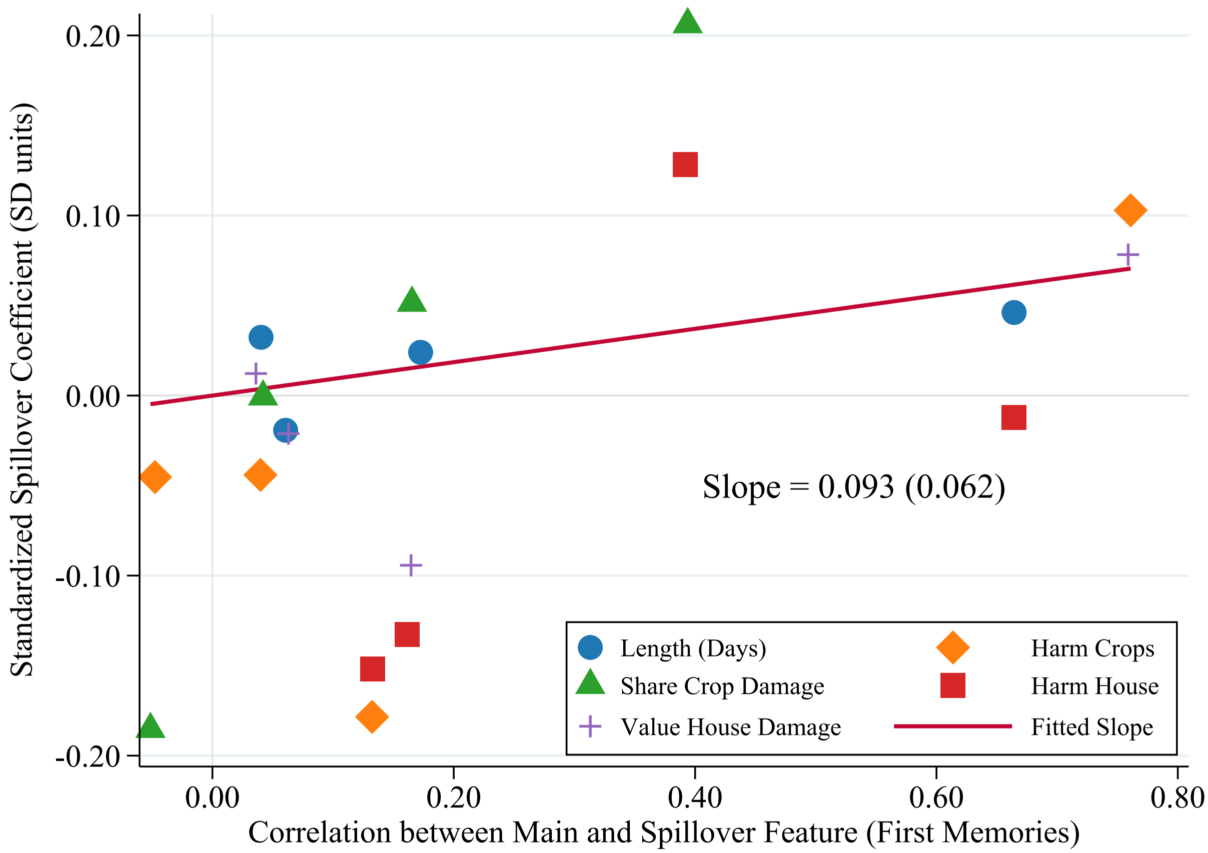
Note: Table 2 shows the impacts of rainfall on the day respondents are interviewed on the characteristics of the floods that the respondent remembers during the memory elicitation. Rainfall on the day of the survey is measured by the enumerator conducting the survey. Column (1) shows the impacts on the number of floods recalled. Column (2) shows impacts on the year of the flood recalled. Column (3) shows impacts on the length of the flood in days. Column (4) shows impacts on an indicator for whether the flood damaged crops. Column (5) shows impacts on the share of total crops that were damaged. Column (6) shows impacts on an indicator for whether the house was damaged. Column (7) shows impacts on the value of property damage. Panel A shows treatment effects with farmer and round fixed effects. Panel B shows treatment effects with farmer, round, and recalled flood year fixed effects. All regressions cluster standard errors at the village level, and p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table 3: Impact of First Recalled Memory Features on Subsequent Memories

	Length (Days)	Harm Crops	Share Crop Damage	Harm House	Value House Damage (BDT)
Panel A. First stage					
Leave-out mean	0.705***	0.156**	0.367***	0.341***	0.335***
	(0.031)	(0.065)	(0.047)	(0.044)	(0.050)
Mean of outcome	18.334	0.931	0.783	0.464	12,991.262
Observations	3,043	2,305	2,290	2,308	2,235
Unique farmers	1,257	1,024	1,017	1,026	992
Panel B. Reduced form					
Leave-out mean	0.573***	0.014	0.263***	0.259***	0.167**
	(0.055)	(0.128)	(0.084)	(0.066)	(0.067)
Mean of outcome	18.727	0.914	0.783	0.437	12,599.214
Observations	1,767	1,300	1,297	1,307	1,275
Unique farmers	591	469	468	472	465
Panel C. IV					
First-mentioned flood characteristic	0.721***	-0.061	0.445***	0.687***	0.482**
	(0.077)	(4.097)	(0.146)	(0.231)	(0.212)
First-stage F-statistic	221.75	0.09	45.09	18.98	17.14
Mean of outcome	18.731	0.914	0.783	0.437	12,685.347
Observations	1,762	1,297	1,292	1,306	1,249
Unique farmers	589	468	466	472	454

Note: Table 3 presents estimates of the effect of flood memory order on recalled flood features. The sample consists of flood memories elicited from farmers across three survey rounds, excluding respondents who failed survey comprehension checks. For each flood feature, the leave-out-mean is constructed as the average of that feature among other farmers in the same village and round who recalled their oldest flood first (if the respondent recalled oldest first) or their most recent flood first (if the respondent recalled most recent first), excluding the respondent's own value. Column (1) shows impacts on flood length in days. Column (2) shows impacts on an indicator for whether the flood harmed crops. Column (3) shows impacts on the share of crops damaged. Column (4) shows impacts on an indicator for whether the flood damaged the house. Column (5) shows impacts on the value of house damage in BDT. All regressions include farmer and recalled flood year fixed effects. Panel A reports first-stage estimates: the regression of the respondent's own first-mentioned flood feature on the leave-out-mean, estimated on the sample of first-mentioned floods. Panel B reports reduced-form estimates: the regression of later-mentioned flood features on the leave-out-mean, estimated on floods mentioned second or later. Panel C reports instrumental variable estimates, where the first-mentioned flood feature is instrumented by the leave-out-mean, estimated on the sample of later-mentioned floods. The first-stage F-statistic from the corresponding first-stage regression is reported in Panel C. Standard errors are clustered at the household level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Figure 3: Similarity and Cross-Feature Reconstruction Impacts



Note: Figure 3 plots the relationship between cross-feature spillover coefficients from the flood memory experiment and the underlying correlation between flood features. Each point represents a pair of flood features, where one is the main feature whose leave-out-mean is used as the regressor and the other is the spillover outcome feature. The horizontal axis shows the pairwise correlation between the main and spillover feature in the first-mentioned flood sample. The vertical axis shows the standardized reduced-form coefficient from regressing the outcome spillover feature on the main feature's leave-out-mean among later-mentioned floods (Panel B of the spillover tables), standardized by the ratio of the standard deviation of the regressor to the standard deviation of the outcome. Point shapes and colors indicate the main feature used to construct the leave-out-mean. All regressions include farmer and recalled flood year fixed effects, cluster standard errors at the household level, and exclude respondents who failed survey comprehension checks. The fitted line is estimated by OLS with the slope and standard error shown on the graph, where p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table 4: Flood Memory Feature Consistency

Match definition	Share matched	N
Panel A. All floods		
Exact year	0.218	4,099
Year ± 1	0.343	4,099
Year ± 2	0.401	4,099
Weak Feature Match	0.209	2,984
Strong Feature Match	0.042	2,983
Year ± 1 and Strong Feature Match	0.020	2,978
Panel B. Both rounds report at least one flood		
Exact year	0.243	3,514
Year ± 1	0.374	3,514
Year ± 2	0.436	3,514
Weak Feature Match	0.243	2,563
Strong Feature Match	0.048	2,562
Year ± 1 and Strong Feature Match	0.023	2,557
Panel C. Both rounds report the same number of floods		
Exact year	0.316	1,328
Year ± 1	0.426	1,328
Year ± 2	0.467	1,328
Weak Feature Match	0.272	1,050
Strong Feature Match	0.071	1,049
Year ± 1 and Strong Feature Match	0.041	1,046

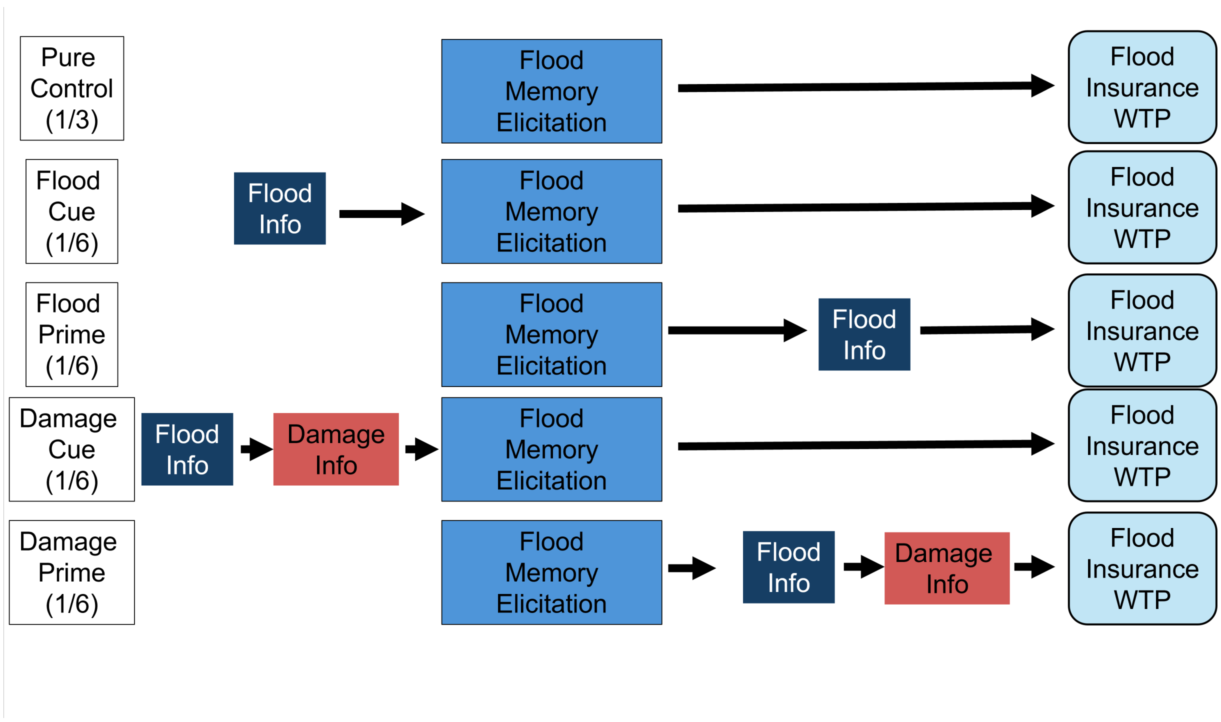
Note: Table 4 reports the share of recalled floods that can be matched to a flood recalled by the same respondent in a later survey round, under six match definitions of varying strictness. A flood recalled in an earlier round is considered matched if any flood recalled by the same person in a later round satisfies the given match criterion. Exact year requires the reported flood year to be identical across rounds. Year ± 1 and Year ± 2 allow the reported flood year to differ by up to one or two years, respectively. Weak Feature Match requires agreement on whether the flood harmed crops and whether it harmed the house, approximate agreement on the share of crops damaged (within 25%), and approximate agreement on flood duration (within 3 days). Strong Feature Match requires exact agreement on whether crops and the house were harmed, exact agreement on the share of crops damaged and flood duration, and agreement on the value of house damage within 25% of the larger reported value. Year ±1 and Strong Feature Match requires both the year-based and strong feature criteria to be jointly satisfied. Year-based match definitions are computed over floods with a non-missing reported flood year. Feature-based match definitions are computed over the subset of floods for which harm questions were asked in both the earlier and later round. The sample is restricted to floods whose reported year predates the survey year of the earlier round. Panel A includes all floods in this universe. Panel B restricts to floods recalled by respondents who reported at least one matching-eligible flood in both the earlier and later round, ruling out cases where a respondent reported floods in one round but not the other. Panel C further restricts to floods recalled by respondents who reported the same number of matching-eligible floods in both rounds.

Table 5: Predictors of Consistent Recall

	Year ± 2	Year ± 1	Exact Year	Weak Feature Match
Panel A. All floods				
Duration (days)	0.010***	0.008***	0.004**	–
	(0.003)	(0.003)	(0.002)	
Harmed Crops	0.069	0.130	0.066	–
	(0.088)	(0.085)	(0.069)	
Share Crop Damage	-0.019	-0.027	0.034	–
	(0.083)	(0.076)	(0.062)	
Harmed Property	0.162***	0.192***	0.127***	–
	(0.046)	(0.043)	(0.035)	
Property Damage (millions BDT)	0.095	0.064	0.221	1.515***
	(0.278)	(0.232)	(0.214)	(0.378)
Control mean	0.338	0.269	0.137	0.358
Observations	1,024	1,024	1,024	1,036
Panel B. Both rounds report at least one flood				
Duration (days)	0.011***	0.009***	0.005*	–
	(0.003)	(0.003)	(0.002)	
Harmed Crops	0.067	0.133	0.061	–
	(0.099)	(0.095)	(0.077)	
Share Crop Damage	-0.014	-0.024	0.050	–
	(0.100)	(0.092)	(0.075)	
Harmed Property	0.181***	0.217***	0.143***	–
	(0.052)	(0.048)	(0.040)	
Property Damage (millions BDT)	0.109	0.072	0.243	1.645***
	(0.298)	(0.251)	(0.233)	(0.430)
Control mean	0.386	0.307	0.156	0.409
Observations	896	896	896	906
Panel C. Both rounds report the same number of floods				
Duration (days)	0.011	0.008	0.002	–
	(0.009)	(0.009)	(0.006)	
Harmed Crops	0.041	0.032	0.195	–
	(0.319)	(0.320)	(0.259)	
Share Crop Damage	0.213	0.326	0.261	–
	(0.249)	(0.252)	(0.206)	
Harmed Property	0.023	0.221*	0.245*	–
	(0.116)	(0.131)	(0.124)	
Property Damage (millions BDT)	0.451	0.250	0.003	1.437***
	(0.417)	(0.415)	(0.306)	(0.462)
Control mean	0.613	0.508	0.287	0.568
Observations	181	181	181	183

Note: Table 5 presents estimates of Equation (6), regressing a match indicator on flood severity measures. The sample is restricted to floods for which harm questions were asked. The match definitions and panel definitions can be found in the text and notes for Table 4. Features used to construct the match are omitted. All regressions include farmer and round fixed effects, standard errors are clustered at the household level, and p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Figure 4: Flood Information Survey Experimental Design



Note: Figure 4 presents the experimental design for the flood information survey experiment.

Table 6: Feature Agreement Among Best Matches

Feature	Agreement rate	Chance rate	Cohen's κ	N
Panel A. Features				
Flood year (exact)	0.365	0.068	0.319	493
Flood year ($\pm$ 1)	0.513	0.115	0.450	493
Flood year ($\pm$ 2)	0.677	0.180	0.607	493
Duration (exact)	0.444	0.314	0.189	493
Duration (within 3 days)	0.517	0.336	0.273	493
Harmed crops (yes/no)	0.978	0.954	0.510	493
Crop damage share (exact)	0.751	0.627	0.332	493
Crop damage share (within 25 pp)	0.951	0.819	0.731	493
Harmed house (yes/no)	0.933	0.523	0.860	493
House damage value (within 10%)	0.418	0.183	0.287	493
House damage value (within 25%)	0.485	0.213	0.345	493
House damage value (within 50%)	0.613	0.294	0.451	493
Panel B. Feature Values				
Flood within past 5 years	0.913	0.835	0.470	493
Flood 5–20 years ago	0.795	0.505	0.586	493
Flood more than 20 years ago	0.858	0.502	0.715	493
Duration equal to one day	0.917	0.902	0.155	493
Duration between one day and one week	0.797	0.699	0.325	493
Duration longer than one week	0.828	0.639	0.523	493
Crop damage share $\leq$ 25%	0.947	0.918	0.353	493
Crop damage share $\geq$ 75%	0.935	0.808	0.662	493
House damage $>$ 75th percentile	0.728	0.643	0.236	279
House damage $<$ 25th percentile	0.713	0.656	0.166	279

Note: Tables 6 reports the rate of agreement on individual flood features between matched flood pairs. Each row corresponds to a single feature and a tolerance threshold for agreement. A flood pair consists of a flood recalled in an earlier survey round and the best-matching flood recalled by the same respondent in a later round, identified via a greedy one-to-one assignment algorithm that prioritizes pairs with the highest match score based on year proximity and feature similarity. A description of this algorithm can be found in the text. Flood year (exact) requires the reported flood year to be identical across rounds, while ($\pm$1) and ($\pm$ 2) allow the year to differ by up to one or two years. Duration (exact) requires identical reported flood lengths, while within 3 days allows the reported duration to differ by up to three days. Harmed crops (yes/no) and harmed house (yes/no) require exact agreement on the binary indicator for whether the flood damaged crops or the house. Crop damage share (exact) requires identical reported shares of crops damaged, while within 25 percentage points allows the shares to differ by up to 25 percentage points. House damage value (within 10%), (within 25%), and (within 50%) require the reported values of house damage to differ by no more than 10, 25, or 50 percent of the larger reported value, respectively. Year-based agreement rates are computed over matched pairs where both rounds report a non-missing flood year. Crop and house damage agreement rates are computed over the subset of matched pairs where harm questions were asked in both rounds. Within feature values, house damage percentiles are calculated excluding zeroes. The chance rate column reports the expected agreement under random pairing, computed by permuting the round-B flood characteristics across matched pairs 500 times and averaging the resulting agreement rates across permutations. Cohen's κ adjusts for this base rate by computing (observed agreement - chance agreement) / (1 - chance agreement), where κ = 0 agreement no better than random, and κ = 1 indicates perfect agreement beyond chance.

Table 7: Impact of Contemporaneous Rain-Status Match on Recall Consistency

	Exact Year	Year ± 1	Year ± 2	Weak Features	Strong Features	Year ± 1 + Strong
Panel A. Same rain status in both rounds vs. different						
Same rain status	0.029*	0.056***	0.063***	0.029	0.011	0.008
	(0.016)	(0.019)	(0.019)	(0.020)	(0.011)	(0.006)
Control mean	0.207	0.330	0.389	0.225	0.042	0.021
Observations	3,862	3,862	3,862	2,743	2,742	2,737
Panel B. Both rounds raining vs. different rain status						
Both rounds raining	0.038	0.086**	0.083**	0.060*	0.031*	0.029**
	(0.030)	(0.034)	(0.035)	(0.036)	(0.018)	(0.014)
Control mean	0.208	0.332	0.391	0.226	0.043	0.021
Observations	2,720	2,720	2,720	1,916	1,916	1,911
Panel C. Both rounds not raining vs. different rain status						
Both rounds not raining	0.025	0.036*	0.051**	0.020	0.004	-0.001
	(0.018)	(0.020)	(0.021)	(0.023)	(0.013)	(0.005)
Control mean	0.207	0.330	0.389	0.226	0.042	0.021
Observations	3,323	3,323	3,323	2,440	2,439	2,434

Note: Table 7 tests whether rainfall conditions on the day of the survey affect the consistency of flood recall across survey rounds. The dependent variable in each column is an indicator for whether a flood recalled in an earlier round can be matched to a flood recalled by the same respondent in a later round, under six match definitions of increasing strictness. Exact year requires the reported flood year to be identical across rounds. Year ± 1 and Year ± 2 allow the reported flood year to differ by up to one or two years, respectively. Weak Feature Match requires agreement on whether the flood harmed crops and whether it harmed the house, approximate agreement on the share of crops damaged (within 25%), and approximate agreement on flood duration (within 3 days). Strong Feature Match requires exact agreement on whether crops and the house were harmed, exact agreement on the share of crops damaged and flood duration, and agreement on the value of house damage within 25% of the larger reported value. Year ±1 and Strong Feature Match requires both the year-based and strong feature criteria to be jointly satisfied. Year-based match definitions are computed over floods with a non-missing reported flood year. Feature-based match definitions are computed over the subset of floods for which harm questions were asked in both the earlier and later round. The sample is restricted to floods whose reported year predates the survey year of the earlier round. In Panel A, the regressor is an indicator equal to one if the enumerator recorded the same rainfall status (both raining or both not raining) on the survey dates of the earlier and later rounds, with the omitted category being different rainfall across the two rounds. Panel B restricts the sample to floods where it was either raining on both survey dates or raining on one but not the other (excluding observations where it was not raining on either date), so the regressor captures the effect of both rounds being conducted in the rain relative to only one. Panel C restricts to floods where it was either not raining on both survey dates or raining on one but not the other (excluding observations where it was raining on both dates), so the regressor captures the effect of both rounds being conducted without rain relative to only one. All regressions include village and round fixed effects. Control means report the average match rate among the different-rain-status group within each panel's sample. Standard errors are clustered at the household level, and p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table 8: Flood Information Experiment Treatment Effects on Insurance Demand by Flood Risk

	(1) All Quintiles	(2) Top 4 Quintiles	(3) Top 3 Quintiles	(4) Top 2 Quintiles	(5) Top Quintile
Flood Before	-6.311**	-6.580**	-3.852	-7.366	-6.948
	(2.781)	(3.282)	(3.942)	(4.614)	(6.609)
Damage Before	8.117**	8.819**	8.855*	10.745*	5.701
	(3.341)	(3.892)	(4.690)	(5.602)	(7.479)
Flood After	-0.896	-1.494	-1.160	-1.506	-4.168
	(3.079)	(3.510)	(4.020)	(4.929)	(6.641)
Damage After	-2.165	-2.870	-1.517	-0.934	1.912
	(3.516)	(4.012)	(4.766)	(5.977)	(8.204)
Control mean	37.007	39.422	39.208	40.192	39.675
Observations	2,082	1,640	1,230	804	390

Note: Table 8 presents treatment effects of the information treatments on willingness to pay for index insurance across different sub-samples by respondents' predicted flood risk. Willingness to pay is elicited via a Becker-DeGroot-Marschak mechanism and top-coded at 200 BDT. The experiment randomly assigns respondents to receive information either before or after the flood memory elicitation or to a pure control group. Within each timing condition, respondents receive either flood occurrence information alone or flood occurrence combined with damage information. "Flood Before" and "Flood After" are indicators for receiving flood occurrence information before or after the memory elicitation, respectively. "Damage Before" and "Damage After" are indicators for additionally receiving damage information before or after the elicitation; these coefficients capture the incremental effect of damage information beyond flood occurrence information alone. Predicted flood risk is constructed from a baseline prediction model, and respondents are divided into quintiles of this distribution. Column (1) includes all respondents. Columns (2) through (5) progressively restrict the sample to respondents with higher predicted flood risk: the top four quintiles, top three quintiles, top two quintiles, and the top quintile only. The control mean reports the average willingness to pay among respondents in the control group (those who received no information treatment) within each column's sample. All regressions use heteroskedasticity-robust standard errors, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table 9: Flood Information Experiment Treatment Effects on Memories

	(1) Share House Damaged	(2) Share Crop Damaged	(3) Mean Crop Damage Amount	(4) Mean House Damage Amount	(5) Mean Flood Duration	(6) Severity Index
Flood Before	-0.049	-0.035	-0.043	-2367.780	-1.349	-0.092
	(0.045)	(0.045)	(0.041)	(1775.314)	(1.104)	(0.106)
Damage Before	0.093*	0.074	0.089*	3090.128	1.819	0.221*
	(0.051)	(0.051)	(0.047)	(2103.200)	(1.278)	(0.122)
Flood After	-0.030	-0.000	0.003	-1382.810	-0.511	-0.027
	(0.045)	(0.045)	(0.042)	(1853.513)	(1.134)	(0.099)
Damage After	0.005	0.004	-0.003	1320.261	0.831	0.006
	(0.052)	(0.052)	(0.048)	(2180.337)	(1.333)	(0.115)
Control mean	0.420	0.720	0.600	14,118.921	12.195	0.211
Observations	853	853	853	853	853	853

Note: Table 9 presents presents treatment effects of the information treatments on the features of floods recalled during the memory elicitation, restricting the sample to respondents in the top two quintiles of predicted flood risk. The experiment randomly assigns respondents to receive information either before or after the flood memory elicitation or to a pure control group. Within each timing condition, respondents receive either flood occurrence information alone or flood occurrence combined with damage information. "Flood Before" and "Flood After" are indicators for receiving flood occurrence information before or after the memory elicitation, respectively. "Damage Before" and "Damage After" are indicators for additionally receiving damage information before or after the elicitation; these coefficients capture the incremental effect of damage information beyond flood occurrence information alone. Column (1) shows impacts on the share of recalled floods that damaged the house. Column (2) shows impacts on the share of recalled floods that damaged crops. Column (3) shows impacts on the mean share of crops damaged across recalled floods. Column (4) shows impacts on the mean value of house damage across recalled floods, winsorized at the 90th percentile. Column (5) shows impacts on the mean duration in days across recalled floods. Column (6) shows impacts on a summary index of flood severity, constructed following Kling et al. (2007) by averaging the standardized values of the five component measures, where each is standardized by subtracting the control group mean and dividing by the control group standard deviation, and the resulting index is re-standardized to have a control group mean of zero and standard deviation of one. For all outcomes, respondents who recalled no floods are assigned a value of zero. The control mean reports the average outcome among respondents in the pure control group. All regressions use heteroskedasticity-robust standard errors, and p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table 10: Flood Information Experiment Treatment Effects on Beliefs

	(1) Expected Flood Days Next Year	(2) Expected Flood Days Next Five Years	(3) Expected Crop Damage Next Year	(4) Expected Crop Damage Next Five Years	(5) Expectations Index
Flood Before	-0.111	0.215	0.014	-0.003	0.015
	(0.616)	(0.796)	(0.034)	(0.035)	(0.085)
Damage Before	0.987	1.546	0.029	0.036	0.125
	(0.752)	(0.969)	(0.039)	(0.040)	(0.102)
Flood After	-0.255	0.311	0.006	0.025	0.025
	(0.659)	(0.823)	(0.034)	(0.036)	(0.088)
Damage After	0.510	1.532	-0.005	0.039	0.092
	(0.774)	(0.994)	(0.040)	(0.042)	(0.104)
Control mean	5.500	8.129	0.368	0.498	-0.007
Observations	848	848	842	842	852

Note: Table 10 presents treatment effects of the information treatments on respondents' expectations about future flooding, restricting the sample to respondents in the top two quintiles of predicted flood risk. The experiment randomly assigns respondents to receive information either before or after the flood memory elicitation or to a pure control group. Within each timing condition, respondents receive either flood occurrence information alone or flood occurrence combined with damage information. "Flood Before" and "Flood After" are indicators for receiving flood occurrence information before or after the memory elicitation, respectively. "Damage Before" and "Damage After" are indicators for additionally receiving damage information before or after the elicitation; these coefficients capture the incremental effect of damage information beyond flood occurrence information alone. Column (1) shows impacts on the expected number of flood days in the next year. Column (2) shows impacts on the expected number of flood days over the next five years. Column (3) shows impacts on the expected share of crops damaged by flooding in the next year. Column (4) shows impacts on the expected share of crops damaged by flooding over the next five years. Column (5) shows impacts on a summary index of flood expectations, constructed following Kling et al. (2007) by averaging the standardized values of the four component measures, where each is standardized by subtracting the control group mean and dividing by the control group standard deviation, and the resulting index is re-standardized to have a control group mean of zero and standard deviation of one. The control mean reports the average outcome among respondents in the pure control group. All regressions use heteroskedasticity-robust standard errors, and *p*-values are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11: The Impact of Contemporaneous Rainfall on Monsoon Memories

	(1)	(2)	(3)	(4)
Rain on Survey Day	-0.059***	-0.054***	-0.056***	-0.042***
	(0.006)	(0.008)	(0.008)	(0.007)
Rain on Survey Day × Years Ago	0.011***	0.013***	0.013***	0.014***
	(0.001)	(0.001)	(0.001)	(0.001)
Years Ago	-0.003***	-0.004***	-0.003***	-0.004***
	(0.001)	(0.001)	(0.001)	(0.001)
Control mean	0.702	0.702	0.702	0.702
Observations	7,787	7,782	7,782	7,782
Question order FE	✓	✓	✓	✓
Farmer FE		✓	✓	✓
Round FE			✓	
Village × Round FE				✓

Note: Table 11 tests whether the effect of rainfall on the day of the survey on recalled monsoon intensity grows with the temporal distance of the memory being elicited. The dependent variable measures past monsoon intensity recall: memories of monsoon intensity 0, 1, 2, 5, or 10 years ago (randomly assigned across respondents). Farmers report their recalled monsoon intensity by distributing 10 buttons across bins representing the number of rainy days per two-week period during the monsoon season. "Rain on Survey Day" captures the effect of contemporaneous rainfall on recalled monsoon intensity, as reported by the enumerator. "Rain on Survey Day × Years Ago" captures how the rain-today effect changes per additional year of temporal distance in the memory being elicited. "Years Ago" controls for the direct relationship between temporal distance and reported monsoon intensity. Column (1) includes question order fixed effects only. Column (2) adds farmer fixed effects. Column (3) adds farmer and round fixed effects. Column (4) includes farmer, village-by-round, and question order fixed effects. All standard errors are clustered at the village level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Appendices

Table of Contents

A Additional Figures and Tables

Table A.1: Farmer Survey Demographics

	Mean	Median
Household Size	4.7	4
Female	.033	0
Age	46	45
Years of Schooling	6.4	5
No Schooling	.21	0
Fewer than 5 Years of Schooling	.52	1
Completed Primary Schooling	.31	0
High School or Above	.13	0
Annual Earnings (USD)	1,656	1,365
Number of Plots	2.9	2
Survey Plot Size	46	33
Years Farming	22	20
Years Farming on Survey Plot	13	10
Lived Elsewhere in Past	.049	0
N	2,261	

Note: Table A.1 presents summary statistics for the main sample of farmers. The survey plot is the plot on which most of the survey questions focused, which was randomly selected with weights proportional to size.

Table A.2: Salinity Trend Summary Statistics

Panel A. Distributions of Responses

Response	Past 10 Years	Future 10 Years
Salinity Increased	0.399	0.468
Salinity Decreased	0.420	0.335
Salinity Stayed the Same	0.163	0.153
Don't Know	0.018	0.044
Total N	6,663	6,663

Panel B. Share of future response (columns) conditional on past response (rows)

Past / Future	Increased	Decreased	Stayed the Same	Don't Know	Row N
Salinity Increased	0.834	0.098	0.048	0.020	2,659
Salinity Decreased	0.220	0.648	0.103	0.030	2,800
Salinity Stayed the Same	0.247	0.136	0.543	0.075	1,087
Don't Know	0.145	0.077	0.120	0.658	117

Panel C. Share of past response (columns) conditional on future response (rows)

Future / Past	Increased	Decreased	Stayed the Same	Don't Know	Row N
Salinity Increased	0.711	0.197	0.086	0.005	3,118
Salinity Decreased	0.117	0.813	0.066	0.004	2,232
Salinity Stayed the Same	0.125	0.282	0.578	0.014	1,020
Don't Know	0.177	0.283	0.276	0.263	293

Note: Table A.2 presents summary statistics for salinity trend memories and forecasts. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" The future question is, "Think about the next 10 years. Do you think the amount of salt in the soil in the typical plot in your village will increase, decrease, or stay the same from now until then?" Panel A shows the share of farmers answering each response to each question. Panels B and C show how answers to the future vary by the answer given to the past question and vice versa.

Table A.3: Salinity Trend Disagreement Statistics

Statistic	Past 10 Years	Future 10 Years
Panel A. Within-village-round disagreement across households		
Mean of max. village share w/ same answer	0.658	0.654
Odds of neighbor w/ same answer	0.506	0.498
Village-round cells	750	750
Panel B. Within-household disagreement across time		
Share households ever change answer	0.733	0.726
Share changing from decrease to increase	0.253	0.224
Share changing from increase to decrease	0.312	0.273
Odds of household giving same answer again	0.449	0.452
Households with >1 round	2,271	2,271
Panel C. Village disagreement: decrease vs. increase/stay same		
Mean of max. village share w/ same answer	0.743	0.768
Odds of neighbor w/ same answer	0.619	0.644
Village-round cells	750	750
Panel D. Household disagreement: decrease vs. increase/stay same		
Share households ever change answer	0.584	0.505
Share changing from decrease to increase/stay same	0.394	0.324
Share changing from increase/stay same to decrease	0.365	0.318
Odds of household giving same answer again	0.595	0.643
Households with >1 round	2,261	2,247

Note: Table A.3 presents statistics on within village and over time disagreement for salinity trend memories and forecasts. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" The future question is, "Think about the next 10 years. Do you think the amount of salt in the soil in the typical plot in your village will increase, decrease, or stay the same from now until then?" Panels A and B use all four response options (Increase, Decrease, Stay the Same, and Don't Know), while Panels C and D restrict to Decrease vs. Increase/Stay the Same. Panels A and C report disagreement within village-round but across households. Panels B and D report disagreement within households but over rounds.

Figure A.1: Random Forest Performance—Past Decrease

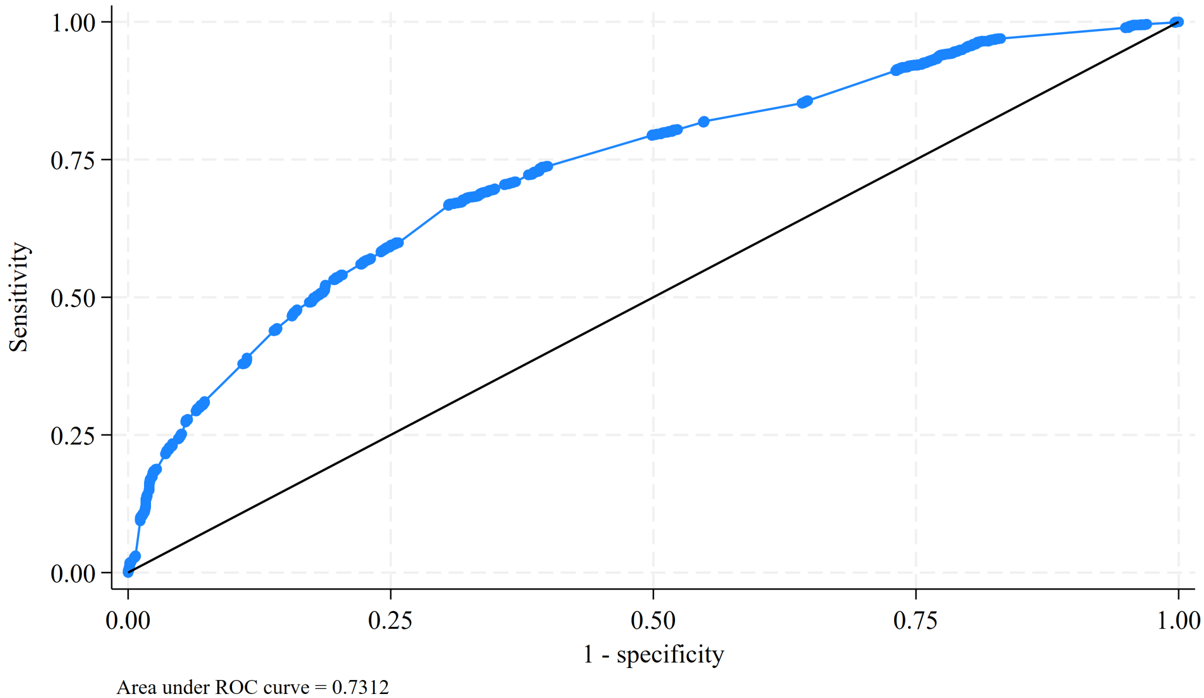
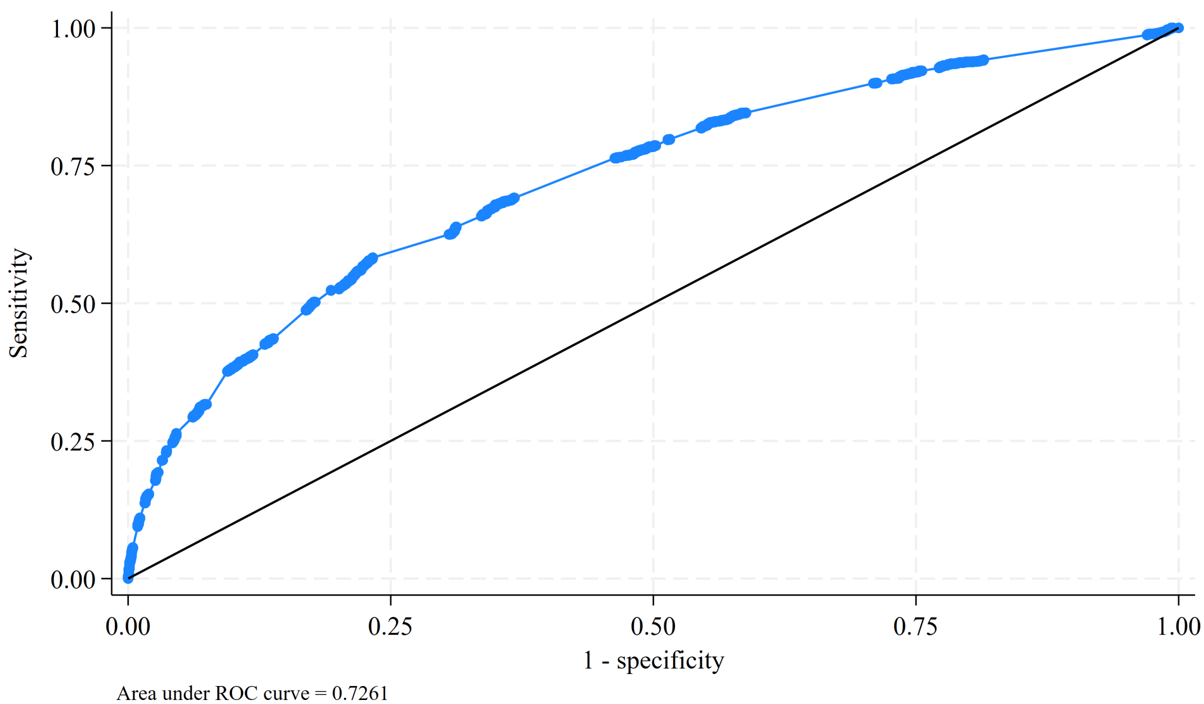


Figure A.2: Random Forest Performance—Future Decrease



Note: Figures A.1 and A.2 plot the receiver operating characteristic (ROC) curve for a random forest classifier predicting whether a respondent reports that soil salinity in their village decreased over the past decade (Figure A.1) and whether a respondent reports that soil salinity in their village will decrease in the next decade (Figure A.2). The model is trained on a random 30% of households (held constant within household across survey rounds) and evaluated on the remaining 70%. The diagonal line represents the performance of a random classifier. AUC statistics are reported below each graph.

Table A.4: Salinity Question Order Experiment Full Results—Beliefs

	No FEs			Village + Round FEs			HHID + Village × Round FEs		
	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)
Asked Past First	0.012	0.033**	0.047***	0.008	0.029**	0.054***	-0.007	0.027*	0.047***
	(0.018)	(0.013)	(0.013)	(0.018)	(0.013)	(0.012)	(0.020)	(0.015)	(0.015)
Past Decrease (Prediction)	0.531***	0.282***	0.369***	0.172***	0.093***	0.190***	-3.238***	-0.293***	-0.148***
	(0.029)	(0.019)	(0.019)	(0.036)	(0.021)	(0.022)	(0.133)	(0.041)	(0.051)
Asked Past First × Past Decrease (Prediction)	0.067*	0.034	0.053**	0.088**	0.053**	0.066***	0.066	0.032	0.042
	(0.039)	(0.026)	(0.026)	(0.038)	(0.025)	(0.024)	(0.041)	(0.029)	(0.030)
Observations	6,141	6,141	6,141	6,141	6,141	6,141	6,079	6,079	6,079
Unique farmers	2,266	2,266	2,266	2,266	2,266	2,266	2,204	2,204	2,204
Control mean	0.315	0.315	0.315	0.315	0.315	0.315	0.316	0.316	0.316

Note: Table A.4 presents all three terms of a difference-in-differences regression of being asked about past salinity trends first, the prediction of the respondent answering that in the past, salinity declined, and the interaction of those two. The prediction of a past decrease in the first, fourth, and seventh columns corresponds to the share of the respondents' neighbors in their village in that same survey wave who answered decrease (the leave-out-mean). In the second, fifth, and eighth columns, the prediction is a binarized version of that variable equal to one if the majority of neighbors answered a decrease and zero otherwise. In the third, sixth, and ninth columns, the prediction comes from a random forest model trained on the leave-out-mean, soil salinity on a randomly selected plot of the respondent as measured during the baseline survey, household size, gender, age, indicators for having a household member over 60 and a male household member over 60, years of schooling, indicators for different levels of schooling, indicator for having migrated in the past, log household earnings, total number of plots, and years cultivating Boro season rice. The first three columns do not include any fixed effects; the next three include village and survey round fixed effects; the last three include household and village-by-round fixed effects. All standard errors are clustered at the household level, and the sample excludes those who failed survey comprehension checks. The outcome is whether the respondent predicted that salinity will decrease in the future. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" The future question is, "Think about the next 10 years. Do you think the amount of salt in the soil in the typical plot in your village will increase, decrease, or stay the same from now until then?" Standard errors are reported in parentheses, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table A.5: Salinity Question Order Experiment Full Results—Seed WTP

	No FEs			Village + Round FEs			HHID + Village × Round FEs		
	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)
Asked Past First	8.503**	3.056	-0.702	10.697***	6.077**	1.192	5.840	2.068	-6.428**
	(3.980)	(2.570)	(2.483)	(3.799)	(2.447)	(2.355)	(4.762)	(3.219)	(3.173)
Past Decrease (Prediction)	6.782	2.414	7.154**	26.237***	10.747**	16.330***	42.154	23.037***	20.880*
	(5.562)	(3.519)	(3.558)	(7.233)	(4.240)	(4.353)	(28.083)	(8.626)	(11.018)
Asked Past First × Past Decrease (Prediction)	-16.800**	-4.818	-11.996**	-17.222**	-7.782	-12.380**	-21.330**	-16.585**	-17.283**
	(8.127)	(4.871)	(5.114)	(7.979)	(4.764)	(4.952)	(10.095)	(6.560)	(7.017)
Observations	4,116	4,116	4,116	4,116	4,116	4,116	3,740	3,740	3,740
Unique farmers	2,246	2,246	2,246	2,246	2,246	2,246	1,870	1,870	1,870
Control mean	70.498	70.498	70.498	70.498	70.498	70.498	71.126	71.126	71.126

Note: Table A.5 presents all three terms of a difference-in-differences regression of being asked about past salinity trends first, the prediction of the respondent answering that in the past, salinity declined, and the interaction of those two. The prediction of a past decrease in the first, fourth, and seventh columns corresponds to the share of the respondents' neighbors in their village in that same survey wave who answered decrease (the leave-out-mean). In the second, fifth, and eighth columns, the prediction is a binarized version of that variable equal to one if the majority of neighbors answered a decrease and zero otherwise. In the third, sixth, and ninth columns, the prediction comes from a random forest model trained on the leave-out-mean, soil salinity on a randomly selected plot of the respondent as measured during the baseline survey, household size, gender, age, indicators for having a household member over 60 and a male household member over 60, years of schooling, indicators for different levels of schooling, indicator for having migrated in the past, log household earnings, total number of plots, and years cultivating Boro season rice. The first three columns do not include any fixed effects; the next three include village and survey round fixed effects; the last three include household and village-by-round fixed effects. All standard errors are clustered at the household level, and the sample excludes those who failed survey comprehension checks. The outcome is the Becker-DeGroot-Marschak elicitation of willingness-to-pay for a salinity tolerant seed. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" Standard errors are reported in parentheses, and p-values are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.6: Salinity Question Order Experiment Full Results—Plate WTP (Placebo)

	No FEs			Village + Round FEs			HHID + Village × Round FEs		
	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)
Asked Past First	1.563	0.944	0.903	1.405	1.234	1.184	-1.447	-0.514	0.516
	(1.521)	(1.003)	(0.848)	(1.509)	(0.990)	(0.818)	(1.911)	(1.284)	(1.041)
Past Decrease (Prediction)	-4.427**	-3.555***	-2.936**	4.035	0.411	1.406	3.499	-4.566	-0.933
	(2.021)	(1.154)	(1.239)	(2.535)	(1.358)	(1.441)	(9.869)	(2.813)	(3.455)
Asked Past First × Past Decrease (Prediction)	-1.207	0.249	-0.962	-0.225	0.303	-0.655	4.289	2.528	1.293
	(2.928)	(1.667)	(1.776)	(2.924)	(1.659)	(1.752)	(3.670)	(2.173)	(2.332)
Observations	4,116	4,116	4,116	4,116	4,116	4,116	3,740	3,740	3,740
Unique farmers	2,246	2,246	2,246	2,246	2,246	2,246	1,870	1,870	1,870
Control mean	26.352	26.352	26.352	26.352	26.352	26.352	26.622	26.622	26.622

Note: Table A.6 presents all three terms of a difference-in-differences regression of being asked about past salinity trends first, the prediction of the respondent answering that in the past, salinity declined, and the interaction of those two. The prediction of a past decrease in the first, fourth, and seventh columns corresponds to the share of the respondents' neighbors in their village in that same survey wave who answered decrease (the leave-out-mean). In the second, fifth, and eighth columns, the prediction is a binarized version of that variable equal to one if the majority of neighbors answered a decrease and zero otherwise. In the third, sixth, and ninth columns, the prediction comes from a random forest model trained on the leave-out-mean, soil salinity on a randomly selected plot of the respondent as measured during the baseline survey, household size, gender, age, indicators for having a household member over 60 and a male household member over 60, years of schooling, indicators for different levels of schooling, indicator for having migrated in the past, log household earnings, total number of plots, and years cultivating Boro season rice. The first three columns do not include any fixed effects; the next three include village and survey round fixed effects; the last three include household and village-by-round fixed effects. All standard errors are clustered at the household level, and the sample excludes those who failed survey comprehension checks. The outcome is the Becker-DeGroot-Marschak elicitation of willingness-to-pay for a plastic plate. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" Standard errors are reported in parentheses, and p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table A.7: Salinity Question Order Reverse Experiment

	No FEs			Village + Round FEs			HHID + Village × Round FEs		
	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)
Asked Future First	-0.050***	-0.049***	-0.050***	-0.042**	-0.045***	-0.054***	-0.036*	-0.047***	-0.051***
	(0.018)	(0.013)	(0.014)	(0.018)	(0.013)	(0.014)	(0.020)	(0.016)	(0.017)
Future Decrease (Prediction)	0.624***	0.360***	0.431***	0.289***	0.144***	0.274***	-3.703***	-0.411***	-0.312***
	(0.029)	(0.020)	(0.018)	(0.041)	(0.025)	(0.025)	(0.150)	(0.047)	(0.060)
Asked Future First × Future Decrease (Prediction)	-0.011	-0.020	-0.007	-0.028	-0.029	-0.018	0.018	0.012	-0.002
	(0.040)	(0.028)	(0.025)	(0.040)	(0.028)	(0.025)	(0.043)	(0.032)	(0.031)
Observations	6,141	6,141	6,141	6,141	6,141	6,141	6,079	6,079	6,079
Unique farmers	2,266	2,266	2,266	2,266	2,266	2,266	2,204	2,204	2,204
Control mean	0.446	0.446	0.446	0.446	0.446	0.446	0.446	0.446	0.446

Note: Table A.7 presents all three terms of a difference-in-differences regression of being asked about future salinity trends first, the prediction of the respondent answering that in the future, salinity will decline, and the interaction of those two. The prediction of a future decrease in the first, fourth, and seventh columns corresponds to the share of the respondents' neighbors in their village in that same survey wave who answered decrease (the leave-out-mean). In the second, fifth, and eighth columns, the prediction is a binarized version of that variable equal to one if the majority of neighbors answered a decrease and zero otherwise. In the third, sixth, and ninth columns, the prediction comes from a random forest model trained on the leave-out-mean, soil salinity on a randomly selected plot of the respondent as measured during the baseline survey, household size, gender, age, indicators for having a household member over 60 and a male household member over 60, years of schooling, indicators for different levels of schooling, indicator for having migrated in the past, log household earnings, total number of plots, and years cultivating Boro season rice. The first three columns do not include any fixed effects; the next three include village and survey round fixed effects; the last three include household and village-by-round fixed effects. All standard errors are clustered at the household level, and the sample excludes those who failed survey comprehension checks. The outcome is whether the respondent reported that salinity decreased in the past. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" The future question is, "Think about the next 10 years. Do you think the amount of salt in the soil in the typical plot in your village will increase, decrease, or stay the same from now until then?" Standard errors are reported in parentheses, and p-values are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.8: Distribution of Number of Floods Recalled by Farmer-Round

Number of Recalled Floods	Count	Share
0	2,636	0.396
1	2,247	0.337
2	1,273	0.191
3	388	0.058
4	90	0.014
5	15	0.002
6	7	0.001
7	3	0.000
8	2	0.000
10	2	0.000
Total	6,663	1.000

Note: Table A.8 presents the distribution of the number of flood memories recalled by respondents per farmer-by-survey.

Table A.9: Flood Memory Feature-Wise Summary Statistics and Cross-Correlation

	Mean	Cross-correlation matrix				
		Length (Days)	Harm Crops	Share Crop Damage	Harm House	Value House Damage (BDT)
Length (Days)	18.527	1.000	0.200***	0.406***	-0.058***	0.024*
Harm Crops	0.920	0.200***	1.000	0.695***	0.055***	0.066***
Share Crop Damage	0.778	0.406***	0.695***	1.000	0.123***	0.149***
Harm House	0.420	-0.058***	0.055***	0.123***	1.000	0.766***
Value House Damage (BDT)	11,841.868	0.024*	0.066***	0.149***	0.766***	1.000
N	5,182					

Note: Column 1 of Table A.9 presents the average value for each feature among the set of flood memories with non-missing information on all five features. Columns 2 through 5 present cross-correlation matrices for these features among the same sample.

Table A.10: Impact of Being Asked About Oldest Flood First on Number of Memories

	(1)	(2)	(3)	(4)	(5)	(6)
Recalled Oldest Flood First	0.014	0.010	-0.010	0.014	0.010	-0.010
	(0.025)	(0.021)	(0.021)	(0.023)	(0.019)	(0.022)
Control mean	0.967	0.967	0.967	0.967	0.967	0.967
Observations	6,663	6,663	6,655	6,663	6,663	6,655
SE clustered at	Farmer	Farmer	Farmer	Village	Village	Village
Treatment FE	✓	✓	✓	✓	✓	✓
Village FE		✓			✓	
Round FE		✓			✓	
Farmer FE			✓			✓
Village $\times$ Round FE			✓			✓

Note: Table A.10 presents regressions of the number of floods recalled by each farmer in each survey round on an indicator for whether that farmer was asked to recall floods beginning with the oldest flood first.

Table A.11: Flood Memory Spillover Feature Impacts of Flood Length

	Length (Days)	Harm Crops	Share Crop Damage	Harm House	Value House Damage (BDT)
Panel A. First mention					
Leave-out mean	0.705***	0.005***	0.011***	0.006***	383.229***
	(0.031)	(0.001)	(0.001)	(0.002)	(77.613)
Corr. with main variable	1.000	0.165	0.394	-0.051	0.042
Corr. p-value	0.000	0.000	0.000	0.013	0.046
Mean of outcome	18.334	0.931	0.782	0.460	12,860.816
Observations	3,043	2,334	2,319	2,339	2,267
Unique farmers	1,257	1,033	1,026	1,036	1,003
Panel B. Reduced form					
Leave-out mean	0.573***	0.002	0.007***	-0.009**	-1.268
	(0.055)	(0.002)	(0.002)	(0.004)	(142.419)
Corr. with main variable	1.000	0.230	0.435	-0.162	-0.019
Corr. p-value	0.000	0.000	0.000	0.000	0.502
Mean of outcome	18.727	0.912	0.782	0.437	12,615.263
Observations	1,767	1,308	1,305	1,315	1,284
Unique farmers	591	472	471	475	468
Panel C. IV					
First-mentioned flood characteristic	0.721***	0.003	0.009***	-0.012**	-20.722
	(0.077)	(0.002)	(0.003)	(0.005)	(177.700)
Corr. with main variable	1.000	0.228	0.434	-0.159	-0.016
Corr. p-value	0.000	0.000	0.000	0.000	0.573
First-stage F-statistic	221.75	176.53	176.37	177.99	165.58
Mean of outcome	18.731	0.912	0.782	0.436	12,593.186
Observations	1,762	1,301	1,298	1,308	1,277
Unique farmers	589	469	468	472	465

Note: Table A.11 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of flood duration (length in days) as the instrument. The first column reports estimates for flood duration itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: whether crops were harmed, the share of crops damaged, whether the house was harmed, and the value of house damage in BDT. The leave-out-mean of flood duration is constructed as the average flood duration reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of flood duration. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of flood duration. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned flood duration is instrumented by the leave-out-mean of flood duration. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between flood duration and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the household level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table A.12: Flood Memory Spillover Feature Impacts of Crop Damage

	Harm Crops	Length (Days)	Share Crop Damage	Harm House	Value House Damage (BDT)
Panel A. First mention					
Leave-out mean	0.156**	4.152***	0.256***	0.047	3,132.067
	(0.065)	(1.272)	(0.065)	(0.074)	(2,480.754)
Corr. with main variable	1.000	0.173	0.664	0.040	0.061
Corr. p-value	0.000	0.000	0.000	0.053	0.004
Mean of outcome	0.931	18.595	0.783	0.464	12,991.262
Observations	2,305	2,933	2,290	2,308	2,235
Unique farmers	1,024	1,225	1,017	1,026	992
Panel B. Reduced form					
Leave-out mean	0.014	1.605	0.095	0.098	-2,194.398
	(0.128)	(2.221)	(0.127)	(0.149)	(6,241.083)
Corr. with main variable	1.000	0.231	0.720	0.053	0.035
Corr. p-value	0.000	0.000	0.000	0.055	0.216
Mean of outcome	0.914	18.750	0.783	0.437	12,592.475
Observations	1,300	1,737	1,297	1,307	1,276
Unique farmers	469	579	468	472	465
Panel C. IV					
First-mentioned flood characteristic	-0.061	-69.196	-2.366	-2.617	17,355.082
	(4.097)	(214.933)	(10.193)	(10.070)	(68,912.368)
Corr. with main variable	1.000	0.232	0.719	0.056	0.040
Corr. p-value	0.000	0.000	0.000	0.043	0.151
First-stage F-statistic	0.09	0.11	0.08	0.11	0.84
Mean of outcome	0.914	18.289	0.783	0.436	12,580.517
Observations	1,297	1,306	1,294	1,304	1,273
Unique farmers	468	472	467	471	464

Note: Table A.12 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of whether the flood harmed crops as the instrument. The first column reports estimates for crop harm itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: flood duration (length in days), the share of crops damaged, whether the house was harmed, and the value of house damage in BDT. The leave-out-mean of crop harm is constructed as the average of the crop harm indicator reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of crop harm. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of crop harm. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned crop harm indicator is instrumented by the leave-out-mean of crop harm. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between crop harm and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the household level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table A.13: Flood Memory Spillover Feature Impacts of Share Crop Damage

	Share Crop Damage	Length (Days)	Harm Crops	Harm House	Value House Damage (BDT)
Panel A. First mention					
Leave-out mean	0.367***	6.621***	0.127***	0.158***	7,281.294***
	(0.047)	(1.006)	(0.043)	(0.057)	(2,128.503)
Corr. with main variable	1.000	0.392	0.664	0.133	0.161
Corr. p-value	0.000	0.000	0.000	0.000	0.000
Mean of outcome	0.783	18.595	0.931	0.464	12,991.262
Observations	2,290	2,933	2,305	2,308	2,235
Unique farmers	1,017	1,225	1,024	1,026	992
Panel B. Reduced form					
Leave-out mean	0.263***	6.132***	-0.015	-0.329**	-10,857.323**
	(0.084)	(1.714)	(0.070)	(0.130)	(4,596.814)
Corr. with main variable	1.000	0.437	0.720	0.039	0.073
Corr. p-value	0.000	0.000	0.000	0.164	0.009
Mean of outcome	0.783	18.750	0.914	0.437	12,592.475
Observations	1,297	1,737	1,300	1,307	1,276
Unique farmers	468	579	469	472	465
Panel C. IV					
First-mentioned flood characteristic	0.445***	13.877***	-0.041	-0.581**	-20,153.189**
	(0.146)	(3.995)	(0.130)	(0.229)	(8,544.891)
Corr. with main variable	1.000	0.439	0.719	0.039	0.077
Corr. p-value	0.000	0.000	0.000	0.160	0.006
First-stage F-statistic	45.09	46.33	42.60	44.38	39.42
Mean of outcome	0.783	18.298	0.914	0.436	12,588.511
Observations	1,292	1,302	1,293	1,300	1,271
Unique farmers	466	470	466	469	463

Note: Table A.13 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of the share of crops damaged as the instrument. The first column reports estimates for the share of crops damaged itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: flood duration (length in days), whether crops were harmed, whether the house was harmed, and the value of house damage in BDT. The leave-out-mean of crop damage share is constructed as the average share of crops damaged reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of crop damage share. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of crop damage share. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned share of crops damaged is instrumented by the leave-out-mean of crop damage share. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between the share of crops damaged and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the household level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table A.14: Flood Memory Spillover Feature Impacts of Property Damage

	Harm House	Length (Days)	Harm Crops	Share Crop Damage	Value House Damage (BDT)
Panel A. First mention					
Leave-out mean	0.341***	1.670**	0.004	0.059*	10,240.455***
	(0.044)	(0.777)	(0.029)	(0.033)	(1,834.986)
Corr. with main variable	1.000	-0.048	0.040	0.132	0.761
Corr. p-value	0.000	0.017	0.056	0.000	0.000
Mean of outcome	0.464	18.595	0.931	0.783	12,991.262
Observations	2,308	2,933	2,305	2,290	2,235
Unique farmers	1,026	1,225	1,024	1,017	992
Panel B. Reduced form					
Leave-out mean	0.259***	-1.441	-0.035	-0.169***	5,437.115**
	(0.066)	(1.093)	(0.032)	(0.041)	(2,750.560)
Corr. with main variable	1.000	-0.160	0.049	0.035	0.780
Corr. p-value	0.000	0.000	0.075	0.207	0.000
Mean of outcome	0.437	18.750	0.914	0.783	12,592.475
Observations	1,307	1,737	1,300	1,297	1,276
Unique farmers	472	579	469	468	465
Panel C. IV					
First-mentioned flood characteristic	0.687***	-5.775	-0.092	-0.442***	15,491.116*
	(0.231)	(3.563)	(0.084)	(0.153)	(8,585.554)
Corr. with main variable	1.000	-0.167	0.050	0.034	0.780
Corr. p-value	0.000	0.000	0.074	0.215	0.000
First-stage F-statistic	18.98	19.18	19.29	19.18	15.57
Mean of outcome	0.437	18.287	0.914	0.783	12,602.351
Observations	1,306	1,308	1,299	1,296	1,275
Unique farmers	472	473	469	468	465

Note: Table A.14 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of whether the flood harmed the house as the instrument. The first column reports estimates for house harm itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: flood duration (length in days), whether crops were harmed, the share of crops damaged, and the value of house damage in BDT. The leave-out-mean of house harm is constructed as the average of the house harm indicator reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of house harm. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of house harm. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned house harm indicator is instrumented by the leave-out-mean of house harm. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between house harm and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the household level, and p-values are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.15: Flood Memory Spillover Feature Impacts of House Damage Amount

	Value House Damage (BDT)	Length (Days)	Harm Crops	Share Crop Damage	Harm House
Panel A. First mention					
Leave-out mean	0.335***	0.085***	-0.000	0.001	0.005***
	(0.050)	(0.020)	(0.001)	(0.001)	(0.001)
Corr. with main variable	1.000	0.036	0.063	0.165	0.759
Corr. p-value	0.000	0.073	0.003	0.000	0.000
Mean of outcome	12,991.262	18.614	0.931	0.785	0.463
Observations	2,235	2,920	2,293	2,280	2,298
Unique farmers	992	1,219	1,018	1,012	1,021
Panel B. Reduced form					
Leave-out mean	0.167**	0.010	-0.000	-0.002**	0.003*
	(0.067)	(0.025)	(0.001)	(0.001)	(0.002)
Corr. with main variable	1.000	-0.016	0.032	0.073	0.781
Corr. p-value	0.000	0.561	0.250	0.009	0.000
Mean of outcome	12,599.214	18.757	0.914	0.783	0.436
Observations	1,275	1,734	1,297	1,296	1,304
Unique farmers	465	578	468	468	471
Panel C. IV					
First-mentioned flood characteristic	0.482**	0.038	-0.001	-0.005*	0.008*
	(0.212)	(0.076)	(0.002)	(0.003)	(0.005)
Corr. with main variable	1.000	-0.026	0.027	0.065	0.784
Corr. p-value	0.000	0.358	0.337	0.021	0.000
First-stage F-statistic	17.14	20.02	20.18	20.12	19.86
Mean of outcome	12,685.347	18.610	0.916	0.786	0.436
Observations	1,249	1,274	1,265	1,264	1,272
Unique farmers	454	460	456	456	459

Note: Table A.15 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of the value of house damage (in BDT) as the instrument. The first column reports estimates for the value of house damage itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: flood duration (length in days), whether crops were harmed, the share of crops damaged, and whether the house was harmed. The leave-out-mean of house damage value is constructed as the average value of house damage reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. For spillover columns in this table, coefficients and standard errors are scaled by 1,000. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of house damage value. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of house damage value. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned value of house damage is instrumented by the leave-out-mean of house damage value. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between the value of house damage and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the household level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table A.16: The Impact of Contemporaneous Rainfall on Beliefs and Adaptation

	Insurance WTP	Expected Flood Days Next Year	Expected Flood Days Next Five Years	Expected Crop Damage Next Year	Expected Crop Damage Next Five Years
Panel A. No fixed effects					
Rain on Survey Day	-5.537***	-0.677	-0.660	-0.032	-0.030
	(2.037)	(0.435)	(0.481)	(0.021)	(0.020)
Control mean	32.869	5.024	7.287	0.304	0.404
Observations	5,751	5,793	5,776	5,687	5,675
Panel B. Household fixed effects					
Rain on Survey Day	-13.020***	-3.171***	-3.662***	-0.129***	-0.123***
	(3.284)	(0.646)	(0.704)	(0.030)	(0.028)
Control mean	33.131	5.110	7.381	0.309	0.409
Observations	5,507	5,561	5,528	5,410	5,389
Panel C. Household and round fixed effects					
Rain on Survey Day	-1.136	-0.240	0.447	0.021	0.062**
	(3.336)	(0.634)	(0.680)	(0.031)	(0.028)
Control mean	33.131	5.110	7.381	0.309	0.409
Observations	5,507	5,561	5,528	5,410	5,389

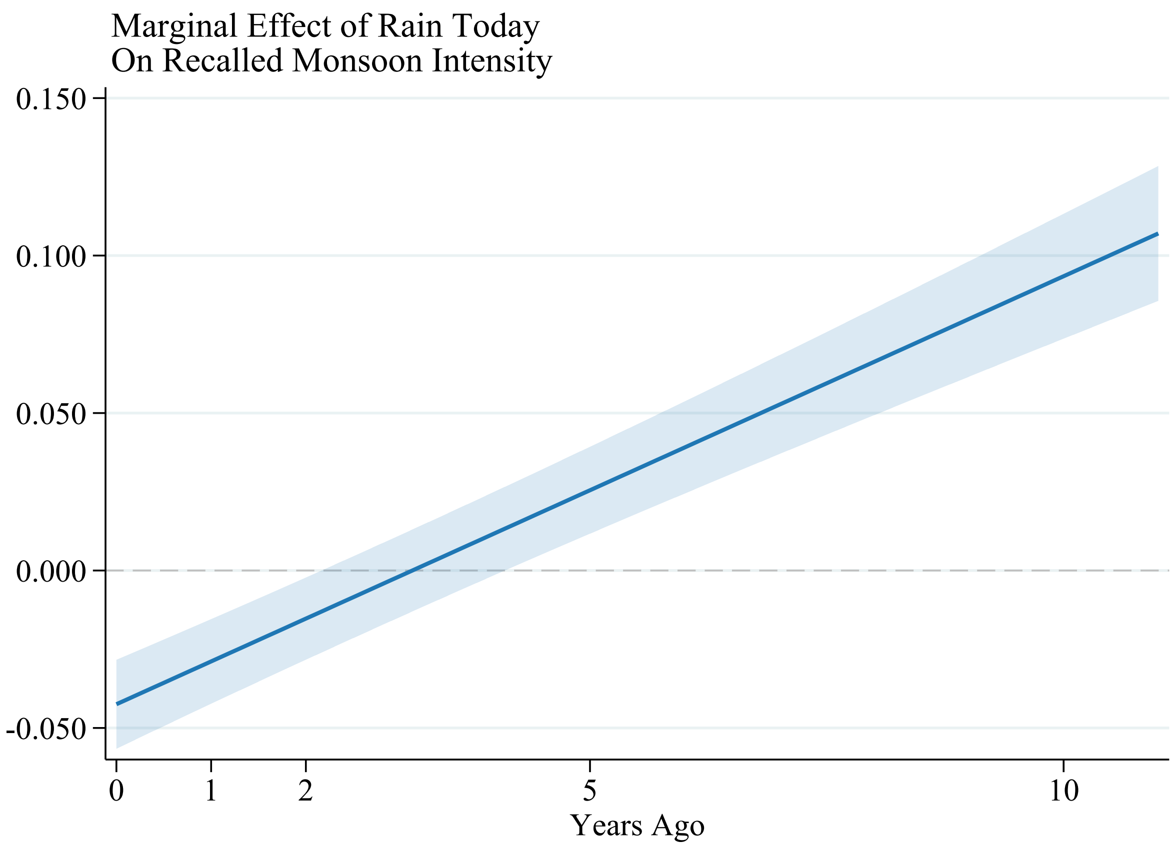
Note: Table A.16 examines whether rainfall on the day of the survey affects respondents' expectations about future floods and their willingness to pay for flood insurance. Column (1) shows impacts on willingness to pay for index insurance, elicited via a Becker-DeGroot-Marschak mechanism, and winsorized at 200 BDT. Column (2) shows impacts on the expected number of flood days in the next year. Column (3) shows impacts on the expected number of flood days over the next five years. Column (4) shows impacts on the expected share of crops damaged by flooding in the next year. Column (5) shows impacts on the expected share of crops damaged by flooding over the next five years. Rainfall on the day of the survey is recorded by the enumerator conducting the interview. Panel A reports estimates without fixed effects. Panel B includes household fixed effects. Panel C includes both household and round fixed effects. All standard errors are clustered at the village level, and p-values are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A.17: Beliefs about Change in Rainfall Over Time

	Past	Future
Low-rain years more common	0.964	0.903
High-rain years more common	0.031	0.069
Rain less predictable	0.201	0.225
Rain drops smaller	0.209	0.189
Rain drops bigger	0.039	0.039
Rain intensity increased	0.029	0.033
Rain intensity decreased	0.126	0.122
N	2,007	1,936

Note: Table A.17 reports the share of respondents selecting each description of how monsoon rainfall has changed over the past decade and how they expect it to change over the next decade. Respondents could select multiple options. The past question asks respondents who reported that rainfall has changed over the past 10 years to describe how it has changed, and the future question asks respondents who reported that rainfall will change over the next 10 years to describe how it will change. The sample excludes respondents who failed survey comprehension checks.

Figure A.3: The Impact of Contemporaneous Rainfall on Monsoon Memories by Recall Period



Note: Figure A.3 plots the marginal effect of rainfall on the day of the survey on recalled monsoon intensity as a function of how many years ago the respondent is asked to remember. The estimates are derived from a linear interaction between rain on the survey day and the temporal distance of the recalled period, pooling memories of monsoon intensity from 0, 1, 2, 5, or 10 years ago (randomly assigned). The regression includes farmer, village-by-round, and question order fixed effects, with standard errors clustered at the village level. The solid line shows the estimated marginal effect at each value of years ago, and the shaded region shows the 95% confidence interval.

Table A.18: The Impact of Contemporaneous Rainfall on Monsoon Forecasts

	(1)	(2)	(3)	(4)
Panel A. Past asked before future vs. future asked first				
Rain on Survey Day	-0.042***	-0.036***	-0.016	0.001
	(0.008)	(0.013)	(0.012)	(0.010)
Rain × Past Before Future	0.012	0.001	0.008	-0.002
	(0.008)	(0.012)	(0.012)	(0.006)
Control mean	0.687	0.688	0.688	0.688
Observations	3,876	3,308	3,308	3,232
Panel B. Past gap interaction (sample: past asked before future)				
Rain on Survey Day	-0.028***	-0.040**	-0.006	0.021
	(0.010)	(0.019)	(0.017)	(0.019)
Rain × Past Memory Gap (Years)	-0.000	0.001	0.000	-0.002
	(0.001)	(0.003)	(0.002)	(0.002)
Control mean	0.691	0.695	0.695	0.695
Observations	2,578	1,502	1,502	1,196
Farmer FE		✓	✓	✓
Round FE			✓	
Village × Round FE				✓

Note: Table A.18 tests whether rainfall on the day of the survey affects respondents' predictions about future monsoon intensity differentially based on being asked about past rainfall memories that were longer ago. The dependent variable in all columns is the respondent's predicted future monsoon intensity, elicited by asking farmers to distribute 10 buttons across bins representing the number of rainy days per two-week period during the monsoon season. In rounds 1 and 2, this question asks about rainfall 10 years in the future; in round 3, it asks about next year. Panel A examines whether being asked about past rainfall before making the future prediction amplifies the effect of rain today on future forecasts. "Rain on Survey Day" captures the baseline effect of contemporaneous rainfall on future predictions among respondents asked about the future first, as reported by the enumerator. "Rain×Past Before Future" captures the additional effect for respondents who answered at least one past rainfall question (either about recent or distant past monsoon intensity) before making their future prediction. The control mean reports the average future prediction among respondents who were not exposed to rain on the survey day and were asked about the future before any past rainfall question. Panel B restricts the sample to respondents who were asked about past rainfall before the future prediction and tests whether the spillover from rain-inflated past memories to future predictions grows with the temporal distance of the preceding past question. "Rain×Past Memory Gap (Years)" interacts rain on the survey day with the number of years ago that the past rainfall question preceding the future question asked about, which varies from 0 (this year's monsoon) to 10 (monsoon intensity a decade ago) based on both the randomized question order and the randomized past reference period. Column (1) includes no fixed effects. Column (2) adds farmer fixed effects. Column (3) adds farmer and round fixed effects. Column (4) includes farmer and village-by-round fixed effects. All standard errors are clustered at the village level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

B Data Appendix

B.1 Sampling

Village Sampling The survey was conducted across 250 villages in the Khulna division of Bangladesh. I began by sampling unions—collections of a few villages that constitute the fourth administrative level in Bangladesh. From the 642 unions in the Global Administrative Areas (2018) data, I exclude 32 urban areas and then select nine unions from which I have government salinity station data, 37 unions with water stations from the Bangladesh Water Development Board, 48 unions that are also included in the Bangladesh Integrated Household Survey sampling frame, and 121 unions that are also included in the 2016-2017 Bangladesh Labor Force Participation Survey sampling frame. This yields 185 unique unions. Both the Bangladesh Integrated Household Survey and the Bangladesh Labor Force Participation Survey were designed to be representative, and just 29 of the 185 initially selected unions fall outside of both of those survey's sampling frames as exclusively part of the government salinity or water stations. Then, I randomly sort the remaining unions, and choose the next 65.

The original sampled list included 250 unions. After enumerators attempted to conduct the listing exercise, they could not find a sufficient number of Boro rice farmers in either Nalian Range or Satkhira Range, reducing the total sample to 248, which was the pre-registered sample size. During the course of the baseline survey, it was discovered that in two additional unions, farmers had a different interpretation of the term "Boro" and did not harvest rice during the relevant season, reducing the sample size to 246. After additional funding was received partway through data collection, however, four new replacement unions were added to bring the total back to 250, following the initial randomization order. After collecting GPS coordinates during the baseline survey, the household locations did not always align in the boundary of the polygon for that union from GADM—this is to be expected as the exact definition of the union can change or be considered differently in the local context than in the official documentation used by GADM, as well as inevitable noise in both the records and GPS coordinates recorded by the enumerators. This discrepancy occurred in 216 cases. In 19 of the 250 villages, more than 50 percent of the household coordinates did not fall in the shapefile boundary. Discrepancies in the GPS coordinates for the randomly selected plot of each farmer occurred in 237 cases—as expected given that plots spread much farther than homes in a village, this rate is higher. The number of villages for which more than 50 percent of plot coordinates fall outside the polygon boundary is also 19. In these cases, I calculate the modal union based on the baseline data, and when relevant for merging in other environmental data, use that alternative polygon boundary.

Farmer Sampling Enumerators visited each union and did an initial listing of 50 households who were planning on harvesting rice during the upcoming Boro season and made the primary agricultural decisions on their land. In almost all unions, this goal of 50 households was achieved and typically within a single village. From this initial list, farmers were randomly ordered to be selected for an interview. Initially, 10 households were selected per union, though this number was revised down to nine given survey length concerns after the first week. On average, 9.1 farmers were surveyed in each union. Before a household was deemed unavailable and a replacement household was selected from the randomized listing order, enumerators attempted to contact them multiple times over multiple days via their phone number collected during the listing.

Timeline Data collection took place in three main survey rounds: Fall 2022, Summer 2023,and Winter 2024/25. Villages were assigned to enumerators based on location to minimize staff travel time. Within enumerator, the order of villages was randomized for all three survey rounds. Enumerators typically completed three surveys a day, spending three days in each village.

B.2 Survey Questions

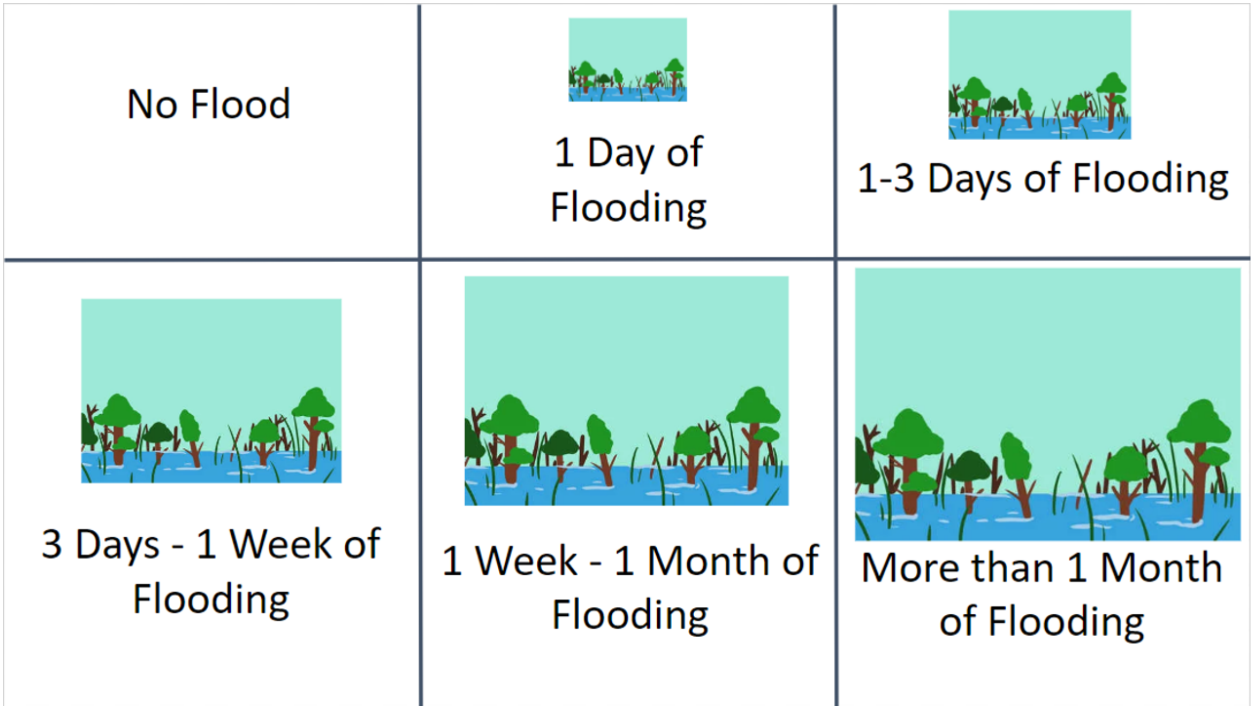
This section presents the full-text of the key questions used in this paper and describes the associated variable construction. The full survey instruments in English and Bangla can be found on my website.

Flooding To capture flood memories, I ask the following questions:

- I would now like to ask you some questions about the flood. By flood I mean unexpected and unwanted water enters your land or house and covers the ground. I only want you to think of a flood happening to you if it covers at least half of one of your plots, or the water is touching your house. Do you understand this definition of flood?
  - Yes
  - No
- Have you experienced flooding on your plots or at your house before?
  - Yes
  - No
- *[Respondents are randomly asked one of the following:]*
- I will now ask you about each flood. We'll start with the most recent flood you remember.
- I will now ask you about each flood. Let's start with the oldest flood you remember.
- *[Begin flood memory loop]*
- I'm now going to ask you about flood [NUMBER]
- When did this flood occur?
  - 0 years ago
  - 1 year ago
  - 2 years ago
  - 3 years ago
  - 4 years ago
  - 5 years ago
  - 6 years ago
  - 7 years ago
  - 8 or more years ago
- *[If 8 or more years ago:]* Year Instructions: Enter 99 if respondent doesn't know.

- *[Only asked in wave 1 and if flooding was 0 years ago:]* Was this flooding part of Cyclone Sitrang?
  - Yes
  - No
  - Don't know
- *[Only asked if flooding was 0 years ago:]* You said that a flood occurred on your plots or at your house this year. Which month?
  - April to May
  - May to June
  - June to July
  - July to August
  - August to September
  - September to October
  - October to November
  - November to December
  - December to January
  - January to February
  - February to March
  - March to April
  - Don't Know
- How long did this flood last?
  - one day of flood
  - 1-3 days of flood
  - 3 days - 1 week of flood
  - 1 week to 1 month of flood
  - more than 1 month of flood
  - Don't know
- Did that flood harm your crops?
  - Yes
  - No
  - Don't know
- How much of your harvest was damaged because of the flood?
  - None
  - A quarter
  - A half
  - Three quarters
  - All
  - Don't Know
- Was your house damaged because of that flood?
  - Yes
  - No
  - Don't know
- How much damage was done to your house? Try to estimate the amount in terms of how much Taka it would cost to fix. Instructions: Enter 9999 if the respondent doesn't know.

Figure B.1: Flood Belief Elicitation Visual Tool



Note: Figure B.1 shows an English translation of the image used to elicit beliefs about flooding, upon which farmers allocated 10 buttons to express their expectations.

- Can you remember another flood?
  - Yes
  - No
  - Don't know
- *[If K15 is "Yes", loop repeats.]*

To capture farmer expectations about flood risk, I ask the following questions, asking farmers to place buttons on Figure B.1.

- Instructions: Get flood photo
- I'm now going to ask you about flooding in the next 12 months. These pictures represent different amounts of flooding that might have occurred. The first picture shows no flooding that occurred on your land or plots. The second shows flooding that lasts for no more than one day. The third shows flooding that lasts more than one day but not more than three days. The fourth shows flooding that lasts more than three days but less than a week. The fifth shows flooding that lasts for more than a week but less than a month. The sixth shows flooding that lasts more than a month. If you think multiple floods happen, then you should add up the total number of days of flooding. For instance, if you think there will be one flood for three days and one flood for a week, then you should say that there was flooding for more than a week. Do you have any questions about these pictures?
  - Yes
  - No

- Don't know

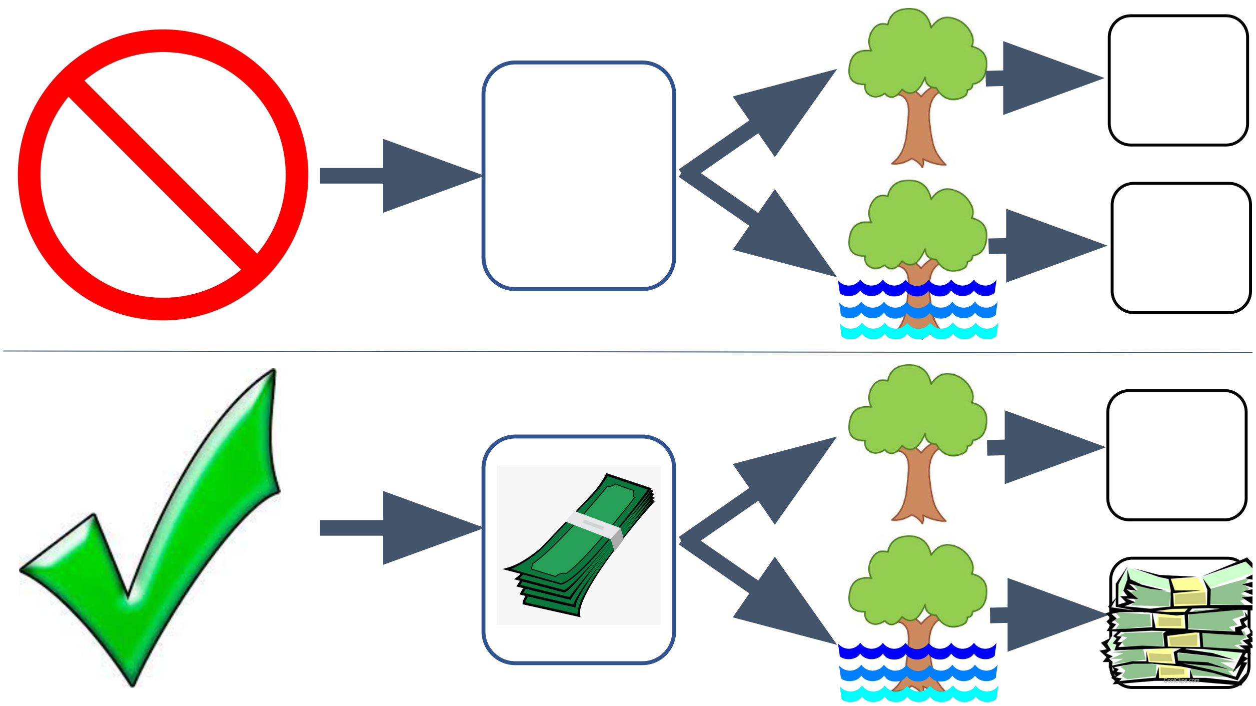
- I will now ask you some questions about next year's floods. I would like to know your best estimate of how much flooding is likely to occur in the next 12 months. Place the buttons on this image according to your guess. If the flood conditions shown in an image closely match your guess, you can place more buttons on that image. Do you understand the process?
  - Yes
  - No
  - Don't know
- Place the buttons on the picture according to your guess.
  - Count the number of buttons on No flood
  - Count the number of buttons on 1-3 days of flood
  - Count the number of buttons on 3 days - 1 week of flood
  - Count the number of buttons on 1 week to 1 month of flood
  - Count the number of buttons on more than 1 month of flood
- Instructions: Enter 99 if respondent says they do not know. If they are uncertain about the answer, tell them to put buttons on multiple options to express this uncertainty.
- *[If does not sum to 10:]* Warning: The number of buttons did not add up to 10. Please go back and check.
- *[If equals 99:]* Warning: Are you sure the respondent doesn't understand the question? If they simply do not know the answer, then they should put buttons on everything that they think might be a possibility, even if they do not know for sure.
- Now, instead of just thinking about the next 12 months, you think about all of the flooding that might occur in the next five years. Think about the type of flooding your land might experience during that entire time. Please place buttons on these images, placing more buttons on the image that most closely matches how many total days of flooding you think your land will experience in the next five years.
  - Count the number of buttons on No flood
  - Count the number of buttons on 1-3 days of flood
  - Count the number of buttons on 3 days - 1 week of flood
  - Count the number of buttons on 1 week to 1 month of flood
  - Count the number of buttons on more than 1 month of flood
- Instructions: Enter 99 if respondent says they do not know. If they are uncertain about the answer, tell them to put buttons on multiple options to express this uncertainty.
- *[If does not sum to 10:]* Warning: The number of buttons did not add up to 10. Please go back and check.
- *[If equals 99:]* Warning: Are you sure the respondent doesn't understand the question? If they simply do not know the answer, then they should put buttons on everything that they think might be a possibility, even if they do not know for sure.

To measure expected flood damages, I use the following questions.

- I would like to know your estimate of the amount of damage in case of flood. Think about the flood situation in each of these pictures. I will ask you about each situation. For each scenario, I would like to know what you think the average amount of damage to your crops and property would be if this amount of flooding occurred. Do you have any questions about this?

  - Yes
  - No
  - Don't know
- How much of your harvest do you think would be damaged from a flood that lasts less than one day?
  - None
  - A quarter
  - A half
  - Three quarters
  - All
  - Don't know
- What is the average amount of damage to your home that you think might happen from a flood that lasts less than one day? Instructions: Enter "99" if the respondent says they do not know.
- How much of your harvest do you think would be damaged from a flood that lasts one to three days?
  - None
  - A quarter
  - A half
  - Three quarters
  - All
  - Don't know
- What is the average amount of damage to your home that you think might happen from a flood that lasts one to three days? Instructions: Enter "99" if the respondent says they do not know.
- How much of your harvest do you think would be damaged from a flood that lasts three days to one week?
  - None
  - A quarter
  - A half
  - Three quarters
  - All
  - Don't know
- What is the average amount of damage to your home that you think might happen from a flood that lasts three days to one week? Instructions: Enter "99" if the respondent says they do not know.
- How much of your harvest do you think would be damaged from a flood that lasts more than one week but less than one month?
  - None
  - A quarter
  - A half
  - Three quarters
  - All
  - Don't know
- What is the average amount of damage to your home that you think might happen

Figure B.2: Flood Insurance Visual Tool



Note: Figure B.2 shows the image used to explain the flood insurance contract as part of the survey.

from a flood that lasts more than one week but less than one month? Instructions: Enter "99" if the respondent says they do not know.

- How much of your harvest do you think would be damaged from a flood that lasts more than one month?
  - None
  - A quarter
  - A half
  - Three quarters
  - All
  - Don't know
- What is the average amount of damage to your home that you think might happen from a flood that lasts more than one month? Instructions: Enter "99" if the respondent says they do not know.
  - None
  - A quarter
  - A half
  - Three quarters
  - All
  - Don't know

For the non-incentivized insurance willingness-to-pay elicitation in waves one and two, I use the following script, using the image in Figure B.2 as a visual aid.

- In some places, there are insurance contracts for bad weather or natural disasters. For example, there may be an insurance contract for the amount of rain. In that case, farmers like you will pay some money to the insurance company at the beginning of the season. If there is not enough rain in that season, the insurance company will refund the money paid to the farmers at the beginning of the season with interest. And if there is enough rain in the season, then the insurance company will not pay any money at the end of the season. Have you ever heard of any such insurance company or contract before?
  - Yes
  - No
  - Don't Know
- I'm now going to ask you about a hypothetical insurance contract. Suppose there is an insurance company offering insurance for flooding. You should imagine that the insurance company is extremely trustworthy.If you accept the contract, that means that every month, you would have to pay a fixed amount to that company. If there is a flood that occurs on your land, then they will pay you a large amount of money. If there is no flood, then you do not receive any money. Does this make sense?
  - Yes
  - No
- Instructions: Turn to the flood insurance picture
- *[Respondents are randomly assigned with equal probability to an insurance payout and an insurance fee. The potential payouts are: 10,000; 15,000; 20,000; 25,000; and 30,000. This value is denoted by "Payout" below. The potential fees are: 20; 30; 40; 50; and 60. This value is denoted by "Fee" below.]*
- Instructions: Point to the relevant part of the picture as you explain each part. Now let's see an example. The insurance company offers you a contract for [Fee] Taka per month. If there is a flood, then the company pays you [Payout] Taka. Let's walk through two scenarios. First, imagine that you do not buy the contract. That is shown by the top row. Then each month, you do not have to pay anything to any company. If no flood occurs, then you never receive any payment from the company and you never have to pay anything. However, if there is a flood, then that might damage your house or your crops. In that case, you would also not receive anything from the company, but you might have damages that might cost you some money. Now, imagine that you do buy the contract. That is shown by the bottom row. Then each month, you have to pay [Fee] taka. If there is no flood, then you do not get paid anything by the company. If there is a flood, it might damage your house or your crops. But the company also pays you [Payout] Taka. Of course, it is hard to know in advance if or when a flood will occur.
- Just to make sure this is clear, I'm going to ask you some questions about these scenarios. If you do not buy the insurance, how much do you get paid if there is a flood?
- *[If greater than 0:]* Actually, you would not be paid anything if you did not buy the insurance. Instructions: Explain again.
- If you do buy the insurance, how much do you get paid if there is a flood?
- *[If does not equal Payout:]* Actually, you would be paid [Payout]. Instructions: Explain

again.

- It is important to remember that it is hard to predict if and when a flood might occur. That means that if you buy insurance, the number of months you have to pay the fee before a flood occurs could be small, could be large, or a flood might never occur. Does that make sense?
  - Yes
  - No
- Instructions: Please answer any of the respondent's questions about how the insurance works.
- Now, imagine that the insurance company pays [Payout] taka in case there is a flood. I am going to ask you if you would buy insurance for different prices. First, I will ask if you would buy this insurance for 10 taka per month. This means that you have to pay 10 taka every month, but if there is a flood at some point, you will get [Payout] taka. Do you accept this contract?
  - Yes
  - No
- *[If "No":]* Now, imagine that the insurance costs 0 taka per month, so it is free. Do you accept this contract? Remember, if your plot gets flooded, you will be paid [Payout] taka if you have signed up for this free insurance.
  - Yes
  - No
- *[If 10 BDT answer is "Yes":]* Now, imagine that the insurance costs 20 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 30 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 40 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 50 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 60 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 70 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No

- *[If "Yes":]* Now, imagine that the insurance costs 80 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 90 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 100 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 110 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 120 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 130 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 140 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 150 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 160 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 170 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 180 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 190 taka per month, and you will be

paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* Now, imagine that the insurance costs 200 taka per month, and you will be paid [Payout] if and when a flood occurs. Do you accept this contract?
  - Yes
  - No
- *[If "Yes":]* What is the highest amount you would be willing to pay per month to accept this contract?

For the incentivized version, I first introduce BDM and do a practice with a pen. I explain insurance using the following script.

"We will now move on to the second item. It is a flood insurance program. You will have the chance to purchase this insurance. In this program, people who participate will be paid a flat payment if a flood occurs on their land in the next 12 months. To understand how this works, please look at this image. In the top are people who do not participate in the payment program. That means they do not have to pay the money that is the price of joining the program. For those people, in the next season, there may or may not be a flood. Whether or not there is a flood, they will not receive any payment because they did not participate in the program. Do you have any questions about what happens if you do not participate in the program? Answer any questions if yes."

"I will now discuss what happens if you do participate. If you look at the bottom, these are the people who chose to participate in the program, and so they do pay the price of joining the program. For those people, in the next season, they might experience a flood or they might not. If they do not experience a flood, then they will not receive any money from the program. In this case, they have paid the price of joining the program, but they will not receive any money back because there was no flood. If there is a flood, then these people will receive a flood payment from the program because they chose to participate and paid the fee to join. The amount of the payment is 10,000 Taka. The amount of money they receive does not depend on how much damage the flood does. They receive the same amount as long as a flood occurs. To claim the money, the participants just need to call this phone number and tell them that a flood occurred. Then the money will be sent to them through bKash to a number that they choose. The amount the participants receive if there is a flood is much more than the amount that they pay to participate in this program. Do you have any questions about what happens if you do participate in the program? Answer any questions if yes"

I then ask the same comprehension questions as above.

"I am now going to go through the same procedure as before for this flood payment program. I will say a price and ask whether you would like to pay that amount to participate in the flood payment program. If you say yes, I will ask you again at a slightly higher price. I will keep asking until you say you would not want to pay that price to participate in the program. As a reminder, if the tablet price is lower than a price that you say you would pay to participate in the program, then you will have to pay that amount. If you do, then I will give you a number that you can call if there is a flood to receive your payment. The program will end in the middle of October, and you can call any time between now and then when there is a flood. You will only be paid for one flood, so if you experience multiple floods over that period, you can only call once. If you pay to participate in the program, then I will also collect a phone number from you that can be used to send you the 10,000 Taka payment if there is a flood on your land. If there is no flood, then you will not receive any payment, and you will not get back the amount that you paid to participate in the program. You will only get money if there is a flood and then you call the number I will give you, and the amount you are paid will not depend on how much damage the flood does to your land. Like with the other goods, you do not have to participate if you do not want to, and you can just say no that you do not want to pay the price that I read to you. If you say no, then you will not have to pay that price. Do you have any questions about this procedure? Answer any questions if yes."

I then conduct the price list BDM elicitation.

Soil Salinity In the soil salinity survey experiment, enumerators asked two main questions in a random order:

1. *Past:* Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?
2. *Future:* Think about the next 10 years.Do you think the amount of salt in the soil in the typical plot in your village will increase, decrease, or stay the same from now until then?

To measure willingness-to-pay for salinity tolerant seeds, I conduct a BDM elicitation, mirroring the method above. I introduce the product as follows: "The last item is a 1 kg packet of BRRI 67, a saline resistant seed for the Boro season. This seed has been designed to grow well even if the soil has a lot of salt in it. If you receive this seed, then you can save it for the next Boro season."

Monsoon Intensity To measure farmers' perceptions of rainfall during the monsoon season, enumerators asked farmers to place buttons across the image in Figure B.3 for the random half of respondents asked about rainfall during the first survey, and across the image in Figure B.4 for the remaining survey rounds (shown in English here). This adjustment was made to address concerns about bottom-coded answers. Enumerators define a rainy day to farmers as one on which it rained for at least an hour with normal size drops. This is consistent with the U.S. Geological Survey definition. I ask farmers about how much it rained during the six months in the Bengali calendar corresponding to mid-May through mid-October. Farmers were asked to place buttons to indicate, for every two weeks during this period, on how many days they expect or recall that it rained, depending on whether the question was about the past or the future.

To convert these responses into a prediction, I assign values to buckets as follows: less than 8 days (7.5 days), less than 10 days (9.5 days), more than 12 days (12.5 days), and all bins k-$k+1$ days ($k + .5$ days).

The exact script is shown below.

- I'm now going to ask you about how much it rained during the monsoon season from the beginning of Jaistha [May/June in Bengali Calendar] to the end of Ashwin [September/October in Bengali Calendar]. It's okay if you don't know for certain, you should

Figure B.3: Rainfall Memory and Belief Elicitation Visual Tool—Survey Round 1

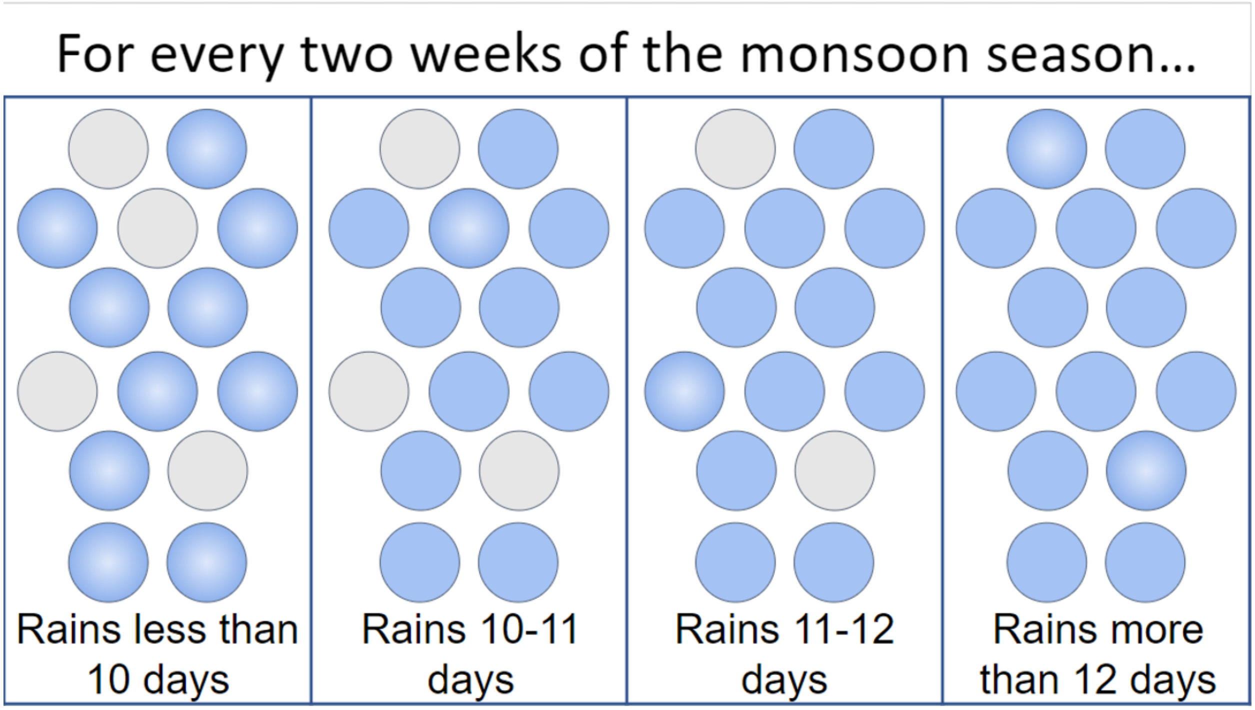
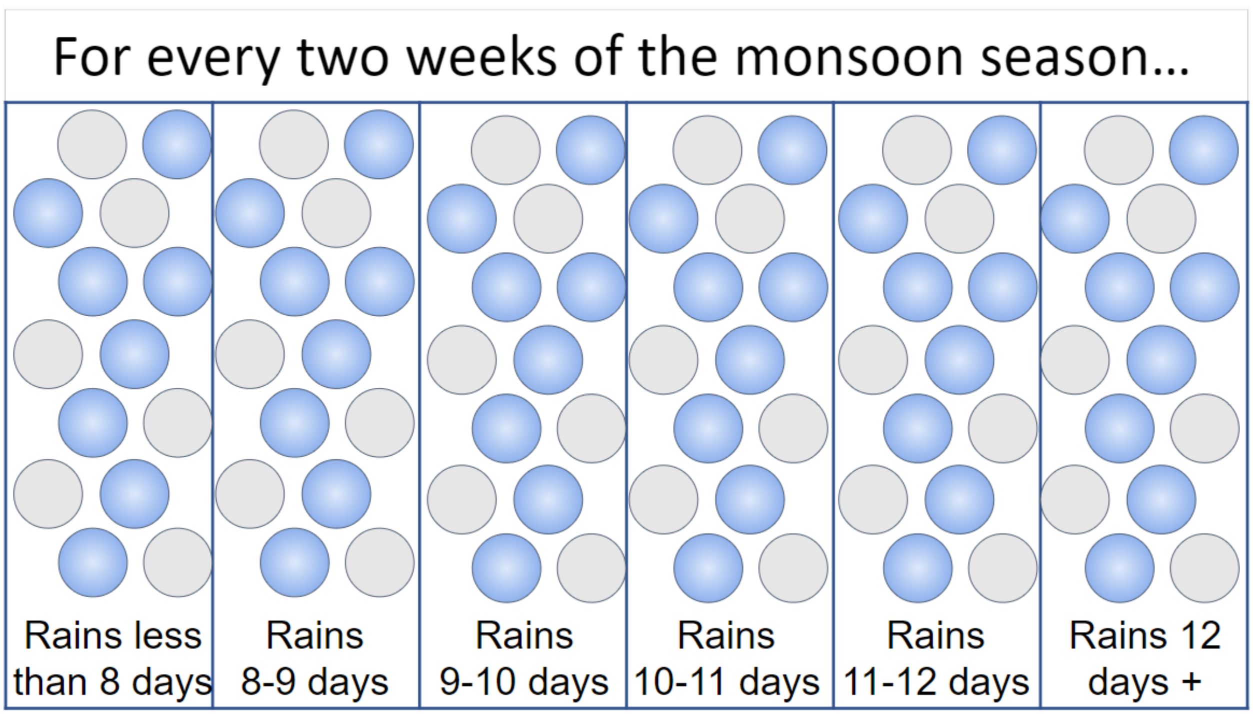


Figure B.4: Rainfall Memory and Belief Elicitation Visual Tool—Survey Round 2+



Note: Figures B.3 and B.4 show English translations of the images used to elicit beliefs about monsoon intensity. Respondents answering about their rainfall memories and beliefs during the first survey in October 2022 placed 10 buttons on Figure B.3; all remaining survey rounds used B.4.

just make your best guess. You can put buttons on multiple squares if you think that there are multiple possibilities. There are six months in this period. I'm going to ask you about how much it rained in this village. For this question, you should say that it rained on a day if it rained for at least an hour with normal size drops.

- I'm going to use this picture to help explain. The pictures show different scenarios about how much it rained in this period. In the first, it shows that on average, for every two weeks during this period, it rained on fewer than 8 days. In the second, it shows that on average, for every two weeks during this period, it rained between 8 and 9 days. In the third, it shows that on average, for every two weeks during this period, it rained between 9 and 10 days. In the fourth, it shows that on average, for every two weeks during this period, it rained between 10 and 11 days. In the fifth, it shows that on average, for every two weeks during this period, it rained between 11 and 12 days. In the sixth, it shows that on average, for every two weeks during this period, it rained on more than 12 of those days.
- Do you have any questions about each of these scenarios?
  - Yes
  - No
- Now, think about how much rain is necessary for your crops to grow without any problems. Please place the buttons below on the pictures that you think represent how much rain is necessary. Do not put buttons on the amount of rain that you think will hurt your crops.
  - Count the number of buttons on fewer than 8 days
  - Count the number of buttons on 8-9 days
  - Count the number of buttons on 9-10 days
  - Count the number of buttons on 10-11 days
  - Count the number of buttons on 11-12 days
  - Count the number of buttons on more than 12 days
- Instructions: Enter 99 if respondent says they do not know. If they are uncertain about the answer, tell them to put buttons on multiple options to express this uncertainty.
- *[If does not equal 10:]* Warning: The number of buttons did not add up to 10. Please go back and check.
- *[If equals 99:]* Warning: Are you sure the respondent doesn't understand the question? If they simply do not know the answer, then they should put buttons on everything that they think might be a possibility, even if they do not know for sure.

e following pattern:

- I'm now going to ask you to place buttons on the scenario that happened this past year, 2022. You may not remember exactly, so you should put more buttons on the scenario that you think is most likely to have happened. How much rain occurred in this village during this season in 2022 ?
  - Count the number of buttons on fewer than 8 days
  - Count the number of buttons on 8-9 days
  - Count the number of buttons on 9-10 days
  - Count the number of buttons on 10-11 days
  - Count the number of buttons on 11-12 days
  - Count the number of buttons on more than 12 days

- Instructions: Enter 99 if respondent says they do not know. If they are uncertain about the answer, tell them to put buttons on multiple options to express this uncertainty.
- *[If does not equal 10:]* Warning: The number of buttons did not add up to 10. Please go back and check.
- *[If equals 99:]* Warning: Are you sure the respondent doesn't understand the question? If they simply do not know the answer, then they should put buttons on everything that they think might be a possibility, even if they do not know for sure.
- Do you think that rainfall patterns of 10 years ago are different than they are today?
  - Yes
  - No
- *[If "Yes;;:]* How do you think the rainfall patterns have changed?
  - Years with low amounts of rain are more common today than 10 years ago
  - Years with high amounts of rain are more common today than 10 years ago
  - Rain is less predictable today
  - Raindrops are smaller today than 10 years ago
  - Raindrops are bigger today than 10 years ago
  - Rain intensity has increased (water per hour)
  - Rain intensity has decreased (water per hour)
- Do you think that rainfall patterns of today are different than they will be in 10 years?
  - Yes
  - No
- *[If "Yes":]* How do you think the rainfall patterns will change?
  - Years with low amounts of rain will become more common
  - Years with high amounts of rain will become more common
  - Rain will become less predictable
  - Rain drops will become smaller
  - Rain drops will become bigger
  - Rain intensity will increase (water per hour)
  - Rain intensity will decrease (water per hour)

C Model Appendix

This section presents the proofs of each prediction.

Prediction 1: Experience salience increases recall All else equal, greater event salience A_n increases probability the DM recalls that experience.

Proof:

Let

$$D(c) = \sum_{\ell=1}^{N} \kappa_\ell \exp\{\alpha S_c(m_\ell, c)\}.$$

Then

$$r_n(c) = \frac{\kappa_n \exp\{\alpha S_c(m_n, c)\}}{D(c)}.$$

Since $\kappa_n = K(A_n, W_n)$, differentiating $r_n(c)$ with respect to A_n gives

$$\frac{\partial r_n(c)}{\partial A_n} = \frac{K_A(A_n, W_n)\exp\{\alpha S_c(m_n, c)\}D(c) - \kappa_n \exp\{\alpha S_c(m_n, c)\}K_A(A_n, W_n)\exp\{\alpha S_c(m_n, c)\}}{D(c)^2}.$$

Factor out $K_A(A_n, W_n)\exp\{\alpha S_c(m_n, c)\}$:

$$\frac{\partial r_n(c)}{\partial A_n} = \frac{K_A(A_n, W_n)\exp\{\alpha S_c(m_n, c)\}\left[D(c) - \kappa_n \exp\{\alpha S_c(m_n, c)\}\right]}{D(c)^2}.$$

Using

$$r_n(c) = \frac{\kappa_n \exp\{\alpha S_c(m_n, c)\}}{D(c)},$$

we obtain

$$\frac{\partial r_n(c)}{\partial A_n} = \frac{K_A(A_n, W_n)}{\kappa_n} r_n(c)\{1 - r_n(c)\}.$$

Because

$$K_A(A_n, W_n) > 0, \qquad \kappa_n > 0, \qquad r_n(c) \in (0, 1),$$

we have

$$\frac{\partial r_n(c)}{\partial A_n} > 0.$$

Prediction 2: Salience increases feature-level storage All else equal, a more salient feature f in an experience n will be encoded and stored with greater likelihood.

Proof:

By construction,

$$\Pr(R_{nf} = 1 \mid \sigma_{nf}, Z_{nf}) = \rho(\sigma_{nf}, Z_{nf}).$$

The result follows directly from the assumption

$$\frac{\partial \rho(\sigma_{nf}, Z_{nf})}{\partial \sigma_{nf}} > 0.$$

Prediction 3: Contextual shocks increase recall of similar events, crowding out dissimilar All else equal, a context that makes experience i more similar to the moment of belief formation increases the probability the DM recalls that experience, crowding out less similar memories.

Proof:

The recall kernel implies

$$r_i(c) = \frac{\kappa_i \exp\{\alpha S_c(m_i, c)\}}{\sum_{\ell=1}^{N} \kappa_\ell \exp\{\alpha S_c(m_\ell, c)\}},$$

and

$$r_j(c) = \frac{\kappa_j \exp\{\alpha S_c(m_j, c)\}}{\sum_{\ell=1}^{N} \kappa_\ell \exp\{\alpha S_c(m_\ell, c)\}}.$$

Taking the ratio cancels the common denominator:

$$\frac{r_i(c)}{r_j(c)} = \frac{\kappa_i}{\kappa_j} \exp\{\alpha[S_c(m_i,c) - S_c(m_j,c)]\}.$$

Taking logs gives

$$\log \frac{r_i(c)}{r_j(c)} = \log \frac{\kappa_i}{\kappa_j} + \alpha\{S_c(m_i,c) - S_c(m_j,c)\}.$$

Now let

$$D(c) = \sum_{\ell=1}^{N} \kappa_\ell \exp\{\alpha S_c(m_\ell,c)\}.$$

Differentiating $r_i(c)$ with respect to $S_c(m_i,c)$ gives

$$\frac{\partial r_i(c)}{\partial S_c(m_i,c)} = \frac{\alpha\kappa_i \exp\{\alpha S_c(m_i,c)\} D(c) - \kappa_i \exp\{\alpha S_c(m_i,c)\}\alpha\kappa_i \exp\{\alpha S_c(m_i,c)\}}{D(c)^2}.$$

Thus

$$\frac{\partial r_i(c)}{\partial S_c(m_i,c)} = \alpha r_i(c)\{1 - r_i(c)\} > 0.$$

For $j \neq i$, the numerator of $r_j(c)$ does not depend on $S_c(m_i,c)$, while the denominator does. Therefore,

$$\frac{\partial r_j(c)}{\partial S_c(m_i,c)} = -\frac{\kappa_j \exp\{\alpha S_c(m_j,c)\}\alpha\kappa_i \exp\{\alpha S_c(m_i,c)\}}{D(c)^2} = -\alpha r_i(c) r_j(c) < 0.$$

Prediction 4: Constructive reconstruction moves memories toward the context All else equal, conditional on recalling an incomplete trace m_n, the reconstructed feasible completion $\tilde{e} \in \mathcal{C}(m_n)$ will be more similar to the context of belief formation than a completion drawn from the baseline reconstruction distribution.

Proof:

Fix trace m_n and context c. For each feasible completion $x \in \mathcal{C}(m_n)$, write

$$s_x \equiv S_c(x,c) \quad \text{and} \quad \nu_x \equiv \nu(x \mid m_n).$$

To make the dependence on the reconstruction parameter explicit, write

$$g_\tau(x \mid m_n, c) = \frac{\nu_x \exp\{\tau s_x\}}{\sum_{z\in\mathcal{C}(m_n)} \nu_z \exp\{\tau s_z\}}.$$

Define the expected similarity between the reconstructed completion and the context by

$$\Phi(\tau) \equiv \sum_{x\in\mathcal{C}(m_n)} g_\tau(x \mid m_n, c) s_x.$$

When $\tau = 0$, reconstruction follows the baseline distribution:

$$g_0(x \mid m_n, c) = \nu_x.$$

Hence

$$\Phi(0) = \sum_{x \in \mathcal{C}(m_n)} \nu_x s_x = \mathbb{E}_{\nu(\cdot|m_n)} \left[S_c(\tilde{e}, c) \right].$$

For $\tau > 0$,

$$\Phi(\tau) = \mathbb{E}_{g_\tau(\cdot|m_n,c)} \left[S_c(\tilde{e}, c) \right].$$

Now differentiate $g_\tau(x \mid m_n, c)$ with respect to τ. Since

$$g_\tau(x \mid m_n, c) = \frac{\nu_x e^{\tau s_x}}{\sum_{z \in \mathcal{C}(m_n)} \nu_z e^{\tau s_z}},$$

we have

$$\frac{\partial g_\tau(x \mid m_n, c)}{\partial \tau} = g_\tau(x \mid m_n, c) \left[s_x - \sum_{z \in \mathcal{C}(m_n)} g_\tau(z \mid m_n, c) s_z \right].$$

Using the definition of $\Phi(\tau)$, this becomes

$$\frac{\partial g_\tau(x \mid m_n, c)}{\partial \tau} = g_\tau(x \mid m_n, c) \left[s_x - \Phi(\tau) \right].$$

Therefore,

$$\Phi'(\tau) = \sum_{x \in \mathcal{C}(m_n)} \frac{\partial g_\tau(x \mid m_n, c)}{\partial \tau} s_x.$$

Substituting the previous expression gives

$$\Phi'(\tau) = \sum_{x \in \mathcal{C}(m_n)} g_\tau(x \mid m_n, c) \left[s_x - \Phi(\tau) \right] s_x.$$

Add and subtract $\Phi(\tau)$ inside the final factor:

$$\Phi'(\tau) = \sum_{x \in \mathcal{C}(m_n)} g_\tau(x \mid m_n, c) \left[s_x - \Phi(\tau) \right] \left[s_x - \Phi(\tau) \right].$$

Thus

$$\Phi'(\tau) = \sum_{x \in \mathcal{C}(m_n)} g_\tau(x \mid m_n, c) \left[s_x - \Phi(\tau) \right]^2 = \mathrm{Var}_{g_\tau(\cdot|m_n,c)} \left(S_c(\tilde{e}, c) \right) \geq 0.$$

Hence $\Phi(\tau) \geq \Phi(0)$ for all $\tau > 0$. The inequality is strict whenever $S_c(x, c)$ varies over $x \in \mathcal{C}(m_n)$ on the support of $\nu(\cdot \mid m_n)$.

Prediction 5: Weaker storage amplifies contextual reconstruction All else equal, conditional on recalling trace m_n, context has a larger effect on reported memory content when feature f is less likely to have been successfully stored.

Proof: Let

$$p_{nf} \equiv \Pr(R_{nf} = 1) = \rho(\sigma_{nf}, Z_{nf}),$$

and let

$$\Pi^a_{nf}(c) \equiv \Pr(\tilde{e}_f = a \mid R_{nf} = 0, n \text{ recalled}, c)$$

denote the probability that reconstruction assigns value $a \in \{0, 1\}$ to feature f when the feature was not retained.

If the DM retained feature f, then the reported value equals the true value:

$$\Pr(Y_{nf}(c) = a \mid R_{nf} = 1, n \text{ recalled}) = \mathbb{1}\{e_{nf} = a\}.$$

If the DM did not retain feature f, then reconstruction assigns value a with probability

$$\Pi^a_{nf}(c).$$

By the law of total probability,

$$\Pr(Y_{nf}(c) = a \mid n \text{ recalled}) = p_{nf}\mathbb{1}\{e_{nf} = a\} + (1 - p_{nf})\Pi^a_{nf}(c).$$

Applying this expression under context c' gives

$$\Pr(Y_{nf}(c') = a \mid n \text{ recalled}) = p_{nf}\mathbb{1}\{e_{nf} = a\} + (1 - p_{nf})\Pi^a_{nf}(c').$$

Subtracting the expression under context c from the expression under context c' yields

$$\Pr(Y_{nf}(c') = a \mid n \text{ recalled}) - \Pr(Y_{nf}(c) = a \mid n \text{ recalled}) = (1 - p_{nf})\{\Pi^a_{nf}(c') - \Pi^a_{nf}(c)\}.$$

The multiplier $1 - p_{nf}$ increases as storage strength p_{nf} decreases.

Prediction 6: Memory changes can shift beliefs All else equal, context-induced changes in recall and reconstruction shift beliefs whenever they change the memory-based support for the hypothesis H.

Proof:

From the belief equation,

$$\widehat{\theta}(c) = \gamma S_c(c, H) + (1 - \gamma)\sum_{n=1}^{N} r_n(c) \sum_{x \in \mathcal{C}(m_n)} g(x \mid m_n, c) S_c(x, H).$$

Define the memory component of beliefs by

$$M(c) \equiv \sum_{n=1}^{N} r_n(c) \sum_{x \in \mathcal{C}(m_n)} g(x \mid m_n, c) S_c(x, H).$$

Using the definition of $M(c)$, this becomes

$$\widehat{\theta}(c) = \gamma S_c(c, H) + (1 - \gamma)M(c).$$

Likewise,

$$\widehat{\theta}(c') = \gamma S_{c'}(c', H) + (1 - \gamma)M(c').$$

Subtracting the expression for $\widehat{\theta}(c)$ from the expression for $\widehat{\theta}(c')$ gives

$$\widehat{\theta}(c') - \widehat{\theta}(c) = \gamma\{S_{c'}(c', H) - S_c(c, H)\} + (1 - \gamma)\{M(c') - M(c)\}.$$

If

$$S_{c'}(c', H) = S_c(c, H),$$

then the direct context term equals zero, so

$$\widehat{\theta}(c') - \widehat{\theta}(c) = (1 - \gamma)\{M(c') - M(c)\}.$$

When $\gamma < 1$ and $M(c') > M(c)$, the right-hand side is strictly positive. Therefore,

$$\widehat{\theta}(c') > \widehat{\theta}(c).$$

Prediction 7: Memory can shape decision-making Memory can shape decision-making when DMs are sufficiently close to their decision thresholds.

Proof: For each DM i,

$$d_i^1 - d_i^0 = \mathbb{1}\{\widehat{\theta}_i^1 \geq \theta_i^\star\} - \mathbb{1}\{\widehat{\theta}_i^0 \geq \theta_i^\star\}.$$

This difference equals 1 exactly when

$$\widehat{\theta}_i^0 < \theta_i^\star \leq \widehat{\theta}_i^1.$$

It equals -1 exactly when

$$\widehat{\theta}_i^1 < \theta_i^\star \leq \widehat{\theta}_i^0.$$

It equals zero otherwise. Therefore,

$$d_i^1 - d_i^0 = \mathbb{1}\{\widehat{\theta}_i^0 < \theta_i^\star \leq \widehat{\theta}_i^1\} - \mathbb{1}\{\widehat{\theta}_i^1 < \theta_i^\star \leq \widehat{\theta}_i^0\}.$$

Taking expectations gives

$$\mathbb{E}[d_i^1 - d_i^0] = \Pr\{\widehat{\theta}_i^0 < \theta_i^\star \leq \widehat{\theta}_i^1\} - \Pr\{\widehat{\theta}_i^1 < \theta_i^\star \leq \widehat{\theta}_i^0\}.$$

If

$$\widehat{\theta}_i^1 \geq \widehat{\theta}_i^0 \quad \text{for all } i,$$

then the event

$$\{\widehat{\theta}_i^1 < \theta_i^\star \leq \widehat{\theta}_i^0\}$$

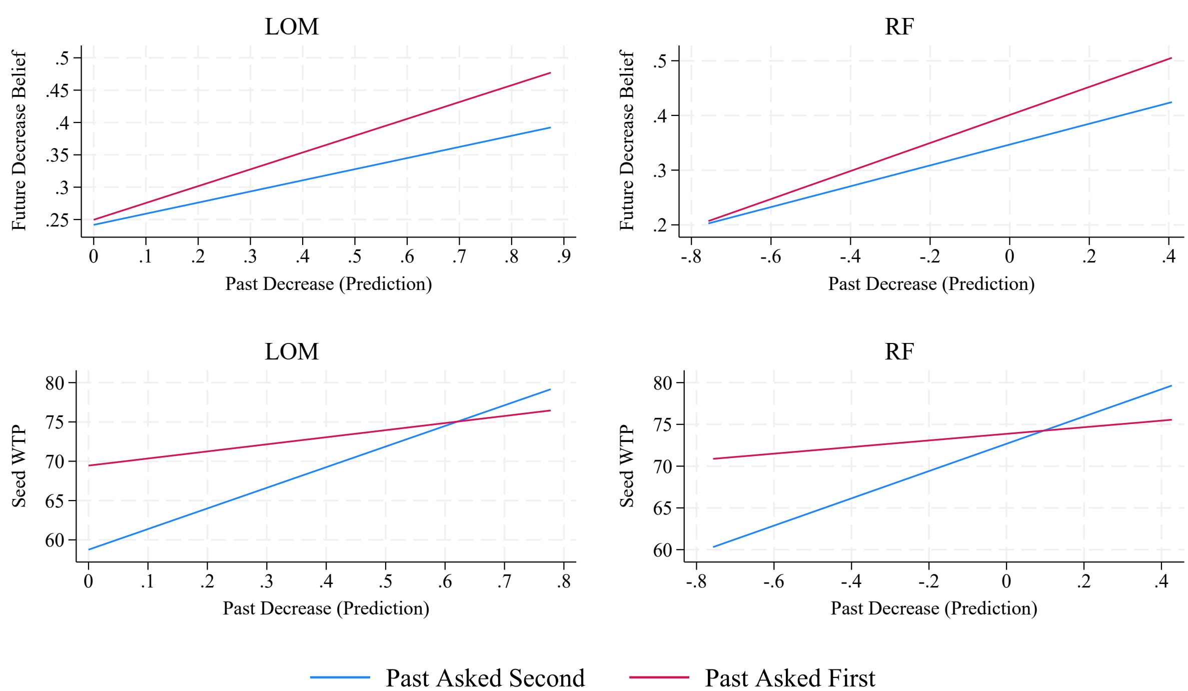
is empty. Hence

$$\mathbb{E}[d_i^1 - d_i^0] = \Pr\{\widehat{\theta}_i^0 < \theta_i^\star \leq \widehat{\theta}_i^1\}.$$

Aggregate behavior changes whenever this probability is strictly positive.

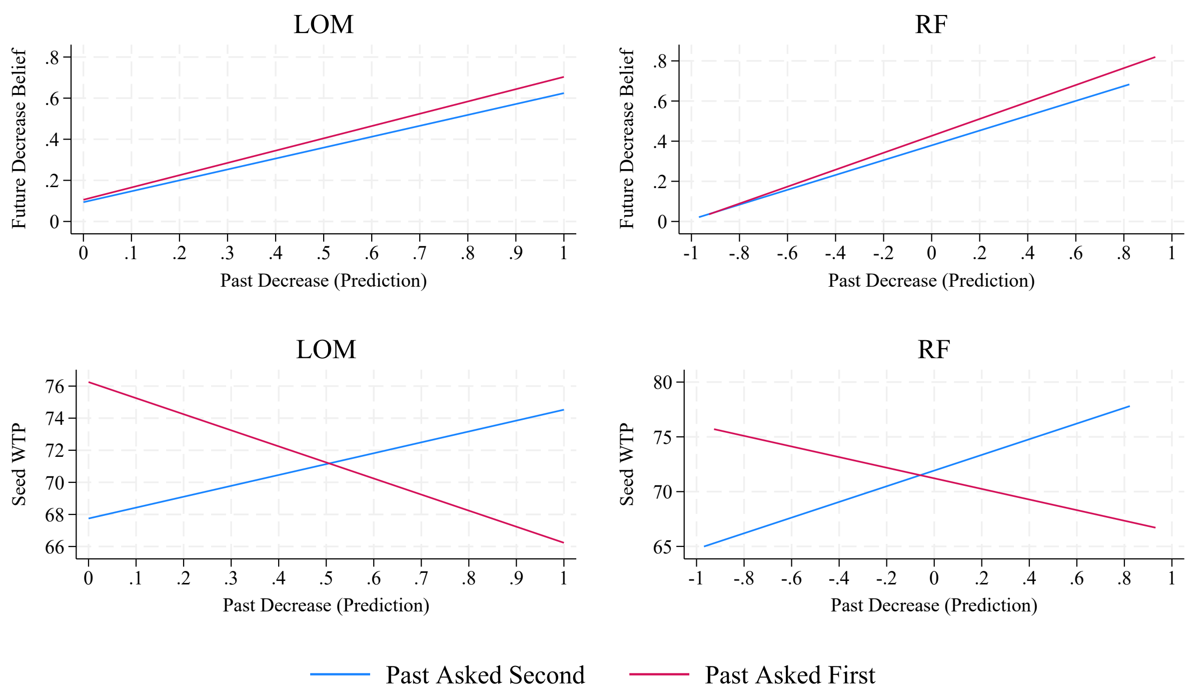
D Robustness Checks

Figure D.1: Salinity Question Order Experiment: Margins Plot



Note: Figure D.1 plots fitted values from regressions of respondents' beliefs about future village salinity decline and seed willingness-to-pay on predicted past salinity decline, separately by whether respondents first answered about about past salinity ("Past Asked First") or not ("Past Asked Second"). Predicted past decline is measured using either the village leave-out-mean or the random forest prediction. Each panel is based on regressions estimated on the sample excluding those who failed comprehension checks and includes union-by-round fixed effects and standard errors clustered at the household level. The lines show regression-adjusted predicted outcomes across the support of the prediction measure.

Figure D.2: Salinity Question Order Experiment: Raw Linear Fit



Note: Figure D.2 plots ordinary-least-squares lines of best fit on the raw data of respondents' beliefs about future village salinity decline and seed willingness-to-pay by predicted past salinity decline, separately by whether respondents first answered about about past salinity ("Past Asked First") or not ("Past Asked Second"). Predicted past decline is measured using either the village leave-out-mean or the random forest prediction. Each panel is based on regressions estimated on the sample excluding those who failed comprehension checks.

Table D.1: Salinity Question Order Experiment Results—Village Clustering

	No FEs			Village + Round FEs			HHID + Village × Round FEs		
	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)
Panel A. Outcome: Future village salinity decrease									
Asked Past First ×	0.067	0.034	0.053*	0.088**	0.053*	0.066**	0.066	0.032	0.042
Past Decrease	(0.046)	(0.029)	(0.029)	(0.043)	(0.028)	(0.027)	(0.042)	(0.029)	(0.029)
(Prediction)									
Observations	6,141	6,141	6,141	6,141	6,141	6,141	6,079	6,079	6,079
Unique farmers	2,266	2,266	2,266	2,266	2,266	2,266	2,204	2,204	2,204
Control mean	0.315	0.315	0.315	0.315	0.315	0.315	0.316	0.316	0.316
Panel B. Outcome: Seed WTP									
Asked Past First ×	-16.800*	-4.818	-11.996**	-17.222**	-7.782	-12.380**	-21.330**	-16.585**	-17.283**
Past Decrease	(8.541)	(5.185)	(5.581)	(8.331)	(4.965)	(5.197)	(9.660)	(6.391)	(7.460)
(Prediction)									
Observations	4,116	4,116	4,116	4,116	4,116	4,116	3,740	3,740	3,740
Unique farmers	2,246	2,246	2,246	2,246	2,246	2,246	1,870	1,870	1,870
Control mean	70.498	70.498	70.498	70.498	70.498	70.498	71.126	71.126	71.126

Note: Table D.1 presents the main interaction term of a difference-in-differences regression of being asked about past salinity trends first, the prediction of the respondent answering that in the past, salinity declined, and the interaction of those two. The prediction of a past decrease in the first, fourth, and seventh columns corresponds to the share of the respondents' neighbors in their village in that same survey wave who answered decrease (the leave-out-mean). In the second, fifth, and eighth columns, the prediction is a binarized version of that variable equal to one if the majority of neighbors answered a decrease and zero otherwise. In the third, sixth, and ninth columns, the prediction comes from a random forest model trained on the leave-out-mean, soil salinity on a randomly selected plot of the respondent as measured during the baseline survey, household size, gender, age, indicators for having a household member over 60 and a male household member over 60, years of schooling, indicators for different levels of schooling, indicator for having migrated in the past, log household earnings, total number of plots, and years cultivating Boro season rice. The first three columns do not include any fixed effects; the next three include village and survey round fixed effects; the last three include household and village-by-round fixed effects. All standard errors are clustered at the village level, and the sample excludes those who failed survey comprehension checks. Panel A shows impacts on whether the respondent predicted that salinity will decrease in the future. Panel B shows impacts on the Becker-DeGroot-Marschak elicitation of willingness-to-pay for a salinity tolerant seed. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" The future question is, "Think about the next 10 years. Do you think the amount of salt in the soil in the typical plot in your village will increase, decrease, or stay the same from now until then?" Standard errors are reported in parentheses, and p-values are denoted as follows: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table D.2: Salinity Question Order Experiment Results—Including Failed Comprehension Checks

	No FEs			Village + Round FEs			HHID + Village × Round FEs		
	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)	(LOM)	(BIN)	(RF)
Panel A. Outcome: Future village salinity decrease									
Asked Past First ×	0.056	0.029	0.041	0.067^{*}	0.042^{*}	0.050^{**}	0.051	0.018	0.032
Past Decrease	(0.038)	(0.025)	(0.025)	(0.037)	(0.024)	(0.024)	(0.040)	(0.028)	(0.029)
(Prediction)									
Observations	6,663	6,663	6,663	6,663	6,663	6,663	6,655	6,655	6,655
Unique farmers	2,279	2,279	2,279	2,279	2,279	2,279	2,271	2,271	2,271
Control mean	0.315	0.315	0.315	0.315	0.315	0.315	0.316	0.316	0.316
Panel B. Outcome: Seed WTP									
Asked Past First ×	-15.079^{*}	-3.939	-11.084^{**}	-15.929^{**}	-6.728	-11.445^{**}	-19.774^{**}	-12.862^{**}	-16.913^{***}
Past Decrease	(7.787)	(4.645)	(4.913)	(7.548)	(4.453)	(4.690)	(9.369)	(5.842)	(6.413)
(Prediction)									
Observations	4,532	4,532	4,532	4,532	4,532	4,532	4,506	4,506	4,506
Unique farmers	2,279	2,279	2,279	2,279	2,279	2,279	2,253	2,253	2,253
Control mean	70.549	70.549	70.549	70.549	70.549	70.549	70.592	70.592	70.592

Note: Table D.2 presents the main interaction term of a difference-in-differences regression of being asked about past salinity trends first, the prediction of the respondent answering that in the past, salinity declined, and the interaction of those two. The prediction of a past decrease in the first, fourth, and seventh columns corresponds to the share of the respondents' neighbors in their village in that same survey wave who answered decrease (the leave-out-mean). In the second, fifth, and eighth columns, the prediction is a binarized version of that variable equal to one if the majority of neighbors answered a decrease and zero otherwise. In the third, sixth, and ninth columns, the prediction comes from a random forest model trained on the leave-out-mean, soil salinity on a randomly selected plot of the respondent as measured during the baseline survey, household size, gender, age, indicators for having a household member over 60 and a male household member over 60, years of schooling, indicators for different levels of schooling, indicator for having migrated in the past, log household earnings, total number of plots, and years cultivating Boro season rice. The first three columns do not include any fixed effects; the next three include village and survey round fixed effects; the last three include household and village-by-round fixed effects. All standard errors are clustered at the household level, and the sample includes all respondents, including those who failed survey comprehension checks. Panel A shows impacts on whether the respondent predicted that salinity will decrease in the future. Panel B shows impacts on the Becker-DeGroot-Marschak elicitation of willingness-to-pay for a salinity tolerant seed. The past question is, "Think back to the past 10 years. Do you think the amount of salt in the soil in the typical plot in your village has increased, decreased, or stayed the same since then?" The future question is, "Think about the next 10 years. Do you think the amount of salt in the soil in the typical plot in your village will increase, decrease, or stay the same from now until then?" Standard errors are reported in parentheses, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table D.3: The Impact of Contemporaneous Rainfall on Number of Recalled Floods

	(1)	(2)	(3)	(4)	(5)	(6)
Rain on Survey Day	-0.145***	-0.181***	-0.175***	-0.145**	-0.181***	-0.175***
	(0.030)	(0.030)	(0.033)	(0.059)	(0.058)	(0.067)
Control mean	0.998	0.998	0.999	0.998	0.998	0.999
Observations	6,504	6,503	6,487	6,504	6,503	6,487
SE clustered at	Farmer	Farmer	Farmer	Village	Village	Village
Village FE	✓	✓		✓	✓	
Round FE	✓		✓	✓		✓
Survey date FE		✓			✓	
Farmer FE			✓			✓

Note: Table D.3 shows the impacts of rainfall on the day respondents are interviewed on the number of recalled floods that the respondent remembers during the memory elicitation. Rainfall on the day of the survey is measured by the enumerator conducting the survey. Columns (1), (2), and (3) cluster standard errors at the farmer level, and Columns (4), (5), and (6) cluster standard errors at the village level. Columns (1) and (4) include village and survey round fixed effects. Columns (2) and (5) include village and survey date fixed effects. Column (3) and (6) include survey round and farmer fixed effects. p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table D.4: The Impact of Contemporaneous Rainfall on Recalled Flood Year

	(1)	(2)	(3)	(4)	(5)	(6)
Rain on Survey Day	1.056**	1.125**	1.386***	1.056*	1.125**	1.386**
	(0.428)	(0.452)	(0.462)	(0.591)	(0.568)	(0.701)
Control mean	2006.763	2006.763	2006.593	2006.763	2006.763	2006.593
Observations	6,309	6,309	5,927	6,309	6,309	5,927
SE clustered at	Farmer	Farmer	Farmer	Village	Village	Village
Village FE	✓	✓		✓	✓	
Round FE	✓		✓	✓		✓
Survey date FE		✓			✓	
Farmer FE			✓			✓

Note: Table D.4 shows the impacts of rainfall on the day respondents are interviewed on flood year that the respondent remembers during the memory elicitation. Rainfall on the day of the survey is measured by the enumerator conducting the survey. Columns (1), (2), and (3) cluster standard errors at the farmer level, and Columns (4), (5), and (6) cluster standard errors at the village level. Columns (1) and (4) include village and survey round fixed effects. Columns (2) and (5) include village and survey date fixed effects. Column (3) and (6) include survey round and farmer fixed effects. p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table D.5: The Impact of Contemporaneous Rainfall on Recalled Flood Length

	(1)	(2)	(3)	(4)	(5)	(6)
Rain on Survey Day	2.749***	2.794***	3.250***	2.749***	2.794***	3.250***
	(0.352)	(0.367)	(0.387)	(0.573)	(0.514)	(0.645)
Control mean	17.485	17.485	17.526	17.485	17.485	17.526
Observations	6,327	6,327	5,948	6,327	6,327	5,948
SE clustered at	Farmer	Farmer	Farmer	Village	Village	Village
Village FE	✓	✓		✓	✓	
Round FE	✓		✓	✓		✓
Survey date FE		✓			✓	
Farmer FE			✓			✓

Note: Table D.5 shows the impacts of rainfall on the day respondents are interviewed on the length of floods that the respondent remembers during the memory elicitation. Rainfall on the day of the survey is measured by the enumerator conducting the survey. Columns (1), (2), and (3) cluster standard errors at the farmer level, and Columns (4), (5), and (6) cluster standard errors at the village level. Columns (1) and (4) include village and survey round fixed effects. Columns (2) and (5) include village and survey date fixed effects. Column (3) and (6) include survey round and farmer fixed effects. p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.6: The Impact of Contemporaneous Rainfall on Any Recalled Flood Crop Damage

	(1)	(2)	(3)	(4)	(5)	(6)
Rain on Survey Day	0.036***	0.038***	0.046***	0.036***	0.038***	0.046***
	(0.011)	(0.013)	(0.014)	(0.012)	(0.013)	(0.015)
Control mean	0.911	0.911	0.911	0.911	0.911	0.911
Observations	5,178	5,177	4,706	5,178	5,177	4,706
SE clustered at	Farmer	Farmer	Farmer	Village	Village	Village
Village FE	✓	✓		✓	✓	
Round FE	✓		✓	✓		✓
Survey date FE		✓			✓	
Farmer FE			✓			✓

Note: Table D.6 shows the impacts of rainfall on the day respondents are interviewed on an indicator for flooding damaging crops that the respondent remembers during the memory elicitation. Rainfall on the day of the survey is measured by the enumerator conducting the survey. Columns (1), (2), and (3) cluster standard errors at the farmer level, and Columns (4), (5), and (6) cluster standard errors at the village level. Columns (1) and (4) include village and survey round fixed effects. Columns (2) and (5) include village and survey date fixed effects. Column (3) and (6) include survey round and farmer fixed effects. p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.7: The Impact of Contemporaneous Rainfall on Share Recalled Flood Crop Damage

	(1)	(2)	(3)	(4)	(5)	(6)
Rain on Survey Day	0.050***	0.054***	0.063***	0.050***	0.054***	0.063***
	(0.014)	(0.015)	(0.017)	(0.018)	(0.018)	(0.022)
Control mean	0.755	0.755	0.756	0.755	0.755	0.756
Observations	5,165	5,164	4,691	5,165	5,164	4,691
SE clustered at	Farmer	Farmer	Farmer	Village	Village	Village
Village FE	✓	✓		✓	✓	
Round FE	✓		✓	✓		✓
Survey date FE		✓			✓	
Farmer FE			✓			✓

Note: Table D.7 shows the impacts of rainfall on the day respondents are interviewed on the share of crop damage per flood that the respondent remembers during the memory elicitation. Rainfall on the day of the survey is measured by the enumerator conducting the survey. Columns (1), (2), and (3) cluster standard errors at the farmer level, and Columns (4), (5), and (6) cluster standard errors at the village level. Columns (1) and (4) include village and survey round fixed effects. Columns (2) and (5) include village and survey date fixed effects. Column (3) and (6) include survey round and farmer fixed effects. p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.8: The Impact of Contemporaneous Rainfall on Any Recalled Flood Property Damage

	(1)	(2)	(3)	(4)	(5)	(6)
Rain on Survey Day	-0.011	-0.005	-0.022	-0.011	-0.005	-0.022
	(0.019)	(0.021)	(0.024)	(0.024)	(0.026)	(0.029)
Control mean	0.435	0.435	0.448	0.435	0.435	0.448
Observations	5,188	5,187	4,721	5,188	5,187	4,721
SE clustered at	Farmer	Farmer	Farmer	Village	Village	Village
Village FE	✓	✓		✓	✓	
Round FE	✓		✓	✓		✓
Survey date FE		✓			✓	
Farmer FE			✓			✓

Note: Table D.8 shows the impacts of rainfall on the day respondents are interviewed on an indicator for a flood damaging property that the respondent remembers during the memory elicitation. Rainfall on the day of the survey is measured by the enumerator conducting the survey. Columns (1), (2), and (3) cluster standard errors at the farmer level, and Columns (4), (5), and (6) cluster standard errors at the village level. Columns (1) and (4) include village and survey round fixed effects. Columns (2) and (5) include village and survey date fixed effects. Column (3) and (6) include survey round and farmer fixed effects. p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.9: The Impact of Contemporaneous Rainfall on Recalled Flood Property Damage

	(1)	(2)	(3)	(4)	(5)	(6)
Rain on Survey Day	-163.397	-148.391	624.978	-163.397	-148.391	624.978
	(687.272)	(732.743)	(854.939)	(836.710)	(830.432)	(989.718)
Control mean	11338.150	11332.709	11749.649	11338.150	11332.709	11749.649
Observations	5,091	5,090	4,619	5,091	5,090	4,619
SE clustered at	Farmer	Farmer	Farmer	Village	Village	Village
Village FE	✓	✓		✓	✓	
Round FE	✓		✓	✓		✓
Survey date FE		✓			✓	
Farmer FE			✓			✓

Note: Table D.9 shows the impacts of rainfall on the day respondents are interviewed on property damage per flood in BDT that the respondent remembers during the memory elicitation. Rainfall on the day of the survey is measured by the enumerator conducting the survey. Columns (1), (2), and (3) cluster standard errors at the farmer level, and Columns (4), (5), and (6) cluster standard errors at the village level. Columns (1) and (4) include village and survey round fixed effects. Columns (2) and (5) include village and survey date fixed effects. Column (3) and (6) include survey round and farmer fixed effects. p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.10: Impact of First Recalled Memory Features on Subsequent Memories—Village Clustering

	Length (Days)	Harm Crops	Share Crop Damage	Harm House	Value House Damage (BDT)
Panel A. First stage					
Leave-out mean	0.705***	0.156*	0.367***	0.341***	0.335***
	(0.035)	(0.095)	(0.057)	(0.051)	(0.057)
Mean of outcome	18.334	0.931	0.783	0.464	12,991.262
Observations	3,043	2,305	2,290	2,308	2,235
Unique farmers	1,257	1,024	1,017	1,026	992
Panel B. Reduced form					
Leave-out mean	0.573***	0.014	0.263**	0.259***	0.167*
	(0.069)	(0.158)	(0.106)	(0.069)	(0.088)
Mean of outcome	18.727	0.914	0.783	0.437	12,599.214
Observations	1,767	1,300	1,297	1,307	1,275
Unique farmers	591	469	468	472	465
Panel C. IV					
First-mentioned flood characteristic	0.721***	-0.061	0.445**	0.687***	0.482*
	(0.093)	(4.969)	(0.177)	(0.236)	(0.253)
First-stage F-statistic	199.60	0.06	31.58	19.54	19.92
Mean of outcome	18.731	0.914	0.783	0.437	12,685.347
Observations	1,762	1,297	1,292	1,306	1,249
Unique farmers	589	468	466	472	454

Note: Table D.10 presents estimates of the effect of flood memory order on recalled flood features. The sample consists of flood memories elicited from farmers across three survey rounds, excluding respondents who failed survey comprehension checks. For each flood feature, the leave-out-mean is constructed as the average of that feature among other farmers in the same village and round who recalled their oldest flood first (if the respondent recalled oldest first) or their most recent flood first (if the respondent recalled most recent first), excluding the respondent's own value. Column (1) shows impacts on flood length in days. Column (2) shows impacts on an indicator for whether the flood harmed crops. Column (3) shows impacts on the share of crops damaged. Column (4) shows impacts on an indicator for whether the flood damaged the house. Column (5) shows impacts on the value of house damage in BDT. All regressions include farmer and recalled flood year fixed effects. Panel A reports first-stage estimates: the regression of the respondent's own first-mentioned flood feature on the leave-out-mean, estimated on the sample of first-mentioned floods. Panel B reports reduced-form estimates: the regression of later-mentioned flood features on the leave-out-mean, estimated on floods mentioned second or later. Panel C reports instrumental variable estimates, where the first-mentioned flood feature is instrumented by the leave-out-mean, estimated on the sample of later-mentioned floods. The first-stage F-statistic from the corresponding first-stage regression is reported in Panel C. Standard errors are clustered at the village level, and p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.11: Flood Memory Spillover Feature Impacts of Flood Duration—Village Clustering

	Length (Days)	Harm Crops	Share Crop Damage	Harm House	Value House Damage (BDT)
Panel A. First mention					
Leave-out mean	0.705***	0.005***	0.011***	0.006**	383.229***
	(0.035)	(0.001)	(0.001)	(0.003)	(97.890)
Corr. with main variable	1.000	0.165	0.394	-0.051	0.042
Corr. p-value	0.000	0.000	0.000	0.013	0.046
Mean of outcome	18.334	0.931	0.782	0.460	12,860.816
Observations	3,043	2,334	2,319	2,339	2,267
Unique farmers	1,257	1,033	1,026	1,036	1,003
Panel B. Reduced form					
Leave-out mean	0.573***	0.002	0.007***	-0.009	-1.268
	(0.069)	(0.001)	(0.002)	(0.006)	(171.912)
Corr. with main variable	1.000	0.230	0.435	-0.162	-0.019
Corr. p-value	0.000	0.000	0.000	0.000	0.502
Mean of outcome	18.727	0.912	0.782	0.437	12,615.263
Observations	1,767	1,308	1,305	1,315	1,284
Unique farmers	591	472	471	475	468
Panel C. IV					
First-mentioned flood characteristic	0.721***	0.003	0.009***	-0.012*	-20.722
	(0.093)	(0.002)	(0.003)	(0.007)	(214.609)
Corr. with main variable	1.000	0.228	0.434	-0.159	-0.016
Corr. p-value	0.000	0.000	0.000	0.000	0.573
First-stage F-statistic	199.60	155.01	155.52	154.23	153.62
Mean of outcome	18.731	0.912	0.782	0.436	12,593.186
Observations	1,762	1,301	1,298	1,308	1,277
Unique farmers	589	469	468	472	465

Note: Table D.11 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of flood duration (length in days) as the instrument. The first column reports estimates for flood duration itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: whether crops were harmed, the share of crops damaged, whether the house was harmed, and the value of house damage in BDT. The leave-out-mean of flood duration is constructed as the average flood duration reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of flood duration. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of flood duration. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned flood duration is instrumented by the leave-out-mean of flood duration. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between flood duration and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the village level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table D.12: Flood Memory Spillover Feature Impacts of Any Crop Damage—Village Clustering

	Harm Crops	Length (Days)	Share Crop Damage	Harm House	Value House Damage (BDT)
Panel A. First mention					
Leave-out mean	0.156*	4.152**	0.256***	0.047	3,132.067
	(0.095)	(1.634)	(0.090)	(0.085)	(2,644.215)
Corr. with main variable	1.000	0.173	0.664	0.040	0.061
Corr. p-value	0.000	0.000	0.000	0.053	0.004
Mean of outcome	0.931	18.595	0.783	0.464	12,991.262
Observations	2,305	2,933	2,290	2,308	2,235
Unique farmers	1,024	1,225	1,017	1,026	992
Panel B. Reduced form					
Leave-out mean	0.014	1.605	0.095	0.098	-2,194.398
	(0.158)	(2.654)	(0.147)	(0.176)	(6,847.799)
Corr. with main variable	1.000	0.231	0.720	0.053	0.035
Corr. p-value	0.000	0.000	0.000	0.055	0.216
Mean of outcome	0.914	18.750	0.783	0.437	12,592.475
Observations	1,300	1,737	1,297	1,307	1,276
Unique farmers	469	579	468	472	465
Panel C. IV					
First-mentioned flood characteristic	-0.061	-69.196	-2.366	-2.617	17,355.082
	(4.969)	(250.935)	(12.606)	(12.457)	(76,077.686)
Corr. with main variable	1.000	0.232	0.719	0.056	0.040
Corr. p-value	0.000	0.000	0.000	0.043	0.151
First-stage F-statistic	0.06	0.08	0.06	0.07	0.82
Mean of outcome	0.914	18.289	0.783	0.436	12,580.517
Observations	1,297	1,306	1,294	1,304	1,273
Unique farmers	468	472	467	471	464

Note: Table D.12 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of whether the flood harmed crops as the instrument. The first column reports estimates for crop harm itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: flood duration (length in days), the share of crops damaged, whether the house was harmed, and the value of house damage in BDT. The leave-out-mean of crop harm is constructed as the average of the crop harm indicator reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of crop harm. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of crop harm. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned crop harm indicator is instrumented by the leave-out-mean of crop harm. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between crop harm and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the village level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table D.13: Flood Memory Spillover Feature Impacts of Share Crop Damage—Village Clustering

	Share Crop Damage	Length (Days)	Harm Crops	Harm House	Value House Damage (BDT)
Panel A. First mention					
Leave-out mean	0.367***	6.621***	0.127**	0.158**	7,281.294***
	(0.057)	(1.424)	(0.058)	(0.067)	(2,322.877)
Corr. with main variable	1.000	0.392	0.664	0.133	0.161
Corr. p-value	0.000	0.000	0.000	0.000	0.000
Mean of outcome	0.783	18.595	0.931	0.464	12,991.262
Observations	2,290	2,933	2,305	2,308	2,235
Unique farmers	1,017	1,225	1,024	1,026	992
Panel B. Reduced form					
Leave-out mean	0.263**	6.132**	-0.015	-0.329	-10,857.323*
	(0.106)	(2.394)	(0.075)	(0.207)	(5,712.220)
Corr. with main variable	1.000	0.437	0.720	0.039	0.073
Corr. p-value	0.000	0.000	0.000	0.164	0.009
Mean of outcome	0.783	18.750	0.914	0.437	12,592.475
Observations	1,297	1,737	1,300	1,307	1,276
Unique farmers	468	579	469	472	465
Panel C. IV					
First-mentioned flood characteristic	0.445**	13.877***	-0.041	-0.581*	-20,153.189**
	(0.177)	(4.455)	(0.135)	(0.341)	(10,142.007)
Corr. with main variable	1.000	0.439	0.719	0.039	0.077
Corr. p-value	0.000	0.000	0.000	0.160	0.006
First-stage F-statistic	31.58	32.42	29.47	30.86	32.68
Mean of outcome	0.783	18.298	0.914	0.436	12,588.511
Observations	1,292	1,302	1,293	1,300	1,271
Unique farmers	466	470	466	469	463

Note: Table D.13 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of the share of crops damaged as the instrument. The first column reports estimates for the share of crops damaged itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: flood duration (length in days), whether crops were harmed, whether the house was harmed, and the value of house damage in BDT. The leave-out-mean of crop damage share is constructed as the average share of crops damaged reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of crop damage share. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of crop damage share. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned share of crops damaged is instrumented by the leave-out-mean of crop damage share. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between the share of crops damaged and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the village level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table D.14: Flood Memory Spillover Feature Impacts of Any Property Damage—Village Clustering

	Harm House	Length (Days)	Harm Crops	Share Crop Damage	Value House Damage (BDT)
Panel A. First mention					
Leave-out mean	0.341***	1.670	0.004	0.059	10,240.455***
	(0.051)	(1.110)	(0.031)	(0.037)	(2,000.061)
Corr. with main variable	1.000	-0.048	0.040	0.132	0.761
Corr. p-value	0.000	0.017	0.056	0.000	0.000
Mean of outcome	0.464	18.595	0.931	0.783	12,991.262
Observations	2,308	2,933	2,305	2,290	2,235
Unique farmers	1,026	1,225	1,024	1,017	992
Panel B. Reduced form					
Leave-out mean	0.259***	-1.441	-0.035	-0.169***	5,437.115*
	(0.069)	(1.454)	(0.030)	(0.045)	(3,208.839)
Corr. with main variable	1.000	-0.160	0.049	0.035	0.780
Corr. p-value	0.000	0.000	0.075	0.207	0.000
Mean of outcome	0.437	18.750	0.914	0.783	12,592.475
Observations	1,307	1,737	1,300	1,297	1,276
Unique farmers	472	579	469	468	465
Panel C. IV					
First-mentioned flood characteristic	0.687***	-5.775	-0.092	-0.442***	15,491.116
	(0.236)	(4.387)	(0.078)	(0.162)	(10,071.912)
Corr. with main variable	1.000	-0.167	0.050	0.034	0.780
Corr. p-value	0.000	0.000	0.074	0.215	0.000
First-stage F-statistic	19.54	19.44	19.77	19.69	16.87
Mean of outcome	0.437	18.287	0.914	0.783	12,602.351
Observations	1,306	1,308	1,299	1,296	1,275
Unique farmers	472	473	469	468	465

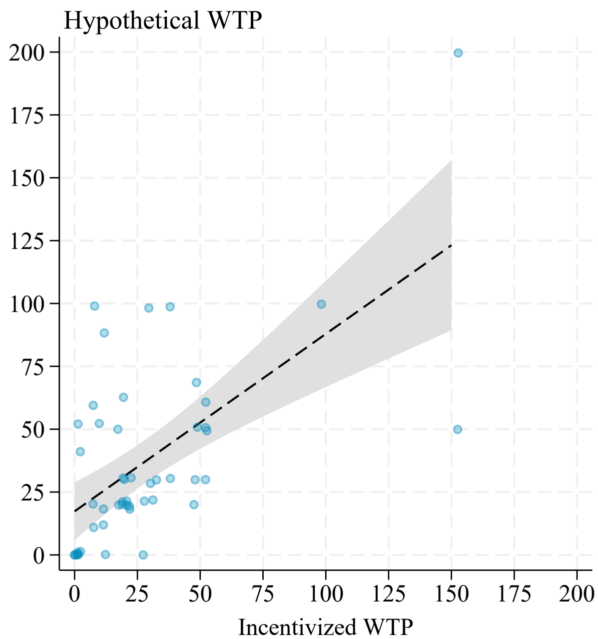
Note: Table D.14 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of whether the flood harmed the house as the instrument. The first column reports estimates for house harm itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: flood duration (length in days), whether crops were harmed, the share of crops damaged, and the value of house damage in BDT. The leave-out-mean of house harm is constructed as the average of the house harm indicator reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of house harm. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of house harm. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned house harm indicator is instrumented by the leave-out-mean of house harm. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between house harm and the column's outcome variable, along with its p-value, computed within the estimation sample. Standard errors are clustered at the village level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table D.15: Flood Memory Spillover Feature Impacts of Property Damage Value—Village Clustering

	Value House Damage (BDT)	Length (Days)	Harm Crops	Share Crop Damage	Harm House
Panel A. First mention					
Leave-out mean	0.335***	0.085***	-0.000	0.001	0.005***
	(0.057)	(0.030)	(0.001)	(0.001)	(0.001)
Corr. with main variable	1.000	0.036	0.063	0.165	0.759
Corr. p-value	0.000	0.073	0.003	0.000	0.000
Mean of outcome	12,991.262	18.614	0.931	0.785	0.463
Observations	2,235	2,920	2,293	2,280	2,298
Unique farmers	992	1,219	1,018	1,012	1,021
Panel B. Reduced form					
Leave-out mean	0.167*	0.010	-0.000	-0.002**	0.003
	(0.088)	(0.031)	(0.001)	(0.001)	(0.002)
Corr. with main variable	1.000	-0.016	0.032	0.073	0.781
Corr. p-value	0.000	0.561	0.250	0.009	0.000
Mean of outcome	12,599.214	18.757	0.914	0.783	0.436
Observations	1,275	1,734	1,297	1,296	1,304
Unique farmers	465	578	468	468	471
Panel C. IV					
First-mentioned flood characteristic	0.482*	0.038	-0.001	-0.005*	0.008
	(0.253)	(0.093)	(0.002)	(0.003)	(0.005)
Corr. with main variable	1.000	-0.026	0.027	0.065	0.784
Corr. p-value	0.000	0.358	0.337	0.021	0.000
First-stage F-statistic	19.92	23.19	23.48	23.40	23.03
Mean of outcome	12,685.347	18.610	0.916	0.786	0.436
Observations	1,249	1,274	1,265	1,264	1,272
Unique farmers	454	460	456	456	459

Note: Table D.15 presents estimates of the spillover effects of flood memory order on recalled flood features, using the leave-out-mean of the value of house damage (in BDT) as the instrument. The first column reports estimates for the value of house damage itself (the "own" effect), while the remaining columns report spillover effects on the other four flood features: flood duration (length in days), whether crops were harmed, the share of crops damaged, and whether the house was harmed. The leave-out-mean of house damage value is constructed as the average value of house damage reported by other farmers in the same village and round who were assigned the same recall order (oldest first or most recent first), excluding the respondent's own value. For spillover columns in this table, coefficients and standard errors are scaled by 1,000. All regressions include farmer and recalled flood year fixed effects, and the sample excludes respondents who failed survey comprehension checks. Panel A reports first-stage estimates on the sample of first-mentioned floods, regressing each outcome on the leave-out-mean of house damage value. Panel B reports reduced-form estimates on later-mentioned floods, regressing each outcome on the leave-out-mean of house damage value. Panel C reports instrumental variable estimates on the later-mentioned flood sample, where the respondent's own first-mentioned value of house damage is instrumented by the leave-out-mean of house damage value. The first-stage F-statistic from the corresponding first-stage regression is reported. Each panel also reports the pairwise correlation between the value of house damage and the column's outcome variable, along with its p-value, computed within the estimation sample. Coefficients and standard errors are scaled by 1,000. Standard errors are clustered at the village level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Figure D.3: Hypothetical vs. Incentivized Flood Insurance WTP



Note: Figure D.3 shows a scatter plot and a line of best fit comparing the incentivized version of the flood insurance willingness-to-pay elicitation against the non-incentivized version in the sample of farmers asked both. Points have been jittered to illustrate mass.

Table D.16: Impact of Contemporaneous Rain-Status Match on Recall Consistency—Village Clustering

	Exact Year	Year ± 1	Year ± 2	Weak Features	Strong Features	Year ± 1 + Strong
Panel A. Same rain status in both rounds vs. different						
Same rain status	0.029*	0.056***	0.063***	0.029	0.011	0.008
	(0.016)	(0.019)	(0.019)	(0.020)	(0.011)	(0.006)
Control mean	0.207	0.330	0.389	0.225	0.042	0.021
Observations	3,862	3,862	3,862	2,743	2,742	2,737
Panel B. Both rounds raining vs. different rain status						
Both rounds raining	0.038	0.086**	0.083**	0.060*	0.031*	0.029**
	(0.030)	(0.034)	(0.035)	(0.036)	(0.018)	(0.014)
Control mean	0.208	0.332	0.391	0.226	0.043	0.021
Observations	2,720	2,720	2,720	1,916	1,916	1,911
Panel C. Both rounds not raining vs. different rain status						
Both rounds not raining	0.025	0.036*	0.051**	0.020	0.004	-0.001
	(0.018)	(0.020)	(0.021)	(0.023)	(0.013)	(0.005)
Control mean	0.207	0.330	0.389	0.226	0.042	0.021
Observations	3,323	3,323	3,323	2,440	2,439	2,434

Note: Table D.16 tests whether rainfall conditions on the day of the survey affect the consistency of flood recall across survey rounds. The dependent variable in each column is an indicator for whether a flood recalled in an earlier round can be matched to a flood recalled by the same respondent in a later round, under six match definitions of increasing strictness. Exact year requires the reported flood year to be identical across rounds. Year ± 1 and Year ± 2 allow the reported flood year to differ by up to one or two years, respectively. Weak Feature Match requires agreement on whether the flood harmed crops and whether it harmed the house, approximate agreement on the share of crops damaged (within 25%), and approximate agreement on flood duration (within 3 days). Strong Feature Match requires exact agreement on whether crops and the house were harmed, exact agreement on the share of crops damaged and flood duration, and agreement on the value of house damage within 25% of the larger reported value. Year ±1 and Strong Feature Match requires both the year-based and strong feature criteria to be jointly satisfied. Year-based match definitions are computed over floods with a non-missing reported flood year. Feature-based match definitions are computed over the subset of floods for which harm questions were asked in both the earlier and later round. The sample is restricted to floods whose reported year predates the survey year of the earlier round. In Panel A, the regressor is an indicator equal to one if the enumerator recorded the same rainfall status (both raining or both not raining) on the survey dates of the earlier and later rounds, with the omitted category being different rainfall across the two rounds. Panel B restricts the sample to floods where it was either raining on both survey dates or raining on one but not the other (excluding observations where it was not raining on either date), so the regressor captures the effect of both rounds being conducted in the rain relative to only one. Panel C restricts to floods where it was either not raining on both survey dates or raining on one but not the other (excluding observations where it was raining on both dates), so the regressor captures the effect of both rounds being conducted without rain relative to only one. All regressions include village and round fixed effects. Control means report the average match rate among the different-rain-status group within each panel's sample. Standard errors are clustered at the village level, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.

Table D.17: Flood Information Experiment Treatment Effects on Memories—All Flood Risk

	(1) Share House Damaged	(2) Share Crop Damaged	(3) Mean Crop Damage Amount	(4) Mean House Damage Amount	(5) Mean Flood Duration	(6) Severity Index
Flood Before	-0.003	-0.026	-0.027	87.709	-0.474	-0.016
	(0.026)	(0.031)	(0.028)	(1101.330)	(0.796)	(0.069)
Damage Before	0.036	0.006	0.017	-104.726	0.312	0.028
	(0.031)	(0.036)	(0.033)	(1265.494)	(0.923)	(0.080)
Flood After	-0.010	-0.031	-0.019	-454.451	-0.789	-0.055
	(0.026)	(0.031)	(0.029)	(1100.036)	(0.802)	(0.067)
Damage After	0.004	0.031	0.007	282.936	0.537	0.026
	(0.031)	(0.037)	(0.033)	(1296.782)	(0.934)	(0.077)
Control mean	0.277	0.627	0.532	9,832.818	12.758	0.000
Observations	2,131	2,131	2,131	2,131	2,131	2,131

Note: Table D.17 presents presents treatment effects of the information treatments on the features of floods recalled during the memory elicitation, including all respondents regardless of flood risk. The experiment randomly assigns respondents to receive information either before or after the flood memory elicitation or to a pure control group. Within each timing condition, respondents receive either flood occurrence information alone or flood occurrence combined with damage information. "Flood Before" and "Flood After" are indicators for receiving flood occurrence information before or after the memory elicitation, respectively. "Damage Before" and "Damage After" are indicators for additionally receiving damage information before or after the elicitation; these coefficients capture the incremental effect of damage information beyond flood occurrence information alone. Column (1) shows impacts on the share of recalled floods that damaged the house. Column (2) shows impacts on the share of recalled floods that damaged crops. Column (3) shows impacts on the mean share of crops damaged across recalled floods. Column (4) shows impacts on the mean value of house damage across recalled floods, winsorized at the 90th percentile. Column (5) shows impacts on the mean duration in days across recalled floods. Column (6) shows impacts on a summary index of flood severity, constructed following Kling et al. (2007) by averaging the standardized values of the five component measures, where each is standardized by subtracting the control group mean and dividing by the control group standard deviation, and the resulting index is re-standardized to have a control group mean of zero and standard deviation of one. For all outcomes, respondents who recalled no floods are assigned a value of zero. The control mean reports the average outcome among respondents in the pure control group. All regressions use heteroskedasticity-robust standard errors, and p-values are denoted as follows: * p< 0.10, ** p< 0.05, *** p< 0.01.

Table D.18: Flood Information Experiment Treatment Effects on Beliefs—All Flood Risk

	(1) Expected Flood Days Next Year	(2) Expected Flood Days Next Five Years	(3) Expected Crop Damage Next Year	(4) Expected Crop Damage Next Five Years	(5) Expectations Index
Flood Before	0.001	0.148	0.004	-0.001	0.007
	(0.498)	(0.586)	(0.023)	(0.023)	(0.064)
Damage Before	0.118	0.649	0.002	0.027	0.044
	(0.571)	(0.671)	(0.026)	(0.027)	(0.074)
Flood After	-0.499	-0.171	-0.007	0.006	-0.023
	(0.496)	(0.583)	(0.023)	(0.024)	(0.064)
Damage After	0.447	0.681	-0.003	0.026	0.056
	(0.578)	(0.665)	(0.026)	(0.028)	(0.074)
Control mean	6.279	8.803	0.356	0.455	0.000
Observations	2,121	2,123	2,111	2,112	2,128

Note: Table D.18 presents presents treatment effects of the information treatments on respondents' expectations about future flooding, including all respondents regardless of predicted flood risk. The experiment randomly assigns respondents to receive information either before or after the flood memory elicitation or to a pure control group. Within each timing condition, respondents receive either flood occurrence information alone or flood occurrence combined with damage information. "Flood Before" and "Flood After" are indicators for receiving flood occurrence information before or after the memory elicitation, respectively. "Damage Before" and "Damage After" are indicators for additionally receiving damage information before or after the elicitation; these coefficients capture the incremental effect of damage information beyond flood occurrence information alone. Column (1) shows impacts on the expected number of flood days in the next year. Column (2) shows impacts on the expected number of flood days over the next five years. Column (3) shows impacts on the expected share of crops damaged by flooding in the next year. Column (4) shows impacts on the expected share of crops damaged by flooding over the next five years. Column (5) shows impacts on a summary index of flood expectations, constructed following Kling et al. (2007) by averaging the standardized values of the four component measures, where each is standardized by subtracting the control group mean and dividing by the control group standard deviation, and the resulting index is re-standardized to have a control group mean of zero and standard deviation of one. The control mean reports the average outcome among respondents in the pure control group. All regressions use heteroskedasticity-robust standard errors, and p-values are denoted as follows: * $p< 0.10$, ** $p< 0.05$, *** $p< 0.01$.