# Learning versus Unlearning:
## An Experiment on Retractions

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September 28, 2022

Abstract. Widely discredited ideas nevertheless persist. Why do we fail to "unlearn"? We study the effectiveness of retractions—the revoking of earlier information—in correcting beliefs. Our experimental design identifies belief updating from retractions—*unlearning*—and compares it to updating from equivalent new information—*learning*. Subjects do not fully unlearn from retractions and update approximately one-third less from retractions versus new information. Although we document several well-known biases in belief updating, our results require an explanation that treats retractions as intrinsically different. We find evidence for one such mechanism, while ruling out several others: retractions are more complex than direct information.

Keywords. Belief Updating, Retractions, Misinformation, Learning.
JEL Codes. D83, D91, C91.

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# 1. INTRODUCTION

Retracted information often continues to influence beliefs, even once widely discredited. Baseless rumors, mistaken earnings announcements, fraudulent findings in published research, false claims of politicians; all tend to linger long after being revealed to be unfounded. Such misinformation is inevitable, and will inevitably influence beliefs, initially. But why is it so frequently easier to learn (incorrect) information than to subsequently unlearn it? Understanding why discredited information persists, and how people *do* unlearn, matters not only for the debate about misinformation—its harm and how to combat it—but also for designing information campaigns more broadly. Is the failure to unlearn fundamental, or is it driven by context-specific factors? To correct beliefs, should we emphasize the error of the original information, or instead emphasize the correct alternative information?

Various established biases in belief updating could result in the failure to unlearn from retractions, each suggesting alternative interventions to effectively correct misinformation.[1] For instance, under *confirmation bias*, individuals update more in the direction of their prior beliefs, and so would update less from retractions of evidence that ex-ante confirmed their beliefs. Alternatively, under *cognitive dissonance*—whereby individuals resist internalizing evidence which indicates that their past actions were incorrect— the effectiveness of retractions would depend on whether individuals have already acted on or engaged with the retracted information.[2] However, as far as we are aware, none of the existing explanations are specific to retractions per se, but apply to any kind of new information, and are therefore unable to explain *inherent* differences between learning and unlearning.

In this paper, we first establish a fundamental asymmetry between learning and unlearning, and then propose and examine one hypothesis for it: that retractions are inherently more complex than evidence that is directly informative about the state. Our simple experimental design allows us to define retractions, to quantify unlearning, and to compare it to learning from new evidence. Subjects are presented with multiple observations that may or may not be informative about an uncertain state. Retractions are amendments of earlier information, informing subjects that

[1] In psychology, the observation that discredited information can have residual impact is known as the *continued influence effect*; we discuss this literature in Section 1.1. We note that we are not aware of any work which focuses on probability assessments, which leaves open the possibility that the mechanism behind it is the same as behind other biases. We also note this updating from retractions—or information about information in general—appears relatively unexplored in economics.

[2] See Akerlof and Dickens (1982) for a discussion of how this bias might emerge and relevant experimental evidence.

an observation was not informative. Our (pre-registered) analysis shows that information still has residual influence even once retracted—retractions are not fully effective—and that beliefs move approximately one-third less in response to retractions than to equivalent new observations, across a rich set of histories. Retractions also result in longer response times and higher variance in updating, suggesting they are inherently more complex and difficult to interpret than (informationally-equivalent) evidence which is directly informative of the state. In short, when addressing misinformation, retractions should not be expected to fully correct beliefs, and additional new information may also be necessary for unlearning.

Our design is a variation on a classic bookbag-and-poker-chips (or urns-and-balls) experiment; we choose this setting since it is the most direct method of connecting behavioral anomalies to theoretical models of information processing. We present subjects with draws of colored balls (blue or yellow) from a box with replacement, with one color being more likely depending on an underlying state. In particular, the box contains a "truth ball" which is either yellow or blue—the underlying state, over which we elicit subjects' beliefs—as well as four "noise balls," two yellow and two blue. After presenting subjects with a series of such draws, in which they are told the color but not the truth/noise status of each ball, we then either present another such draw, or inform subjects whether a randomly chosen earlier ball draw was the truth ball or a noise ball. This latter event—in particular, when an earlier draw is disclosed to be a noise ball and thus uninformative of the underlying state—is what we refer to as a *retraction*. After each event, we elicit beliefs on the underlying state, allowing us to make two comparisons of particular interest: test (a), beliefs following retractions versus beliefs without having observed the retracted observation in the first place—whether retractions work; and test (b), beliefs following retractions versus beliefs following new draws which yield identical Bayes updates (in our setup, a draw of the opposite color to that which is retracted)—retractions versus equivalent new observations.

Our first set of results shows that subjects fail to fully unlearn from retractions and that there is a fundamental asymmetry between learning and unlearning. First, running test (a)—whether retractions work—we find that beliefs consistently display a residual effect of the retracted observation: subjects assign greater probability to the state being of the same color as the retracted observation. Second, running test (b)—comparing retractions to equivalent new observations—we show that beliefs update less in response to a retraction than to an equivalent new observation. Both results are robust across multiple variants of the experiment and hold regardless of details of the retraction, for example, whether the information is confirmatory or not, or whether priors are more moderate or more extreme. The magnitude of this effect also appears economically meaningful; beliefs move on average one-third less when information is a retraction (see Section 5.3).

Why are retractions less effective? We leverage our design to speak to mechanisms. To begin, our theoretical analysis considers a general class of *quasi-Bayesian*[3] updating models (which nests Bayesian updating), and shows that our results cannot be reconciled with any explanation that does not treat retractions as inherently different: While test (a) need not hold under all such explanations—unlearning need not be complete—test (b) would always return a null result—unlearning and learning would be equivalent. We thus turn to mechanisms under which the two are inherently different.

We propose that retractions are less effective because interpreting them requires conditional reasoning, and is thus more difficult than interpreting new observations, consistent with existing work associating higher complexity with a greater 'compression' of beliefs. Retractions require conditional reasoning because they provide information about how to interpret past information, rather than about the underlying state directly. To test this proposed mechanism, we take two approaches. First, we provide evidence that updating from retractions is harder by considering process data and behavioral traits previously used in the literature. Second, we consider two instances in which belief updating from or after retractions is intuitively more difficult, and test both whether subjects update less in these instances and whether our behavioral markers confirm that the updating is more complex.

We consider two behavioral markers of the complexity of an updating task, and compare them under updating from retractions versus new observations. Specifically, we identify the effect of retractions versus new information on decision times and the variability of belief reports, both of which have been used as measures of cognitive noise in past work—see, for instance, Enke and Graeber (2020), Khaw et al. (2021), and Frydman and Jin (2022). Both proxies are larger when updating from retractions, as compared to equivalent new information, with updating taking 10% longer and leading to more than a one-third increase in variance. So, while previously we showed that updating is less effective from retractions than from equivalent direct evidence about the underlying state, these results suggest that it is also more cognitively demanding.

We analyze two situations in which updating is intuitively more complex: when earlier (versus more recent) observations are retracted, and when there has already been a retraction. For the first,

[3] We define this term formally in Section 2.1.

we argue that when retractions refer to the most recent observation, updating from a retraction is easier, since it requires simply to return to the belief held previously. Indeed when retractions refer to the most recent observation, they are more effective in inducing unlearning, with subjects updating more from them (although they are still not fully effective nor as influential as new observations). Moreover, corresponding decision times are shorter and belief variability lower, validating these proxies of complexity. For the second, if it is harder to update from retractions, one might expect it to also be harder to update from new observations *following* retractions. Our design enables us to identify the effect of past retractions on future updating, by comparing comparing updating when there was a previous retraction to when instead there was previously an informationally equivalent new observation. Examining both updating itself, as well as our behavioral markers for difficulty, we find that subjects indeed update less from new observations following a retraction, take longer to do so, and their estimates exhibit greater variability. Taken together, this evidence is consistent with our proposed mechanism and also supports our proxy measures for complexity.

Having presented and supported our proposed mechanism, we then turn to two alternative mechanisms in which retractions are treated inherently differently, and which could thus potentially explain our results. First, we document that the ineffectiveness of retractions is not caused by retractions exacerbating confirmation bias. In fact, they flip it: when updating from new observations, subjects (slightly) overinfer and do more so when observations confirm the prior—indicating confirmation bias—whereas when updating from retractions they underinfer and exhibit anti-confirmation bias. While this thus cannot explain the ineffectiveness of retractions, it again underlines how retractions are treated fundamentally differently than new observations, despite being informationally equivalent in our setting.

Second, we study whether retractions being ineffective is driven by information being hard to unlearn once it has been "acted" upon, a type of endowment effect for information. If so, the residual effect of retracted signals could be due to subjects having previously *used* the signals to state their beliefs, before the retraction. To test this, we randomly assign subjects to a treatment arm of the experiment where we only elicit beliefs at the end of a sequence of signals and retractions, and not after each draw. We fail to reject the hypothesis that retractions have the same effect on belief updating as in our baseline setup, where beliefs are elicited after each draw.

Finally, we test for whether our results simply reflect some limited subject understanding in what the data generating process was. We are able to rule out misinterpreting that the draws are made with replacement. We also consider dropping subjects who are 'noisy' or prone to mistakes (e.g. updating in the wrong direction), or who did not correctly answer at first try unincentivized comprehension questions. A theme that emerges is that our results are stronger when restricting to subjects who appear to have understood the task better.[4] We conclude that our findings are not an artifact of some consistent misinterpretation of the design. Although we are not powered for a fully-fledged within-subject analysis, inspection of individual heterogeneity in our results indicates that the failure of retractions to inducing unlearning, and their ineffectiveness compared with new observations, is a general phenomenon in our sample.

We believe that the observation that retractions are *fundamentally less effective* has practical value. Taken together, our results provide guidelines regarding how and when individuals can be expected to update beliefs with information about information, of potential relevance for information campaigns. They also show that the finding is general, and not tied to any particular domain. A policymaker deciding whether to provide guidance that may need to be corrected later should understand that this may not be so easy—it is in general unreasonable to expect a retraction to result simply in the "deletion" of a piece of information. Our suspicion is that in many real-world cases, appreciating the inability to correct retractions ex-post would have changed the calculus regarding decisions to disseminate information.

We present our theoretical framework in Section 2, after reviewing the literature below. Section 3 lists our main hypotheses and Section 4 presents our design and implementation. Our analysis comes in three parts: Section 5 documents retraction failures, Section 6 examines mechanisms (both our main proposal and others), and Section 7 discusses robustness. Section 8 concludes.

### 1.1. Literature Review of Related Experimental Evidence

Our work fits squarely within two literatures; one studying the impact of retractions, and one studying belief updating.

[4] This is perhaps unsurprising, since to find any effect requires subjects to act differently for retractions; if subjects answered randomly or always answer 50-50 we would not document any difference. In contrast, it is worth emphasizing that most of our sample did very well on unincentivized comprehension questions, confirming our assertion that our design achieved its desired simplicity despite also containing sufficient richness to define retractions and speak to mechanisms.

## Retractions

In psychology, the idea that information may have a residual impact even once retracted was articulated in an important early study by Johnson and Seifert (1994) and is known as the *continued influence effect* (see e.g. Ecker et al., 2022). Lewandowsky et al. (2012) surveys this literature and highlights several possible reasons; we briefly mention that none appear capable of explaining our results, given our experimental design.[5]

To the best of our knowledge, all past experiments on retractions involve information that is (at least partially) subjective. This leaves open that subjects are in fact interpreting them correctly within their subjective worldview. However, our theoretical framework highlights a more serious issue: unless one carefully implements retractions in a particular way, it may be that actually subjects *should* rationally underreact to them. Finally, since equivalent new information is not presented in these experiments, they do not separately identify retraction failures from other well-known biases, such as confirmation bias; we discuss these biases below.

Our contribution is to demonstrate and quantify retraction ineffectiveness as distinct from biases in updating from direct information. By focusing on a "context-free" setting, our results suggest that the failure of retractions is a general phenomenon and not due to idiosyncratic features of each of the settings in which it had previously been documented. We briefly review some of these:

*Political Information.* Perhaps the largest number of experiments in this literature have studied the correction of information in political settings. While interpreting magnitudes is sometimes difficult in these studies, most show retractions have diminished effectiveness in political contexts.[6] For instance, in the context of the 2016 US Presidential election (Swire et al., 2017; Nyhan et al., 2019) and the 2017 French Presidential election (Barrera et al., 2020), fact-checking did improve factual

[5] Two of the four explanations highlighted involve memory; our design explicitly shuts down the memory channel by reminding subjects of all information they have seen. One explanation relates to difficulties in dislodging mental models in settings with complex causal chains; the suggestion is that subjects cannot disregard information when a narrative is built around it. Our setting appears too stripped down for complex narratives to have significant role. The last explanation involves a distaste for acknowledging mistakes. While this factor does not *appear* relevant to our design, if anything we provide evidence against it as responsible for our results, since we find that it does *not* matter how often subjects are asked to state their beliefs.

[6] In the context of highly politically charged topics, retractions may in rare cases *backfire*, leading subjects to believe more strongly in the retracted information. Nyhan and Reifler (2010) noted the occurrence of backfiring in an experiment where they provided subjects with information about the presence of weapons of mass destruction in Iraq during the early 2000s, and subsequently provided them with corrections. This extreme form of retraction failure, for the most part, has not been replicated. See Nyhan (2021) for an authoritative discussion.

knowledge, but was less effective than the original corrected information. Many studies suggest motivated reasoning as the main explanation for the ineffectiveness of retractions in political contexts.[7] Although it may indeed play a significant role, our results indicate that retractions fail even in the absence of motivated reasoning.

*Financial Information.* Other work has focused on the effectiveness of retractions in financial settings, where designs tend to involve presentations of earnings reports or related financial statements and then instructions to disregard. The focus is typically less on beliefs themselves, but rather how the information is *used* in assessments or investments. Grant et al. (2021), Tan and Tan (2009), and Tan and Koonce (2011) run experiments using such designs, finding that retractions have diminished effectiveness in these domains, and discuss ways this can be combated.

*Jury Trials.* Jury trials often feature information which jurors are instructed to disregard. Experiments on this question tend to focus on whether the reason evidence should be disregarded matters. Kassin and Sommers (1997), Thompson et al. (1981) and Fein et al. (1997) conduct experiments documenting that juries do not always simply disregard information if instructed to do so. While these studies do show retracted information is not so easily disregarded, it is less clear that this reflects a departure from Bayesian rationality, since the retracted information is often meaningful.

**Belief Updating Biases**

Our paper builds on the experimental literature on errors in belief updating. Benjamin (2019) provides a comprehensive survey; of independent interest, we replicate many of its key findings.[8]

Our goal is to identify and distinguish the failure of unlearning from retractions from other well-known biases. For instance, we document *base-rate neglect* (whereby agents underweight the prior when updating; see, e.g., Esponda et al. 2020), as well as *confirmation bias*, discussed above (see also Rabin and Schrag, 1999).[9] Several models have been proposed to explain these biases in belief updating, namely probability weighting (see e.g. Kahneman and Tversky, 1979, 1992) and cognitive imprecision (Woodford, 2020; Enke and Graeber, 2020; Thaler, 2021). We show in our theoretical framework that the diminished effectiveness of retractions is *distinct from these biases* and cannot be explained by models that do not treat retractions inherently differently.

[7] Various studies have articulated how motivated reasoning influences belief processing in political domains; for instance, see Angelucci and Prat (2020), Thaler (2020), and Taber and Lodge (2006).

[8] For recent papers studying these biases, see, for instance, Ambuehl and Li (2018), Coutts (2019), and Thaler (2021).

[9] To avoid confounding factors, our design features exogenous information; Charness et al. (2020) study how biases may influence subjects' *choice* of sources of information.

Our analysis suggests that "information about information" is harder to process than "direct information." Though our focus on *un*learning is new, there is precedent for the idea that contingent reasoning entails higher cognitive effort. One of the first documented difficulties of contingent reasoning was Charness and Levin (2005), in winner's curse settings.[10] Perhaps most related to our study is Enke (2020), which documents in a pure prediction setting that many subjects consistently fail to account for the informational content from the *absence* of observations, suggesting a failure of contingent reasoning. One microfoundation for "information about information" being harder to process than "direct information" is that subjects face *higher cognitive imprecision* in their understanding of the informativeness of a retraction than of an observation—see Woodford (2020) for a survey on the literature and Enke and Graeber (2020) and Thaler (2021) for recent applications to belief updating.

A final connection worth highlighting is between our framework and the *principle of restricted choice* from Miller and Sanjurjo (2019). Miller and Sanjurjo (2019) argue that many famous mistakes in probabilistic reasoning emerge from failing to account for how signals provided by an information structure are *restricted*. Perhaps most relevant to our exercise is the *Monty Hall Problem*, where a subject is asked to select one of three doors, with one hiding a prize and two hiding goats. After making a choice, one of the *un*selected doors that hides a *goat* is revealed. The subject is then offered to switch their choice. The principle of restricted choice is relevant because only unselected doors *without a prize* can be revealed; thus, the unselected door *not* revealed to hide a goat is more likely to hide a prize. Despite this, Friedman (1998) shows that subjects err with striking consistency, choosing often to keep their choices.[11] Revealing the validity of a ball in this paper may strike some readers as analogous to revealing whether a door is hiding a goat.

Two points on this connection are critical. First, since only incorrect information can be retracted, restrictions like those from Miller and Sanjurjo (2019) do naturally emerge for *certain* implementations of retractions. Second, *our* implementation of retractions is *unrestricted*, thus eliminating the relevance of what Miller and Sanjurjo (2019) identify as a source of Monty Hall mistakes. As this design detail is subtle, we defer further discussion to our theoretical framework.

[10] See Esponda and Vespa (2014) and Martínez-Marquina et al. (2019) for more on difficulties in contingent reasoning in particular games.

[11] See also Borhani and Green (2018) for a theoretical treatment. To our knowledge, follow-on work to Friedman (1998) has not altered the underlying mathematical problem, instead varying other circumstances around it such as incentives (Palacios-Huerta, 2003) or how it is presented and explained to participants (James et al., 2018).

## 2. FRAMEWORK

This section presents formal definitions and includes our main framework.

### 2.1. Learning: Generating Information and Updating Beliefs

We first describe the "truth-or-noise" information arrival processes we use in our experiment, and explain how many belief updating biases can be explained using *quasi-Bayesian models*, which we define below. In the next section, we articulate why our findings will not be explained by any such model alone, and instead requires an explanation specific to the nature of retractions.

We consider a decisionmaker who forms beliefs over a state $\theta$, which takes one of two values with equal probability, say $\theta \in \{-1, 1\}$. At time $t$, the decisionmaker has access to observation $s_t \in \{-1, 1\}$ informative of the state $\theta$, and independent conditional on the $\theta$. Throughout, we use the term "signal" as a generic term for information, and the term "observation" or "draw" for signals $s_t$ that provide *direct* information about the state. We use $P(\cdot)$ to denote *objective* probabilities associated with the data generating process, and $b(\theta \mid \cdot)$ to denote the decisionmaker's *subjective* beliefs about the state.

Each observation $s_t$ can either be *true*, in which case $s_t = \theta$, or *noise*, in which case it is given by an independent $\epsilon_t$. We denote the former event by $\{n_t = 0\}$ and the latter by $\{n_t = 1\}$. Formally,

$$s_t = (1 - n_t) \cdot \theta + n_t \cdot \epsilon_t, \tag{1}$$

where $n_t \in \{0, 1\}$ and $n_t$, $\epsilon_t$ and $\theta$ are independent. For simplicity, we write $S_t = \{s_1, \ldots, s_t\}$.

For a Bayesian decisionmaker, $b(\theta \mid S_t) = P(\theta \mid S_t)$. Past work has routinely rejected this hypothesis. One way to test for deviations from Bayesian updating (see Benjamin, 2019) is to note that log-odds updates are constant when observations are identically distributed; that is, if $K(s_{t+1}) = \log\left(P(s_{t+1}|\theta)/P(s_{t+1}|-\theta)\right)$, then for a Bayesian decisionmaker the following equation

$$\log\left(\frac{b(\theta \mid S_{t+1})}{b(-\theta \mid S_{t+1})}\right) = \alpha \log\left(\frac{b(\theta \mid S_t)}{b(-\theta \mid S_t)}\right) + \beta K(s_{t+1}), \tag{2}$$

should hold for $\alpha = 1$ and $\beta = 1$. Base rate neglect, for instance, corresponds to the hypothesis that $\alpha < 1$; underinference corresponds to the hypothesis that $\beta < 1$.

A common alternative is to instead assume a strictly increasing probability weighting function

$f$ exists such that:

$$b(\theta \mid S_t) = f(P(\theta \mid S_t)).$$

Even if $\alpha \neq 1$ or $\beta \neq 1$, as long as $f$ is strictly increasing, it is invertible, so $f^{-1}(b(\theta \mid \cdot)) = P(\theta \mid \cdot)$. It then follows that $b(\theta \mid \cdot)$ is given by the following identity:

$$\log\left(\frac{f^{-1}(b(\theta \mid S_{t+1}))}{f^{-1}(b(-\theta \mid S_{t+1}))}\right) = \log\left(\frac{f^{-1}(b(\theta \mid S_t))}{f^{-1}(b(-\theta \mid S_t))}\right) + K(s_{t+1}), \tag{3}$$

As long as some $f$ exists such that $b(\theta \mid \cdot) = f(P(\theta \mid \cdot))$, one could determine $f$ by using (3) to "trace out" $f$.[12] Inspired by Cripps (2021), we call such a decisionmaker "quasi-Bayesian:"[13]

**Definition 1.** *We say that a decisionmaker is a "quasi-Bayesian" if there exists a strictly increasing $f$ such that $b(\theta \mid s)$ can be derived from $b(\theta)$ by (i) computing $f^{-1}(b(\theta))$, (ii) determining $f^{-1}(b(\theta \mid s))$ using (3), and (iii) composing the result with $f$ to obtain $b(\theta \mid s)$.*

Note that, to accommodate some forms of confirmation bias, it may be necessary to allow the function $f$ to depend on the initial belief $b(\theta)$ subjects update from; we strive, however, to be as agnostic as possible about whether this should be the case, and our comparisons will hold across a number of possible assumptions.

Updating rules satisfying this requirement are commonly used in experimental work (e.g. Angrisani et al., 2019).[14] Among possible microfoundations for such distortion is the hypothesis that the agent faces some cognitive imprecision, as posited by models of cognitive uncertainty, efficient coding, and sequential sampling.[15] In the spirit of the models in this literature, consider a situation in which our decisionmaker faces uncertainty about how to interpret the likelihood of evidence $s_{t+1}$ and update beliefs. Suppose that the decisionmaker's prior is given by $K(s_t) \sim \mathcal{N}(0, \sigma^2)$,

[12] For a general signal history, the decisionmaker's posterior belief is then

$$b(\theta \mid S_{t+1}) = f\left(\frac{f^{-1}(b(\theta \mid S_t))P(s_{t+1}|\theta)}{f^{-1}(b(\theta \mid S_t))P(s_{t+1}|\theta) + f^{-1}(b(-\theta \mid S_t))P(s_{t+1}| -\theta)}\right).$$

[13] Cripps (2021) axiomatizes quasi-Bayesian updating showing that a decisionmaker's belief updating will satisfy this property as long as their updating is "divisible"—roughly, that observations are treated as exchangeable.

[14] Since at least Kahneman and Tversky (1979), various forms of $f$ have been proposed, criticized, and debated. See McGranaghan et al. (2022) for a recent contribution and a discussion.

[15] While distinct, the literatures are closely related. Efficient coding (Wei and Stocker, 2015) and cognitive uncertainty models have been increasingly popular in economics; e.g. Khaw et al. (2021), Frydman and Jin (2022), Enke and Graeber (2020), and Thaler (2021). Models of sequential sampling provide a relationship between cognitive uncertainty and time through evidence accumulation (Bhui and Gershman, 2018). See Ratcliff et al. (2016) for a survey of sequential sampling models in psychology and neuroscience, and Fudenberg et al. (2018) and Gonçalves (2022) for recent applications in economics.

and that by deliberating the decisionmaker obtains $n$ estimates $K(s_{t+1}) + \sigma_\zeta \cdot \zeta_i$ with additive Gaussian noise $\zeta_i \sim \mathcal{N}(0,1)$, until becoming sufficiently certain about $K(s_{t+1})$.[16] This yields posterior log-odds updates similar to the above:

$$\log\left(\frac{b(\theta \mid S_{t+1})}{b(-\theta \mid S_{t+1})}\right) = \log\left(\frac{b(\theta \mid S_t)}{b(-\theta \mid S_t)}\right) + \beta K(s_{t+1}) + \beta\frac{\sigma_\zeta}{\sqrt{n}}\zeta_i,$$

with $\beta = \dfrac{\sigma^2}{\sigma_\zeta^2/n + \sigma^2}$. It is then possible to characterize the agent's expected posterior belief by a probability weighting function such that $b(\theta \mid \cdot) = f(P(\theta \mid \cdot))$, with underinference being intrinsically associated to the agent's cognitive uncertainty.

As a prelude to our analysis of mechanisms later, we highlight some testable predictions that emerge. Suppose we find that $\beta$ decreases in the above equation. According to this model, this could be generated by (a) a decrease in $n$, (b) an increase in $\sigma_\zeta$, or (c) a decrease in $\sigma$. A standard approach in the literature associates $n$ with the decisionmaker's response time, the idea being that the decisionmaker obtains one such signal per unit of time spent deliberating.[17] This rationalizes the general finding that decisionmakers take more time on simple tasks when these tasks become less immediately apparent.[18,19]

### 2.2. Unlearning: Testing that Retractions are Different

We now turn to updating *from retractions*, and highlight some subtleties that emerge in pursuit of our goal of showing that retractions are treated fundamentally differently from other information. We begin with a formal definition:

**Definition 2.** *Consider the data generating process described in (1). A retraction at time $t$ consists in informing the decisionmaker that $n_\tau = 1$, thus implying the observation $s_\tau$ was noise.*

In order to update beliefs as a Bayesian following a retraction, the decisionmaker must know how the retraction is generated, that is, how $\tau$ was chosen. We consider the following:

[16] Since the purpose of this formalism is to fix ideas about existing concepts and relate cognitive uncertainty with "quasi-Bayesian" updating, we purposefully leave the threshold for 'sufficient certainty' as exogenous.

[17] For discussions of this point, see, e.g., Krajbich et al. (2010).

[18] Early versions of these results can be found in, for instance, Banks et al. (1976), Buckley and Gillman (1974), or Ratcliff (1978); to our knowledge, scientific consensus accepts the basic finding.

[19] We note that to conclude that updating is harder at a given history, it is necessary to compare jointly how $\beta$ (responsiveness to beliefs), $n$ (reaction time), and $\frac{\sigma_\zeta}{\sqrt{n}}$ (belief variance). At a given history—so that $\sigma$ is fixed—in order to rationalize (a) *increase* in $n$ given (b) a decrease in $\beta$, it must also be the case that (c) $\sigma_\zeta/\sqrt{n}$ increases.

**Definition 3.** *A* verifying retraction *is a retraction in which* $\{\tau = \ell\}$ *is independent from other observations' truth value,* $\ell \leq t$.

In our experiment, this is implemented by selecting $\tau$ uniformly at random from $\{1, ..., t\}$ and subsequently revealing $n_\tau$ to the decisionmaker; that is, whether this observation is noise or not.[20] Note that a Bayesian decisionmaker should be able to follow Bayes rule and update beliefs following retractions without any ambiguity.[21]

The following result relates the following three quantities, our main comparisons in the paper:

(1) $b(\theta|S_t, n_\tau = 1)$, the decisionmaker's belief after observing the retraction $n_\tau = 1$;
(2) $b(\theta|S_t \setminus s_\tau)$, the decisionmaker's belief had the retracted observation $s_\tau$ never been observed.
(3) $b(\theta|S_t \cup s_{t+1})$, the decisionmaker's belief after observing a new observation $s_{t+1}$ instead of the retraction

**Proposition 1.** *Suppose retractions are verifying. For any quasi-Bayesian updating rule with a fixed (initial-belief independent) $f$, as described in Definition 1, (1) and (2) are identical. Moreover, given any (possibly initial-belief dependent) quasi-Bayesian updating rule $f$, (1) and (3) are identical if and only if its loglikelihood is negative of the retracted observation, $K(s_{t+1}) = -K(s_\tau)$.*

The proof of this proposition essentially follows from a careful application of Bayes rule and observing that quasi-Bayesian updating rules still satisfy this identity under the transformation $f^{-1}$. In fact, an identical argument could be used to introduce additional history dependence into the updating rule; our identification strategy below would remain valid. The key point is that, while one may wish to entertain a variety of models to accommodate a plethora of biases, any differences between (1) and (2) or (3) in our experimental setup will require retractions to be treated as intrinsically different.

We emphasize, however, that the assumption that the retractions are verifying is important and the result is not generally true without it: unless retractions are explicitly verifying, a Bayesian decisionmaker typically *would* in general update from them differently, even when observations are independent and identically distributed conditional on the state. Our formulation corresponds to situations where the verification does not condition on the outcome; for instance, asking a

[20] Note that this implies that when $n_\tau = 0$, the decisionmaker learns their past information was actually true and, in the current setting, this would result in degenerate Bayesian posterior beliefs.

[21] This lack of ambiguity distinguishes our experiment from Liang (2020), Shishkin and Ortoleva (2021), and Epstein and Halevy (2020).

defendant to testify about a past claim under oath, fact-checking a randomly selected collection of claims from politicians, or an independent replication of the analysis in a scientific study (assuming the outcomes of such replications were not themselves selected). By contrast, if information about past evidence is disclosed only when the evidence is found to be uninformative of the state—as occurs in the retraction of academic papers or with fact-checkers targeting misinformation—then the retraction of a piece of evidence would give more credence to *non-retracted* evidence.[22]

We focus on verifying retractions to reduce the potential confounds in the design, so that Proposition 1 holds. Note that the principle of restricted choice (see Section 1.1) clarifies why Monty Hall issues (a) could be relevant for non-verifying retractions, but (b) not for verifying retractions. With verifying retractions, any observation is targeted for "retraction" independently of its truth value and a retraction asserts the noise value of the retracted observation, but provides no additional information about the truth value of *other* observations. For non-verifying retractions—if, for instance, only noise observations are targeted—these additional restrictions on which observations are retracted can be, in general, meaningful.[23] And indeed, Proposition 1 is no longer true if retractions provide information related to how other evidence was generated. While these issues are certainly relevant in a number of circumstances, we deliberately preclude this phenomenon to make updating from retractions not only as simple as possible, but especially to make it equivalent to deleting retracted evidence and nothing more. Additionally, this discussion suggests that verifying retractions should be the simplest case for subjects, and thus are the natural starting point.

Our hypothesis will be that, when the updating task is harder, subjects experience higher cognitive uncertainty about how to update beliefs, resulting beliefs being less affected by information on average (i.e. smaller expected changes in beliefs $b(\theta|S_{t+1}) - b(\theta|S_t)$), and higher variance in belief updates.[24] This association between noise in belief updating, decision time, and task difficulty has been noted before and is in line with the literatures on cognitive uncertainty, efficient coding, and sequential sampling models, and their empirical findings (see, e.g. Frydman and Jin, 2022; Frydman and Nunnari, 2022). We return to these predictions in the following section, when

[22]In ongoing research we also examine a version of this experiment using targeted (i.e., non-verifying) retractions; the results are largely consistent, although direct comparisons between the two are unwarranted, as in this case it is not in general true that $P(\theta \mid S_t, n_\tau = 1) = P(\theta \mid S_t \setminus s_\tau) = P(\theta \mid S_t \cup -s_\tau)$. These results are available from the authors upon request.

[23]Similarly to the information provided in the Monty Hall problem, since only doors with goats can be revealed.

[24]For instance, taking the number of estimates $n$ positively related to time and as increasing in prior variance $\sigma^2$ and estimate variance $\sigma^2_\zeta$, there are conditions under which our illustrative model exhibits these traits.

discussing our hypotheses.

To summarize, updating from retractions in our setup is made as simple as possible. Over a broad class of belief updating rules—including Bayesian updating and generalizations common in the literature—a retraction is equivalent both to deleting the retracted observation *and* to receiving a new observation; this does not depend on which observations were observed, nor does it require any information on past data. Furthermore, as we detail in Section 4, our experimental design emphasizes this simplicity by also removing other sources of complications:

1. The prior about the state and the noise are both symmetric ($P(\theta = 1) = P(\epsilon_t = 1) = 1/2$).
2. Observations are independent and identically distributed conditional on the state and the log-likelihood of their realizations is symmetric around zero ($K(s_t) = -K(-s_t)$) and therefore retracting $s_\tau$ is equivalent to observing an additional opposite observation $s_{t+1} = -s_\tau$, a necessary and sufficient condition for such equivalence as per Proposition 1.
3. The details of the data-generating process are graphically described in an intuitive manner and both these and full history of signals (observations and retractions) are always visible to subjects.
4. The decisionmaker observes a small number of observations (up to four).

## 3. HYPOTHESES

The goal of the paper is to study updating from retractions and to compare it to updating from new observations. Our first hypothesis concerns under updating from retractions. Part (a), a formulation of the continued influence effect, concerns the failure of retractions to correct beliefs, while part (b) compares the influence of retractions to that of equivalent new information:

**Hypothesis 1** (Retractions are Ineffective)**.** *Subjects (a) fail to fully internalize retractions, and (b) treat retractions as less informative than an otherwise equivalent piece of new information.*

We emphasize that the use of the term "retractions" in this hypothesis reflects the meaning in Definition 2, with "otherwise equivalent" reflecting the last case of Proposition 1. Thus, relative to the work on retractions surveyed above, this hypothesis conjectures that retraction failures can emerge solely as a (specific) departure from Bayesian updating and not due to context-specific elements.

Note that while (a) and (b) both reflect retractions being less effective, and that one conclusion may be *suggestive* of the other, they are ultimately distinct. In principle, both new observations and retractions could be treated as equivalent and less informative than an earlier observation, leading to (a) without (b). Conversely, new observations and retractions could be treated differently, but with retractions being internalized fully and a distinct departure from Bayesian updating yielding overreactions to new observations, leading to (b) without (a).

Our next hypotheses concern our proposed explanation for why retractions are less effective than new observations: retractions are harder to process. That is, we conjecture that an extra layer of complexity is introduced for retractions, as subjects must consider what a retraction implies about past information; since new observations (without retractions) are exchangeable, this step is not required when learning from new observations alone. To test it, in line with our discussion in the previous section, we consider two proxies for increased cognitive complexity: longer decision times and greater belief report variance. We conjecture that both will reflect the additional complexity inherent to this kind of conditional reasoning:

**Hypothesis 2** (Retractions are Harder)**.** *Processing retractions is more difficult than processing new observations, resulting in longer decision time and greater belief variance.*

We also exploit the dynamic information arrival in our experimental design to test whether intuitively 'harder' retractions are less effective and similarly increase decision time and belief variance. In particular, when earlier observations are retracted, there is a layer of added complexity in belief updating: 'unlearning' them entails forming beliefs about a dataset not previously observed. In contrast, retracting the most recent observation only requires returning to the belief held prior to that observation, $b(\theta \mid S_t, \text{Retraction of } s_t) = b(\theta \mid S_{t-1})$. Thus, insofar as retraction failure is tied to their inherent complexity, retractions may lead to more (less) effective unlearning of past information when the information is (not) immediately retracted. This motivates the following hypothesis:

**Hypothesis 3** (Harder Retractions are Harder)**.** *Retractions of less recent observations are (a) less effective, and (b) result in longer decision time and greater belief variance.*

If a retraction is harder to process, then it is plausible that it is also harder to update from new observations following a retraction. This constitutes another expression of our proposed mechanism, which we articulate as a related hypothesis:

**Hypothesis 4** (Updating after Retractions)**.** *Processing new observations after retractions results is more difficult, resulting in (a) subjects updating less from new observations, and in (b) longer decision time and greater belief variance.*

We conclude by considering alternative explanations for retraction ineffectiveness. Our design deliberately shuts off common explanations for the continued influence effect—e.g. imperfect memory, motivated reasoning, complex narratives, reliability of the source of retractions—and our theoretical framework shows that retractions being less effective *requires* them to be treated differently from new observations. However, being treated differently does not necessarily imply that the same biases in belief updating are not present. We then consider if retraction ineffectiveness is simply an expression of well-known biases:

**Hypothesis 5** (Similar Updating Biases)**.** *The same biases in belief updating from new observations are present in updating from retractions.*

Lastly, we consider a further alternative explanation for retraction ineffectiveness: that retractions are less effective simply because it is difficult to disregard evidence that has been acted on or engaged with. In contrast to an inherent greater complexity of retractions, this alternative mechanism relies on a form of cognitive dissonance or an endowment effect applied to information, suggesting the following hypothesis:

**Hypothesis 6** (Retracting Used Evidence)**.** *Retractions are ineffective only when individuals have acted on the retracted observations.*

Taken together, our hypotheses posit a diminished effectiveness of retractions, address several implications of our proposed mechanism—that retractions are harder to process—and consider two alternative mechanisms that could underlie this outcome.

## 4. EXPERIMENTAL DESIGN

In this section, we describe the overall experimental design—which is summarized visually in Figure 1[25]—and then we provide details on the experimental interface and protocols. As our goal is to directly test the theoretical framework in Section 2, the basic data generating process matches the theoretical framework presented there; subjects were provided full information regarding how observations would be drawn and how performance-based compensation would be provided.

[25]We note that these figures include screenshots from the experimental interface; this can also be seen in Appendix C.

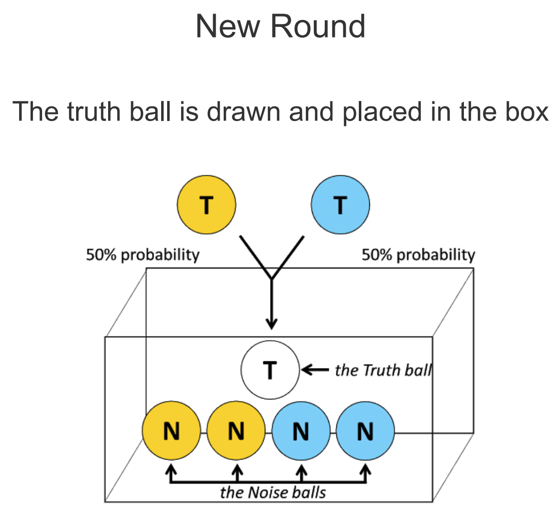
### 4.1. Basic Design

We first describe one round of the basic experimental design. Each *round* of the experiment has up to four *periods*, with beliefs elicited at the end of each period. Each subject plays a total of 32 rounds, and no feedback on performance is provided until the end of the experiment, when performance-based payouts are made. In each round, the sequence of events is as follows:

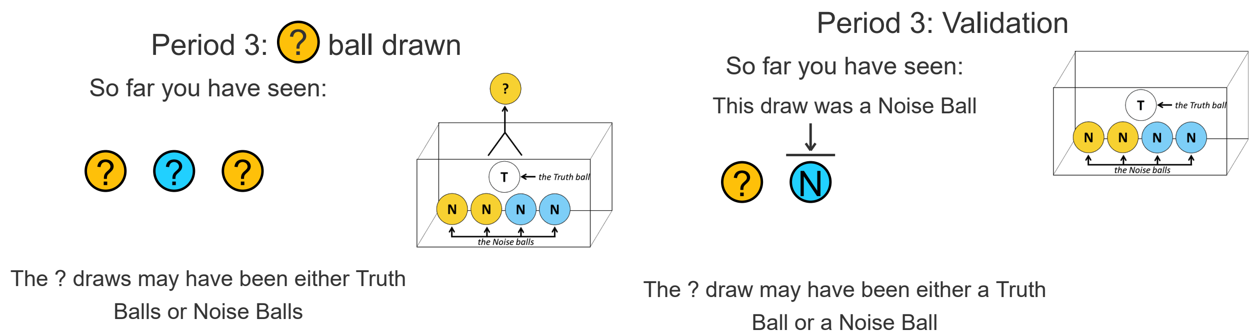
1. At the start of the round, a *truth ball* (referring to the state $\theta$) is chosen at random to be either yellow or blue, with equal probability. The truth ball is then placed into the box with four *noise balls*, two yellow and two blue (corresponding to $P(n_t = 1) = 1/5$ and $P(\epsilon_t = 1) = 1/2$ in the information arrival process described in Section 2).
2. In periods one and two, subjects obtain a *new observation*: a draw from the box, with replacement. They are told the color of the ball but not whether it is the truth ball or a noise ball.
3. In periods three and four, and independently across periods, subjects either obtain a new observation (as above), with probability 1/2, or they observe a verification of an earlier observation from the same round, with complementary probability. Under a verification, one of the prior draws is chosen at random and it is revealed whether it was a noise ball—a *retraction*—or the truth ball. If the draw is revealed to have been the truth ball, the round ends, as at that point the state (the color of the truth ball) is fully revealed.

Additionally, at the end of each period—that is, after each new signal (observation or retraction)—subjects report their belief regarding the probability that the truth ball is blue vs. yellow. These reports are incentivized, as detailed later in the section.

As explained in detail in Section 5.1, this simple design allows us to identify the effect of retractions on belief updating. Comparing beliefs after a given observation is retracted to beliefs in histories in which the retracted draw was not made to begin with allows us to test whether retractions are effective (Hypothesis 1a). Comparing changes in beliefs in response to retractions with changes in beliefs in response to equivalent new ball draws allows us to test whether learning from retractions is different from learning from new observations (Hypothesis 1b). Indeed, a key aspect of our design is that a new draw of one color is informationally equivalent to a retraction of the opposite color (for a (quasi-)Bayesian, i.e., a general class of models allowing for deviations from Bayesian updating, see Definition 1)—what matters for updating from the prior is the difference in




(a) *Determining the state.* At the beginning of each round, a truth ball was selected at random, with equal probability of being yellow or blue, and placed into a box with four noise balls, two yellow and two blue.




(b) *Ball draws and retractions.* Rounds consisted of (up to) four periods, each of which consisted of either a new draw or a verification, followed by the elicitation of subjects' beliefs over the color of the truth ball. For a new draw (left), a ball was drawn from the box (with replacement), and subjects were told its color but not whether it was the truth ball or a noise ball. For a verification (right), an earlier draw was chosen at random, and subjects were told whether that ball was a noise ball (a retraction) or the truth ball. If it was the truth ball, the round ended. The history of the round was displayed throughout.

Figure 1: Summary of Experimental Visuals

the number of balls of each color; see Proposition 1. In turn, making these comparisons across different histories tests *when* retractions are more or less ineffective (Hypotheses 1a, 5), and test whether retractions affect subsequent updating (Hypothesis 4).

### 4.2. Single-Elicitation Treatment

Our experiment features a between-subject treatment. At the start of the experiment, each subject is randomly allocated to one of two treatments. With 1/2 probability they are allocated to the *baseline treatment*, as described above. With 1/2 probability, they are allocated to the *single-elicitation treatment*, whose purpose is to test whether requiring subjects to report their beliefs in *every* period—and hence to act on draws before they are retracted—affects the efficacy of retractions (Hypothesis 6).

In the single-elicitation treatment, the sequence of events is the same as in the baseline treatment, except for two differences: (1) beliefs are only elicited at the end of each round, rather than each period; (2) with probability 1/3, the round ends in period two; with probability 2/3, the round ends in period three. The design ensures that while we do not observe the *entire* belief path, we are nevertheless able to form estimates for beliefs after two draws, as well as beliefs after three draws when the third draw is either a retraction or a new observation.

### 4.3. Implementation Details

**Experimental Interface.** A summary of the explanatory visuals shown to subjects is given in Figure 1 and the full instructions of the experiment can be found in Online Appendix C. Beliefs were reported using a slider, which displayed both the probability they assign to the truth ball being yellow, as well as the probability they assign to the truth ball being blue. Immediately after the instructions, subjects were given two rounds of unincentivized "practice" to familiarize themselves with the interface.

**Subject Pool and Comprehension Checks.** The experiment was run on Amazon Mechanical Turk (henceforth MTurk) on June 16-18, 2020. In order to ensure adequate statistical power, we targeted 200 subjects per treatment group. We recruited a total of 415 subjects, 211 subjects for our baseline setup and 204 for the single elicitation treatment.

We took four main steps in order to ensure that our subject pool was of high quality. First, we included Captchas throughout the experiment in order to filter out bots. Second, we included comprehension questions in the instructions which subjects needed to answer correctly in order to proceed with the experiment.[26] The questions summarized the key points the subjects needed to

[26]See Online Appendix C for the instructions as presented to the subjects in the experiment.

understand, and would have been very difficult to answer correctly without having understood the instructions. While unincentivized, the majority of the subjects answered all questions correctly on the first try (55%), and 90% answered correctly by the second try—with uniform random guesses, the probability of answering all correctly on first try would be lower than 1%. Third, as detailed below, we used a payment scheme which involved a high baseline and reward pay. Fourth, we restricted our study to be held only during business hours (Eastern Standard Time), and we restricted eligibility to US adults and precluded the possibility of repeating the experiment.

These quality checks were important for us to be able to meaningfully test our hypotheses. Excessively noisy answers would have attenuated our results: while a subject answering 50-50 to everything would not be a Bayesian, they would also demonstrate no differential updating from retractions. It was also important that subjects understood retraction should *not* be treated as evidence for the opposite state. Misinterpreting the instructions in this way would suggest retractions should be treated as *more* informative than new information, again working against us finding evidence for our hypothesis.

Consistent with our quality controls being largely effective, our results are robust to multiple sample restrictions. For example, our results hold if we exclude those who appear to be answering randomly or inconsistently, or those who did not answer the comprehension test questions correctly on their first (or second) attempts—see Section 7.1 for details.

**Payments.** We incentivized subjects to report their beliefs truthfully using a binarized scoring rule (see Hossain and Okui (2013) and Mobius et al. (2013)). By reporting $b \in [0, 100]$, a subject would receive \$12 with probability $(1 - (\mathbf{1}\{\theta = 1\} - b/100)^2)$ and \$6 with complementary probability, where $\theta$ equals 1 (-1) when the truth ball is yellow (blue). In the instructions—but not in the main interface—we provided information on the elicitation procedure, phrased as eliciting the probability the truth ball was either yellow or blue, and explained that the procedure was meant to ensure they were incentivized to answer truthfully. To determine payments, we used a report from a single randomly selected period of a randomly selected round. We also asked additional questions on mathematical ability, which were incentivized by providing a \$0.50 reward if they answered correctly a randomly chosen question.

The average compensation was of \$20.02/hour, with subjects spending on average 29 minutes in the experiment. For comparison, this rate is similar to the MTurk experiment of Enke and Graeber (2020), and four times the MTurk average of \$5.

### 4.4. Preregistration

Our experiment was registered using the AEA RCT Registry under RCT ID AEARCTR-0003820. The experimental design and recruitment targets were pre-registered, as were our Hypotheses 1, 3a, 4a, 5, and 6. The hypotheses pertaining to response time and belief variance (2 and its variations, 3b and 4b) were introduced subsequently, as feedback we received convinced us they provided evidence for our proposed mechanism.

## 5. IDENTIFYING UNLEARNING FAILURES

We divide our empirical results into two parts. In this section, we demonstrate that retractions fail to correct beliefs and are less effective than providing new evidence. By relying on a setup in which context-specific explanations discussed above are precluded, we provide the first identification of this failure as a general feature of belief updating. In Section 6, we consider why retractions are less effective and present evidence for our proposed explanation (and against some alternatives).

We begin by explaining our empirical strategy in Section 5.1. In Section 5.2, we validate our experimental setting by showing that updating from new observations—*learning*—is similar to that found in the existing literature. Then, in Section 5.3, we turn to the main topic of the paper, updating from retractions—*unlearning*. We ask do retractions work, and do people update differently from retractions versus new observations? (Hypotheses 1a and 1b)

### 5.1. Empirical Strategy

There are two distinct empirical tasks: identifying the effectiveness of unlearning (vs. learning) for a given history; and aggregating the results across different histories. For both, we lean on the simplicity of our experimental design to make the analysis non-parametric when possible.

#### 5.1.1. Identifying Learning versus Unlearning

To test the effectiveness of unlearning and to compare it to learning, we perform two distinct comparisons throughout our analysis, corresponding to parts (a) and (b) of Hypothesis 1 and explained visually in Figure 2:

(a) *Testing unlearning*: Are subjects' beliefs after seeing a retraction the same as if the retracted observation had never been observed in the first place?

$$b(\theta \mid \text{Observations, Retraction of Observation } s_\tau) = b(\theta \mid \text{Observations} \setminus \text{Observation } s_\tau)$$

(b) *Comparing unlearning to learning*: Do subjects update equally from retractions as from equivalent (in terms of Bayesian belief updates) new information?

$$b(\theta \mid \text{Observations, Retraction of Observation } s_\tau) = b(\theta \mid \text{Observations} \cup \text{New Observation } -s_\tau)$$

To outline the regressions for these basic tests, we introduce some notation. Denote by $b$ the subject's beliefs—the probability they assign to the truth ball being yellow—and by $s$ the signal in question. We treat signals as $+1$ if they favor the belief that the truth ball is yellow—new draws of a yellow ball or retractions of a blue ball—and $-1$ if they favor it being blue.[27] Finally, denote by $r$ a dummy variable indicating whether the signal is a retraction ($r = 1$) or a new observation ($r = 0$).

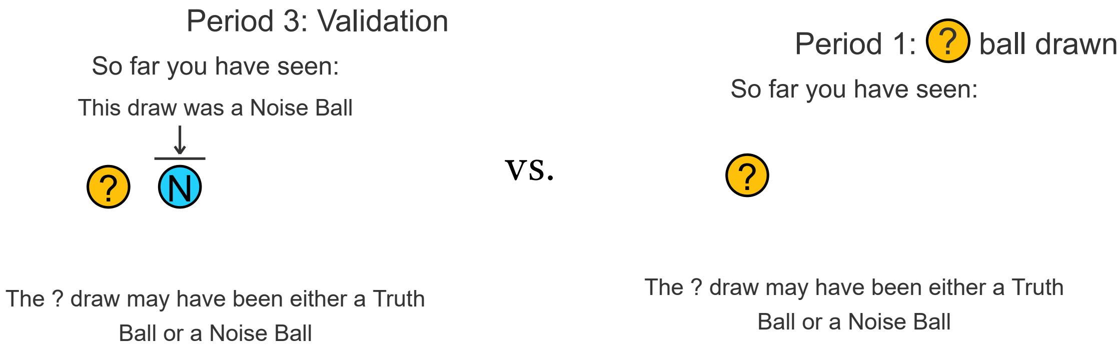
With this notation in hand, for a specific history, we can perform both tests (a) and (b) with the regression:

$$b = \beta_0 + \beta_1 \cdot r \cdot s. \tag{4}$$

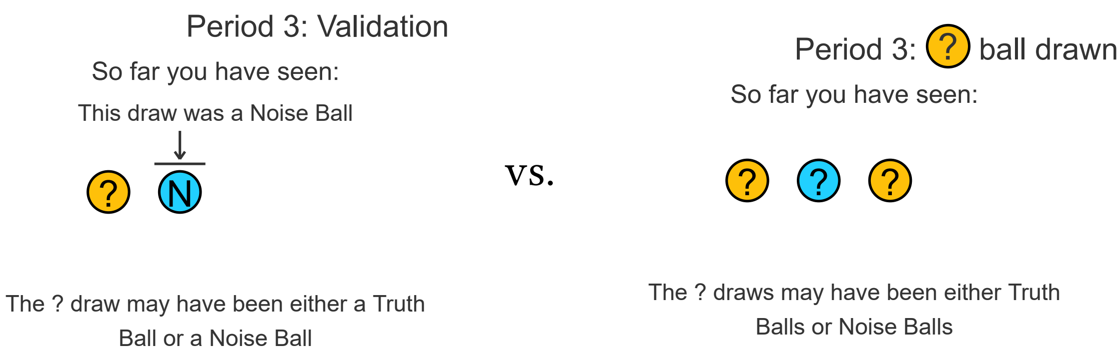
The sample for test (a) comprises beliefs after the retraction as well as when the retracted observation had not been observed to begin with, while for test (b) it comprises beliefs after the retraction and beliefs after a new observation of the opposite sign. The coefficient of interest for both tests is $\beta_1$. Under test (a), if $\beta_1$ is zero, retractions work: beliefs are as if the retracted signal was never seen; if it is negative, retracted signals continue to influence beliefs. Under test (b), if $\beta_1$ is negative, beliefs move less in response to retractions than to equivalent new signals. To give concrete examples, as illustrated in Figure 2, test (a) would compare beliefs in period 3 having observed *(yellow, blue, retraction of the blue)*, to those in period 1 having just observed *(yellow)*; while test (b) would compare beliefs in period 3 having observed *(yellow, blue, retraction of the blue)*, to those in period 3 having observed *(yellow, blue, yellow)*.

When analyzing tests of unlearning, test (a), we compare belief reports in levels, while for test (b) we compare effects on beliefs in both levels and changes (first differences), since the test is specifically about how beliefs *change* in response to retractions. We use beliefs as reported by subjects, on a linear scale (0 to 100), except when we analyze biases in belief updating in Sections 5.2 and 6.2.1, where we use the log-odds scale to be consistent with existing literature. Levels

[27] To be precise, if an earlier signal of value $v$ is retracted, then $s = -v$




(a) *Do retractions work?* We compare beliefs after a retraction in period $t$ ($\in \{3, 4\}$) to beliefs after an (equivalent) "compressed history" in period $t - 2$; that is, the history with the retracted ball removed. Thus, in the illustrated example, beliefs after the retraction in period 3 are compared to beliefs in period 1 when there has only been a yellow draw.




(b) *Are retractions treated differently from equivalent new observations?* We compare beliefs after a retraction in period $t$ ($\in \{3, 4\}$) to beliefs after an equivalent new observation (of opposite color to the draw which was retracted), also in period $t$. Thus, in the illustrated example, beliefs after the retraction of the blue ball in period 3 are compared to beliefs in period 3 when the history through period 2 is the same but a yellow ball is drawn in period 3.

Figure 2: Illustrative examples to explain the empirical strategy

has the advantage that extreme beliefs, near 0 or 100, are not overly inflated; log-odds has the advantage that the experimental signals should lead to a constant change in the log-odds belief, independent of the prior. As we show in Online Appendix B.4.2, our conclusions are robust to relying exclusively on log-odds.

### 5.1.2. Aggregating Results Across Histories, Using Fixed Effects

While we report results disaggregated by case, showing that they are qualitatively consistent across histories, the results are simpler to digest when aggregated. We do so by pooling the sample across histories in suitably modified versions of the above regressions. The basic identification concern in pooling across histories is using identifying variation which compares updating from retractions in one history to updating from new signals in a different history, especially given the well documented heterogeneity in updating from new signals across histories.

We ensure that we are only identifying off within-history variation by using appropriately defined fixed effects. To explain them, denote by $H_t$ the history up to and including period $t$, that is, the set of all the draws observed as well as the retractions, fixing the order. For the tests of unlearning, (a), we use fixed effects for what we refer to as a *compressed history*, $C(H_t)$: the history, removing any retracted ball draws as if they had never occurred to begin with, keeping the order fixed. For instance, a history of *(yellow, blue, retraction of the blue)* would be equivalent to *(yellow)*.[28] For the comparisons to new information, test (b), we include fixed effects at the level of the *sign history*, $S(H_t)$, which is the history without distinguishing whether signals were new observations or retractions. For example, *(blue, yellow, retraction of the blue)* is equivalent to *(blue, yellow, yellow)*. Once we include these fixed effects in the pooled regression, if there have not been retractions in previous periods, then we compare the a retraction of the ball of one color to the informationally equivalent new observation of the opposite color, conditional on what happened in all previous periods of the round.

### 5.2. Learning: Updating from New Observations

As a first step in our analysis, in part as a test of validity of experimental setting, we examine the belief paths of subjects when they are not shown retractions using the same empirical approach as previous papers in the literature.

In the absence of a retraction, the design is very similar to many others surveyed by Benjamin (2019) and subjects seem to correctly understand the setting, as reported beliefs track Bayesian posteriors closely.[29] We show that the results are largely consistent with the main findings from the literature, suggesting that any differences in our subsequent analysis can indeed be attributed to distinct features of retractions. In Table 1, we present Grether-style (Grether, 1980) log-odds regressions—a workhorse model of analysis in this literature—enabling a direct comparison to

[28]Note that compressed histories do not distinguish between the retracted observation having been drawn in period 1 or period 2. For example, both *(yellow, blue, retraction of the blue)* and *(blue, yellow, retraction of the blue)* have the same compressed history, *(yellow)*.

[29]In Online Appendix B.2 we show reported beliefs and Bayesian posteriors disaggregated by history (Figure 6) as well as the distance between them (Figure 7).

| | (1) | (2) |
|---|---|---|
| | $l_t$ | $l_t$ |
| Prior ($l_{t-1}$) | $0.834^{***}$ | $0.800^{***}$ |
| | (0.037) | (0.037) |
| Signal ($s_t$) | $1.126^{***}$ | $0.998^{***}$ |
| | (0.071) | (0.072) |
| Signal Confirms Prior ($s_t \cdot c_t$) | – | $0.417^{***}$ |
| | | (0.135) |
| R-Squared | 0.41 | 0.41 |
| Observations | 18491 | 18491 |

Clustered standard errors at the subject level in parentheses.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 1: Updating from New Observations

*Notes*: This table reports updating from new ball draws. The sample consists of all elicited beliefs of subjects in the baseline treatment (beliefs elicited each period), excluding those elicited after a verification, within a given round. The outcome is the log-odds of beliefs in period $t$, $l_t$. $s_t$ is the signal in round $t$ (+1 or -1, multiplied a constant factor of Bayesian updating such that the coefficient on $s_t$ would be 1 under Bayesian updating). $c_t := \mathbf{1}\{\text{sign}(l_{t-1}) = \text{sign}(s_t)\}$ denotes whether the signal at $t$ confirms the prior at $t-1$.

existing experimental results on belief updating. Specifically, Table 1 shows the following specification, restricted to the cases where there has not been a retraction (so only new observations):

$$l_t = \beta_0 + \beta_1 \cdot l_{t-1} + \beta_2 \cdot s_t \cdot K \tag{5a}$$

and

$$l_t = \beta_0 + \beta_1 \cdot l_{t-1} + \beta_2 \cdot s_t \cdot K + \beta_3 \cdot s_t \cdot K \cdot c_t \tag{5b}$$

where $t$ is the period, $l_t$ is the log-odds of the beliefs reported at $t$,[30] $s_t$ is the signal in round $t$ (+1 or -1), $c_t := \mathbf{1}\{\text{sign}(l_{t-1}) = \text{sign}(s_t)\}$ is an indicator function that equals 1 when the signal at $t$ confirms the prior at $t-1$, and $K > 0$ is a constant factor of Bayesian updating.

The usefulness of using a log-odds framework in this setup is that for a Bayesian decisionmaker log-odds beliefs move by a constant amount, depending only on the likelihood of the observation. Hence, in the above regression, a Bayesian decisionmaker would exhibit a coefficient $\beta_2 = 1$. Benjamin (2019) notes that this tends not to be the case: for the two incentivized studies with sequential observations he reviews, the estimated coefficient is $.528$. Thaler (2021) provides evidence that subjects overinfer (resp. underinfer) from signals in similar symmetric environments

[30] All tables involving log-odds of beliefs treat $b_t = 100$ and $b_t = 0$ respectively as $b_t = 100 - \delta$ and $b_t = \delta$. We chose $\delta = 0.1$ so as to avoid biasing the regression with extreme outliers. The results are robust to varying $\delta$ and to dropping subjects that answer $b_t \in \{0, 100\}$.

whenever $P(s_t = \theta \mid \theta) \geq 1/2$ is below (resp. above) approximately 3/5, coinciding with our parameters in the experimental design.

In the most parsimonious of our specifications, we $\hat{\beta}_2 = 1.126$, indicating mild over-inference from new observations, although not statistically different from 1 ($p$-value = .0762). Once we include the effect of confirmatory information, the coefficient becomes virtually equal to 1 ($p$-value = .976), while $\beta_3 > 0$ ($p$-value$< .001$). This overinference from confirmatory information—that is, $\beta_2 + \beta_3 > 1$ ($p$-value $= .0018$)—has been previously documented (e.g. Charness and Dave, 2017). Together, this suggests our subjects slightly over-react to new observations but that this is mostly driven by confirmation bias: they update more from a signal when the belief movement is in the direction of their prior. We also verify another deviation from Bayesian updating identified in the literature: subjects exhibit base-rate neglect. In other words, they underweight the prior, as evidenced by $\beta_1 < 1$.

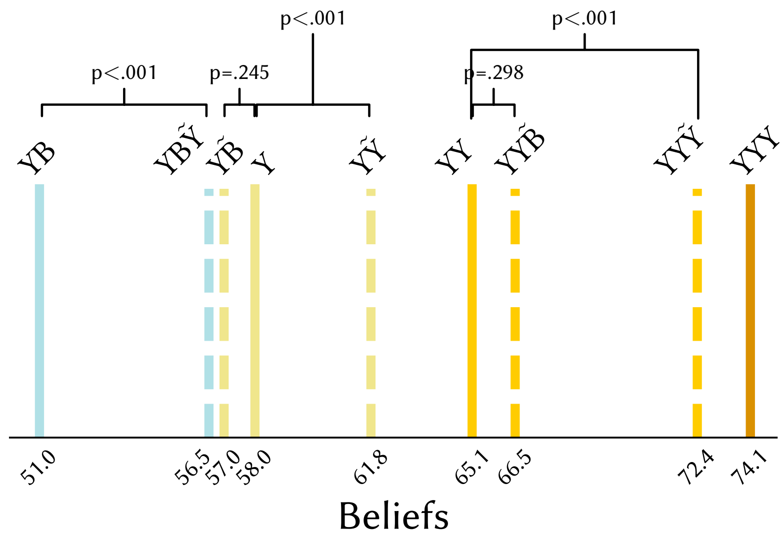
To summarize, in our analysis of this data, we do not find any significant departures from existing literature on belief updating.[31] It is helpful to keep these general patterns in mind below when interpreting our results; we emphasize that while subjects depart from Bayesian updating, they do so in a way consistent with what one would expect from the literature. Furthermore, since we find these biases in updating from new observations, any additional departure due to retractions cannot be attributed to explanations that are not specific to the nature of the information source.

### 5.3. Unlearning: Updating from Retractions (Hypothesis 1)

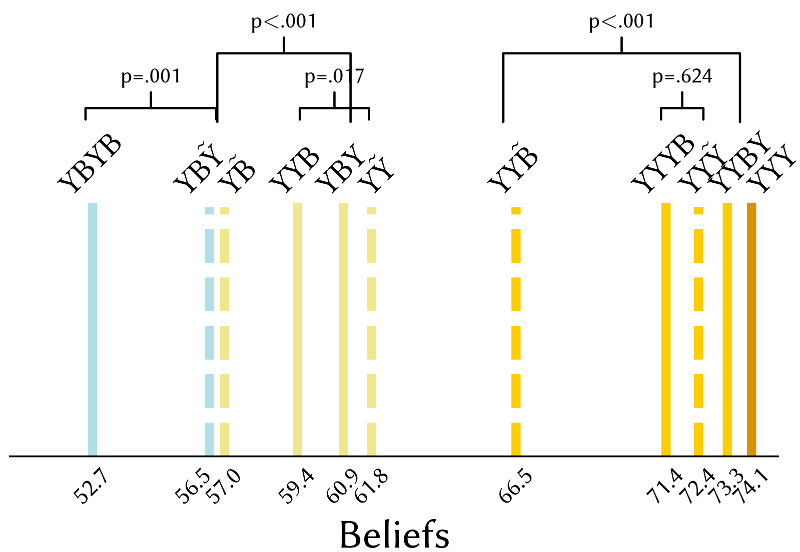
This section presents our first main findings, on the failure to fully "unlearn" from retractions and on the differences in belief updating from retractions as opposed to new observations.

Our first result, and the key finding of the paper, is the empirical support of Hypothesis 1: retractions are ineffective, in that (a) retracted observations are not fully disregarded (Prior vs. Retraction), and (b) beliefs are less responsive to retractions than to equivalent new signals (Retraction vs. New Draw). Figure 3 depicts mean beliefs across different histories and demonstrates both parts of the hypothesis. Figure 4 confirms this when looking at changes in beliefs: the change in beliefs following a retraction is smaller than the changes in beliefs both when (a) the subsequently-retracted observation was originally observed, and (b) when, instead of a retraction, subjects observe an equivalent new observation.

[31] In Online Appendix B.4.1, we reestimate the specifications in Table 1 using probability weights so as to render different histories equally likely. Not only do the conclusions remain unchanged, the estimates are extremely similar.



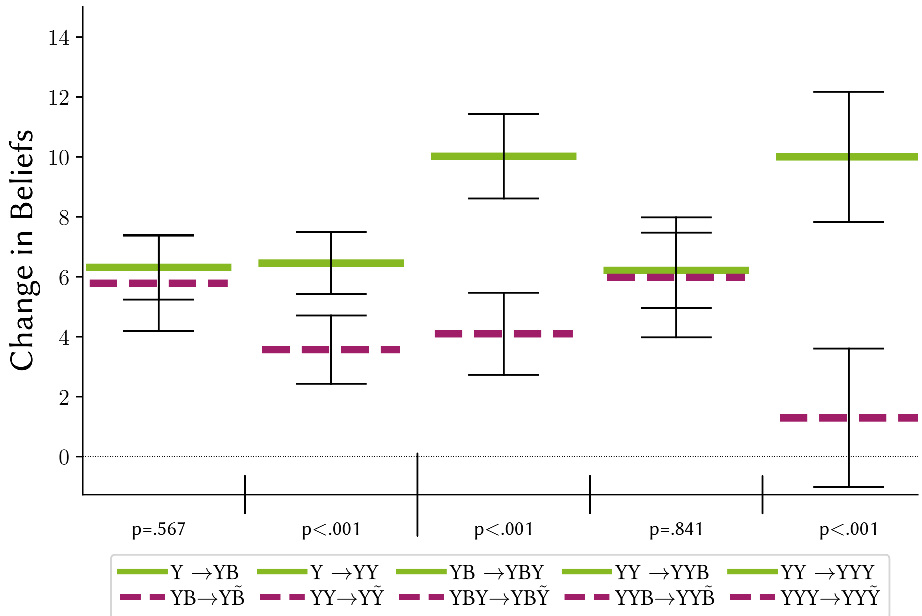

(a) Prior vs. Retraction



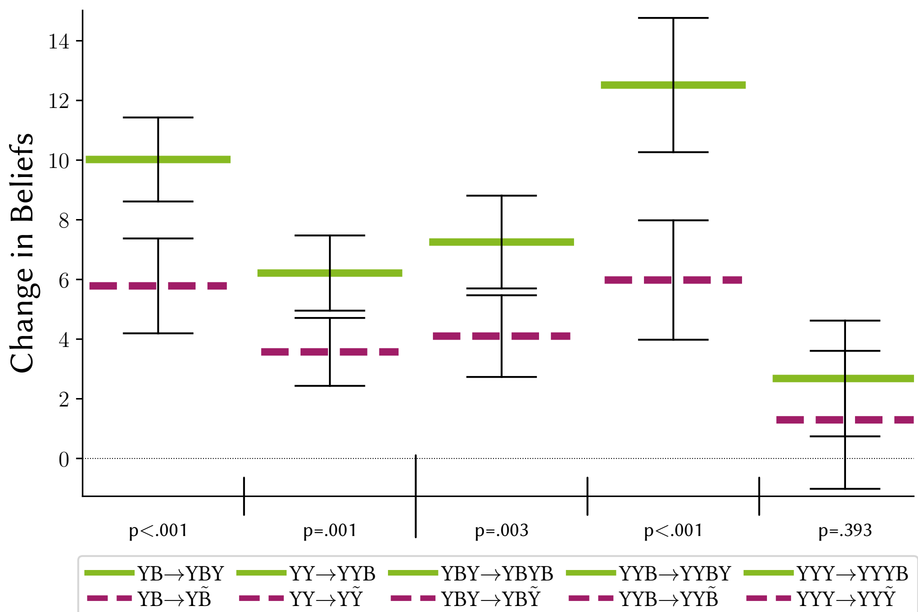

(b) New Draw vs. Retraction

Figure 3: Retractions are Ineffective: Beliefs (Hypothesis 1)

*Notes*: The figure displays mean reported beliefs, disaggregated by history, where a tilde denotes a retracted observation. Dashed lines indicate histories which end in retractions, solid lines those which do not. Lines of the same color correspond to histories inducing the same Bayesian posterior. Within a color, under Hypothesis 1, mean beliefs will lie to the right after a retracted yellow draw and to the left after a retracted blue draw. $p$-values for the specific tests are displayed and were obtained by a regression similar to columns (1) and (2) of Table 2 but restricted to the disaggregated histories, using standard errors clustered at the subject level. Belief reports are symmetrized around 50, e.g. $100 - b(B\tilde{Y})$ is treated as $b(Y\tilde{B})$, where a tilde denotes a retracted observation. The sample paths do not condition on sequence order: e.g. $Y\tilde{B}$ and $\tilde{B}Y$ are bundled together. The sample consists of subjects in the baseline treatment (beliefs elicited each period).




(a) Previous Draw vs. Retraction




(b) New Draw vs. Retraction

Figure 4: Retractions are Ineffective: Changes in Beliefs (Hypothesis 1)

*Notes*: The figure compares the change in beliefs following the retraction of an observation to (a) the change in beliefs when the observation was first drawn, and (b) the change in beliefs following an equivalent new observation. Belief reports are symmetrized around 50, e.g. $-(b(B\tilde{Y}) - b(BY))$ is treated as $(b(Y\tilde{B}) - b(YB))$; equivalently, we normalize the direction in which the updating should occur by considering $\Delta b_t \cdot s_t$. The sample paths do not condition on sequence order: e.g. $Y\tilde{B}$ and $\tilde{B}Y$ are bundled together. The sample consists of subjects in the baseline treatment (beliefs elicited each period). The whiskers denote 95% confidence intervals using standard errors clustered at the subject level; $p$-values were obtained by auxiliary regressions similar to column (3) of Table 2, but restricting to the disaggregated histories.

| | Prior vs. Retraction | New Draw vs. Retraction | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $b_t$ | $b_t$ | $\Delta b_t$ | $\Delta b_t$ |
| Retraction ($r_t$) | 0.201 | -0.167 | -0.351 | -0.242 |
| | (0.275) | (0.368) | (0.355) | (0.363) |
| Retracted Signal ($r_t \cdot s_t$) | -3.134$^{***}$ | -3.628$^{***}$ | -3.701$^{***}$ | -3.316$^{***}$ |
| | (0.601) | (0.726) | (0.670) | (0.675) |
| Signal ($s_t$) | – | – | – | 8.658$^{***}$ |
| | | | | (0.510) |
| Compressed History FEs | Yes | No | No | No |
| Sign History FEs | No | Yes | Yes | No |
| Lagged Sign History FEs | No | No | No | Yes |
| R-Squared | 0.34 | 0.34 | 0.18 | 0.15 |
| Observations | 22578 | 22578 | 22578 | 9074 |

Clustered standard errors at the subject level in parentheses.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 2: Retractions are Ineffective (Hypothesis 1)

*Notes*: This table tests whether retractions are effective in inducing 'un-learning' and compares their effectiveness relative to new direct information. The sample includes beliefs of subjects in the baseline treatment (beliefs elicited each period). Column (1) tests part (a) of the hypothesis, whether retractions work, by comparing beliefs after a retraction to beliefs after the equivalent compressed history. The outcome is the beliefs in period $t$, $b_t \in [0, 100]$. In the case of a retraction, $s_t$ is the opposite sign of the original observation being retracted in round $t$ (+1 if an earlier -1 signal is retracted, -1 if an earlier +1 signal is retracted). The regression includes fixed effects for the compressed history of draws. Columns (2) to (4) test part (b) of the hypothesis, whether people update less from retractions compared to equivalent new observations. The specifications include fixed effects for the sign history. In column (2), the outcome is the beliefs in period $t$, $b_t$. In columns (3) and (4), the outcome is the first difference in beliefs. Column (4) uses lagged sign history fixed effects to enable us to compare the magnitude of $r_t \cdot s_t$ to $s_t$, which is otherwise absorbed by the fixed effects. In columns (1)-(3), the sample excludes cases in which the truth ball is disclosed and in which there was a retraction in the past; column (4) further restricts to periods 3 and 4.

We pool these results across different histories in Table 2. Column (1) is a test of (a) and corresponds to the following regression:[32]

$$b_t = \beta_1 \cdot r_t \cdot s_t + \beta_2 \cdot r_t + F_{C(H_t)}, \tag{6a}$$

where we exclude from the sample cases in which the truth ball is disclosed or in which there

[32]The coefficient on $r$, which is added when we aggregate across histories, it has no meaning in itself. It identifies whether beliefs are on average shifted toward yellow when retractions occur, given the fixed effects. The coefficient will depend not only on how the realized frequency of blue and yellow observations compares to that of retractions, but also on which one is the base group for the fixed effects.

was a previous retraction. As explained in Section 5.1.2, controlling for compressed history fixed effects $F_{C(H_t)}$ compares, for example, the beliefs after observing $(s_1, s_2, n_2 = 1)$ to those reported when only observation $s_1$ was seen.

Columns (2)-(4) test (b) and correspond to variants of the following regression:

$$b_t = \beta_1 \cdot r_t \cdot s_t + \beta_2 \cdot r_t + \beta_3 \cdot s_t + F_{S(H_t)}, \tag{6b}$$

where we again exclude from the sample cases in which the truth ball is disclosed or in which there was a previous retraction. Controlling for sign history fixed effects, $F_{S(H_t)}$, means we compare for example beliefs reported after $(s_1, s_2, n_2 = 1)$ to those reported after $(s_1, s_2, s_3 = -s_2)$. In column (2) the dependent variable is belief levels, whereas in columns (3) and (4) it is *change* in beliefs. Column (4) uses less stringent fixed effects—those for *lagged* signed history $F_{S(H_{t-1})}$—so that the signal term $s_t$ is not absorbed by the fixed effects, but only includes periods 3 and 4. This enables us to benchmark the differential effect of retractions, $\beta_1$, by comparing it with the effect of new observations, $\beta_3$.

The key finding for both tests is that the differential effect of retractions on beliefs, $\beta_1$ the coefficient on $r_t \cdot s_t$, is negative and consistent in magnitude across all of the specifications we study. Retractions are treated *differently*, and in particular as if they were less informative than equivalent new observations. To quantify this effect, a simple comparison shows that beliefs move approximately one-third less when information is in the form of a retraction. This can be seen from column (4) of Table 2, by comparing the coefficient on the retracted signal—the interaction term between the signal and the retraction variables—to the coefficient on the signal variable itself. Performing this back-of-the-envelope calculation in other ways, for example by dividing the coefficient on $r_t \cdot s_t$ in column (3) by the average update from a new observation in the corresponding sample, consistently finds that beliefs update around $1/3$ from retractions relative to new observations.

## 6. WHY DO RETRACTIONS FAIL?

We now turn to the question of why retractions are not effective, and why they affect beliefs less than equivalent new observations. If the ineffectiveness of retractions in inducing unlearning was due to biases in updating from new observations, by Proposition 1, there would be no difference in updating from new observations or from retractions. Having demonstrated that, in fact, retractions are treated as less informative than an otherwise equivalent new observation (Hypothesis 1b), our starting point is the observation that any explanation should be germane to retractions themselves - established biases such as confirmation bias cannot explain our results. Here, we test falsifiable predictions of such models which thus could explain our results.

We divide our analysis of mechanisms into two parts. First, we provide evidence that retractions are fundamentally harder to process. Second, we discuss alternative mechanisms which could plausibly generate our results, and show that they do not.

### 6.1. Retractions are Harder to Process

We propose and find support for a mechanism which could explain why retractions are less effective: retractions are simply harder to process. Our design made retractions as simple as possible, rendering retracting a signal equivalent to the intuitive benchmark of deleting it, as well as to an opposite observation. Yet, despite the informational equivalence between learning and unlearning, new observations are simply information, while retractions are information *about* information (indicating how to interpret past observations). This qualitative difference implies that retractions necessitate conditional reasoning, which may make it harder to assess their informational content.

We explore three associated hypotheses, suggested by the theoretical model in Section 2, which posit that the diminished effectiveness of retractions is caused by a corresponding increase in the cognitive difficulty of updating (measured by $\sigma_\zeta^2$). First, we test whether retractions induce longer decision times and greater belief variance Hypothesis 2. Second, we argue that retractions of less recent observations are relatively harder to process. We therefore examine whether retractions of more recent observations (vs. less recent) differ in (a) effectiveness in inducing unlearning, and (b) resulting decision times and belief variance (Hypothesis 3). Third, we test whether *after retractions* subjects (a) update less from new observations, and (b) take longer and exhibit higher variance in belief reports (Hypothesis 4).

#### 6.1.1. Retractions, Longer Decision Times and Greater Belief Variance (Hypothesis 2)

The idea that retractions are more cognitively taxing motivates our hypothesis that they result in greater cognitive imprecision, as discussed in Section 2. To test this, we rely on two proxies for cognitive imprecision: decision times and variability in responses.

To test whether decision times are longer when subjects are faced with a retraction (Hypothesis

| | Prior vs. Retraction | | New Draw vs. Retraction | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $\log(\mathrm{dt}_t)$ | $\mathrm{V}(\mathrm{b}_t \mid h_t)$ | $\log(\mathrm{dt}_t)$ | $\mathrm{V}(\mathrm{b}_t \mid h_t)$ |
| Retraction ($r_t$) | 0.053*** | 128.1*** | 0.101*** | 71.7*** |
| | (0.016) | (20.300) | (0.015) | (22.119) |
| Mean Decision Time (secs) | 6.674 | – | 6.674 | – |
| Compressed History FEs | Yes | Yes | No | No |
| Sign History FEs | No | No | Yes | Yes |
| R-Squared | 0.01 | 0.02 | 0.02 | 0.02 |
| Observations | 22578 | 3030 | 22578 | 3030 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Retractions are Harder (Hypothesis 2)

*Notes*: This table tests if retractions induce longer decision times and greater belief variance. Columns (1) and (3) test if retractions induce longer decision times, measured in log-seconds (Hypothesis 2a). The sample includes decision times of subjects in the baseline treatment (beliefs elicited each period), excluding cases in which the truth ball is revealed and those in which there was a retraction in the past. Column (1) compares decision time after a retraction to decision time after the equivalent compressed history. Column (3) compares decision time after a retraction to decision time after an equivalent new draw. Columns (2) and (4) test whether retractions induce higher belief volatility (Hypothesis 2b). Column (2) compares belief variance following a retraction to belief variance at equivalent histories in which the retracted signal was not drawn; the dependent variable is the sample variance of beliefs of a given subject, conditional on permuted compressed histories and on whether a retraction occurred. Column (4) compares belief variance following a retraction to belief variance following an equivalent new draw; the dependent variable is the sample variance of beliefs of a given subject, conditional on permuted sign histories and on whether a retraction occurred.

2a), we use an identification strategy similar to the one used to test the effects of retractions on belief updating (Section 5.1). Specifically, in Table 3, we estimate two versions of

$$\log(dt_t) = \beta_1 \cdot r_t + F \tag{7a}$$

where $dt_t$ is the subjects' decision time in seconds. $F$ corresponds to either compressed history fixed effects or sign history fixed effects, as in columns (1) and (2)-(4) of Table 2 and under the same sample restrictions. In the first case, we compare decision times following a retraction with those at histories in which the retracted observation was never drawn. In the second, we compare decision times following a retraction versus an informationally equivalent new observation.

We pursue an analogous strategy to identify the effect of retractions on belief variance and test Hypothesis 2b. To test if retractions increase belief variance relative to histories in which the retracted observation was never drawn, we calculate the sample belief variance at the subject-level conditional on the compressed history and on whether a retraction was observed, $\text{Var}(b_t \mid C(H_t), r_t)$. We treat compressed histories that are the same up to permutations as the same compressed history so as be able to estimate within-subject belief variance at a given (permuted) compressed history. To we test whether belief variance following a retraction is greater than at informationally equivalent histories in which the retracted observation was never drawn, we estimate the following equation:

$$\text{Var}(b_t \mid C(H_t), r_t) = \beta_1 \cdot r_t + F_{C(H_t)}. \tag{7b}$$

Analogously, to compare belief variance following a retraction versus an equivalent new observation, we calculate the sample belief variance, for each subject, conditional on the sign history $S(H_t)$ (again, allowing for permutations) and on the occurrence of a retraction in period $t$, and estimate

$$\text{Var}(b_t \mid S(H_t), r_t) = \beta_1 \cdot r_t + F_{S(H_t)}. \tag{7c}$$

We note that, by conditioning on permutations of compressed/sign histories, we hold fixed Bayesian posteriors; i.e. permutations of a compressed/sign history are informationally equivalent. For notational simplicity, we write $V(b_t \mid h_t)$ to denote the subject-level sample variance conditional on (permuted) compressed/sign histories as described above.

The results, in Table 3, confirm both Hypotheses 2a and 2b. Specifically, column (1) shows that subjects take approximately 5% longer reporting beliefs following a retraction when compared to cases in which the retracted observation was never drawn. Comparing retractions with equivalent new draws, belief reporting following a retraction takes 10% longer (column (3)). Columns (2) and (4) provide an analogous comparison for belief variance estimated at the subject level, which retractions increase by over one third. In both cases, we see that belief variance increases following a retraction. In Appendix B.4.8, we provide an alternative test for whether variance of beliefs is higher for retractions than for new observations, whereby we disaggregate by (permuted) compressed and sign histories; the results are consistent. In Appendix B.4.7, we show results on response times remain valid when controlling for the round number and its interaction with retractions. While our results show subjects take less time in later rounds, the increase in decisions time caused by retractions remains consistent in later rounds, when subjects have had more experience observing retractions.[33]

Our results suggest that retractions are not only treated differently, they are also harder to process. In line with the literature on cognitive imprecision, one interpretation consistent with our results is that such increased complexity is reflected in a noisier perception of the informativeness of a retraction relative to direct information about the state of the world.

#### 6.1.2. Harder Retractions are Harder (Hypothesis 3)

We exploit the fact that our design entails dynamic arrival of information to distinguish retractions according to whether or not they refer to the last observation. Specifically, we argue that retractions of observations received earlier may induce a more complex reevaluation of previously observed signals, relative to retractions of observations received more recently. When, at time $t$, the observation received at $t-1$ is retracted, then subjects need but to revert to the belief they held at $t-2$, that is, before receiving that observation. Hence a natural conjecture is that retractions are more effective in inducing 'unlearning' when they to refer to information that was just received (Hypothesis 3a), in which case subjects only need report their beliefs from the previous period. Moreover, if retractions of more recent observations are then less complex and insofar as decision time and belief variance proxy for cognitive complexity, lower decision times and belief variance in cases in which retracted observations occurred in the previous period (Hypothesis 3b).

In columns (1) and (4) of Table 4, we report the same basic specifications described in Section 5.3, tests of (a) retractions and (b) retractions versus new information, but with the addition of an indicator variable for whether the last signal observed was retracted $(rl_t)$, as well as its interaction with the signal itself. We find greater effectiveness of retractions when these correspond to the most recently received ball draw: it is easier to disregard a piece of information if it arrived more recently. However, subjects still fail to fully disregard retracted signals, even when they are of the most recent draw, as reflected by the sum of the coefficients on $r_t \cdot s_t$ and $rl_t \cdot s_t$ being negative ($p$-value$=.021$) and over 1/3 of the size of $r_t \cdot s_t$.

Analogously, we expand the analysis of decision times and of (conditional sample) variance of beliefs from Table 3 to also account for whether the retracted observation corresponds to the last draw or not. Columns (2) and (5) add the indicator for whether the last signal observed

[33] Note that subjects are fully informed they may see a retraction prior to any round where they do, and the interface is as similar as possible for new draws and retractions; hence we do not find it surprising we do not detect a difference depending on whether subjects has seen more retractions in the past.

| | Prior vs. Retraction | | | New Draw vs. Retraction | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | $b_t$ | $\log(dt_t)$ | $V(b_t \mid h_t)$ | $b_t$ | $\log(dt_t)$ | $V(b_t \mid h_t)$ |
| Retraction ($r_t$) | 0.630 | 0.088*** | 131.1*** | -0.158 | 0.119*** | 77.5*** |
| | (0.412) | (0.018) | (24.558) | (0.438) | (0.018) | (26.426) |
| Retracted Signal ($r_t \cdot s_t$) | -4.341*** | – | – | -4.258*** | – | – |
| | (0.685) | | | (0.800) | | |
| Last Draw Retracted ($rl_t$) | -0.981 | -0.080*** | -80.828** | -0.029 | -0.042* | -76.454** |
| | (0.662) | (0.019) | (33.165) | (0.697) | (0.021) | (33.968) |
| Retracted Signal | 2.737*** | – | – | 1.436** | – | – |
| x Last Draw Retracted ($rl_t \cdot s_t$) | (0.678) | | | (0.641) | | |
| Mean Decision Time (secs) | – | 6.674 | – | – | 6.674 | – |
| Compressed History FEs | Yes | Yes | Yes | No | No | No |
| Sign History FEs | No | No | No | Yes | Yes | Yes |
| R-Squared | 0.34 | 0.01 | 0.02 | 0.34 | 0.02 | 0.02 |
| Observations | 22578 | 22578 | 3295 | 22578 | 22578 | 3295 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Harder Retractions are Harder (Hypothesis 3)

*Notes*: This table tests if retractions of less recent observations: are less effective (columns (1) and (4)), result in shorter decision times (columns (2) and (5)), and induce lower belief variance (columns (3) and (6)). The sample is restricted to subjects in the baseline treatment (beliefs elicited each period). The dependent variable in columns (1) and (4) is the beliefs in period $t$, $b_t \in [0, 100]$. In the case of a retraction, $s_t$ is the opposite sign of the original observation being retracted in round $t$ (+1 if an earlier -1 signal is retracted, -1 if an earlier +1 signal is retracted). In columns (2) and (5), the dependent variable corresponds to the decision time at a given period. The dependent variable in columns (3) and (6) is the sample variance of beliefs of a given subject, conditional on on whether or not a retraction occurred, on whether the last observation was retraction, and on the permuted compressed history (column (3)) or the permuted sign history (column (6)). Columns (1) and (4) excludes beliefs in periods following the disclosure of the truth ball and in which there was a retraction in the past; columns (2) and (5) consider decision times under the same conditions.

was retracted $(rl_t)$ to equation (7a). Columns (3) and (6) mimic the specifications in equation (7b) and (7c) by conditioning not only on permuted compressed/sign history and the occurrence of a retraction, but also on whether the retraction refers to the last draw. In line with greater effectiveness, we further observe that belief reporting is starkly faster when the retraction refers not to an earlier but to the last draw—columns (2) and (5) of Table 4—and also that retractions of more recent observations induce lower belief variances—columns (3) and (6) of Table 4.

To summarize, our results suggest that more recent observations are easier to retract than more distant observations: their retractions are cognitively less demanding and also more effective in inducing unlearning—albeit less effective than obtaining informationally equivalent direct information.

#### 6.1.3. Updating After Retractions (Hypothesis 4)

To conclude this section, we consider the effect that retractions have on updating from subsequent new evidence. To our knowledge, this is the first time that data of this kind is collected and analyzed, and, therefore, existing literature provides little guidance. Our design precludes any consideration of drawing inferences on the credibility of the source following a retraction. Our posited mechanism, however, suggests that if a retraction is more difficult to process, it may be more difficult to update following a retraction.

We first test if beliefs change less in response to a signal following a retraction by estimating

$$\Delta b_t = \beta_1 \cdot r_{t-1} \cdot s_t + \beta_2 \cdot r_{t-1} + F_{S(H_t)} \tag{8a}$$

where $r_{t-1}$ is an indicator for whether a retraction occurred in the previous period. We consider all beliefs from all periods, except cases when the truth ball is disclosed or when there are multiple retractions within a round. The use of sign history fixed-effects $F_{S(H_t)}$ allows us to identify the effect of a retraction in the previous period on belief updating. Our dependent variable of interest is the *change* in beliefs $\Delta b_t$ and not level beliefs $b_t$ since observing an effect could be due to the effect of retractions on updating at $t-1$ and not on subsequent updating.[34] For robustness, we also report the same specification using the change in log-odds beliefs instead.

To gauge if updating from new observations after retractions is harder, we use an analogous identification strategy and estimate

$$\log(dt_t) = \beta_1 \cdot r_{t-1} + F_{S(H_t)}. \tag{8b}$$

Finally, we rely on the same approach as described in Section 6.1.1 to test if previous retractions affect belief volatility. In particular, we calculate for each subject the sample variance of their beliefs conditional on whether or not a retraction occurred in the previous period and on the (permuted) sign history; we denote such quantity by $V(b_t \mid h_t)$. Then, using $V(b_t \mid h_t)$ as a

[34] For example, subjects could update correctly from $s_t$, but have different priors $b_{t-1}$ depending on whether they observed a retraction at $t-1$ or not.

| | New Draw vs. Retraction | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $\Delta \mathrm{b}_t$ | $\Delta \mathrm{l}_t$ | $\log(\mathrm{dt}_t)$ | $\mathrm{V}(\mathrm{b}_t \mid h_t)$ |
| Retraction ($r_{t-1}$) | 0.794 | 0.074 | 0.083*** | 136.0*** |
| | (0.805) | (0.068) | (0.025) | (40.526) |
| Retracted x Signal ($r_{t-1} \cdot s_t$) | -1.881** | -0.229*** | – | – |
| | (0.782) | (0.069) | | |
| Mean Decision Time (secs) | – | – | 6.675 | – |
| Compressed History FEs | No | No | No | No |
| Sign History FEs | Yes | Yes | Yes | Yes |
| R-Squared | 0.18 | 0.15 | 0.02 | 0.03 |
| Observations | 21270 | 21270 | 21270 | 2611 |

Clustered standard errors at the subject level in parentheses.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 5: Updating After Retractions (Hypothesis 4)

*Notes*: This table tests if processing new observations after retractions results is more difficult, resulting in subjects updating less from new observations (columns (1) and (2)), in longer decision times (column (3)), and greater belief variance (column (4)). The sample includes beliefs of subjects in the baseline treatment (beliefs elicited each period). In columns (1)-(3), the sample excludes cases in which the truth ball is disclosed and in which there was a retraction in period 4. Column (4) compares belief variance after a retraction to belief variance after an equivalent new draw; the dependent variable is the sample variance of beliefs of a given subject, conditional on permuted sign histories and on whether a retraction occurred in the previous period.

dependent variable, we estimate a version of equation (8b), where fixed effects refer to permuted sign history.

The results are presented in Table 5. Columns (1) and (2) suggest that, indeed, subjects infer less from new observations following a retraction, with the coefficient on $r_{t-1} \cdot s_t$ being significantly negative.[35] Moreover, having had a retraction in the previous period does significantly increase decision time by 8% (column (3)), indicating greater cognitive imprecision in evaluating available evidence. Column (4) shows that belief variance increases, further corroborating this assessment. To conclude, retractions make subsequent updating from news observations less responsive and more cognitively demanding, consistent with retractions themselves being harder to process.

[35]Considering beliefs in levels $b_t$ in column (1) leads to similar estimates, but is less amenable to interpretation. Further robustness checks we undertook show that expanding the column (2)'s specification to control for the prior $l_{t-1}$ and its interaction with $r_{t-1}$ leaves the estimate of the coefficient on $r_{t-1} \cdot s_t$ and its standard error virtually unchanged.

### 6.2. Alternative Explanations

In this section, we consider alternative—but not necessarily competing—explanations for why retractions are ineffective in inducing unlearning and less effective than new signals. First, we examine whether biases in updating from retractions are nevertheless similar to those in updating from new observations, and whether such similarities could underlie retraction failures. Second, we test whether retraction ineffectiveness is driven by retracted evidence having been actively used by the subjects. We then conclude with a summary of the mechanisms which we are able to rule out.

#### 6.2.1. Similar Updating Biases (Hypothesis 5)

According to Proposition 1, any explanations that do not treat retractions differently—such as confirmation bias—cannot explain why retractions fail. Here we seek to determine whether, despite their being treated differently, updating from retractions is nevertheless affected by biases akin to those affecting updating from new observations, and whether similarity in these biases could explain retraction failures. Specifically, we return to the regression specifications which were the focus of Section 5.2, equations (5a) and (5b), which are the conventional specifications in the literature analysing other deviations from Bayesian updating in similar experiments. We then fully interact these specifications with the retraction variable, $r_t$, corresponding to whether the signal was in the form of a retraction:

$$\begin{aligned} l_t = \beta_0 + \beta_1 \cdot l_{t-1} + \beta_2 \cdot s_t \cdot K + \beta_3 \cdot s_t \cdot K \cdot c_t + \\ + r_t \cdot [\gamma_0 + \gamma_1 \cdot l_{t-1} + \gamma_2 \cdot s_t \cdot K + \gamma_3 \cdot s_t \cdot K \cdot c_t] \end{aligned} \tag{9}$$

The inclusion of the interactions allows us to detect how previously documented deviations from Bayesian updating vary, depending on whether or not the signal is a retraction. In other words, they provide a flexible functional form in order to capture the effect of retractions as discussed in Section 2.

The results can be found in Table 6. A striking pattern emerges: when updating from new draws subjects (slightly) overinfer from signals ($\beta_2 \geq 1$) and do more so when signals confirm the prior ($\beta_3 > 0$); in contrast, when updating from retractions they *under*-infer ($0 < \beta_2 + \gamma_2 < 1$) and exhibit *anti*-confirmation bias ($\beta_3 + \gamma_3 < 0$).[36] In sum, belief updating from retractions

[36]We conduct $F$-tests to analyze the statistical significance of such observations: $\beta_2$ is not significantly different

| | All Periods | | Period 3 | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $l_t$ | $l_t$ | $l_t$ | $l_t$ |
| Prior ($l_{t-1}$) | 0.834*** | 0.800*** | 0.904*** | 0.839*** |
| | (0.037) | (0.037) | (0.051) | (0.051) |
| Signal ($s_t$) | 1.126*** | 0.998*** | 1.647*** | 1.314*** |
| | (0.071) | (0.072) | (0.135) | (0.142) |
| Signal Confirms Prior ($s_t \cdot c_t$) | – | 0.417*** | – | 0.705*** |
| | | (0.135) | | (0.246) |
| Retraction ($r_t$) | -0.033 | -0.030 | -0.034 | -0.026 |
| | (0.021) | (0.021) | (0.033) | (0.034) |
| Retraction x Prior ($r_t \cdot l_{t-1}$) | 0.019 | 0.070* | -0.093* | -0.002 |
| | (0.040) | (0.042) | (0.053) | (0.051) |
| Retracted Signal ($r_t \cdot s_t$) | -0.768*** | -0.541*** | -1.286*** | -0.825*** |
| | (0.092) | (0.097) | (0.156) | (0.160) |
| Retraction x Signal Confirms Prior ($r_t \cdot s_t \cdot c_t$) | – | -0.675*** | – | -1.051*** |
| | | (0.173) | | (0.241) |
| R-Squared | 0.44 | 0.44 | 0.43 | 0.43 |
| Observations | 22578 | 22578 | 6081 | 6081 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Similar Updating Biases (Hypothesis 5)

*Notes*: This table tests whether biases in belief updating are different when updating from retractions. It estimates the standard specifications in Table 1, but interacting its terms with whether the signal was a retraction. The sample consists of all elicited beliefs of subjects in the baseline treatment (beliefs elicited each period), excluding those elicited after a verification, within a given round. Columns (1) and (2) consider all periods; columns (3) and (4) restrict the sample to period 3. The outcome is the log-odds of beliefs in period $t$, $l_t$. $s_t$ is the signal in round $t$ (+1 or -1, multiplied a constant factor of Bayesian updating such that the coefficient on $s_t$ would be 1 under Bayesian updating). $c_t := \mathbf{1}\{\text{sign}(l_{t-1}) = \text{sign}(s_t)\}$ denotes whether the signal at $t$ confirms the prior at $t-1$.

exhibits the opposite biases when compared to updating from new draws, a conclusion which is robust across all specifications. This strengthens the conclusion that retractions are treated differently from new signals, inasmuch as the behavioral responses to retractions are not simply accentuating pre-existing biases; in fact, retractions induce opposite biases in belief reporting behavior.[37]

from 1 in column (2) ($p$-values= .974), and significantly larger than 1 in the remaining columns; $\beta_2 + \gamma_2$ is always

| | Prior vs. Retraction | New Draw vs. Retraction |
|---|---|---|
| | (1) | (2) |
| | $b_t$ | $b_t$ |
| Final ($\text{Fin}_t$) | 0.175 | 0.143 |
| | (0.938) | (0.997) |
| Retraction ($r_t$) | -0.048 | -0.230 |
| | (0.326) | (0.400) |
| Retracted Signal ($r_t \cdot s_t$) | -2.404*** | -3.658*** |
| | (0.622) | (0.712) |
| Final x Retraction ($\text{Fin}_t \cdot r_t$) | – | 0.049 |
| | | (0.754) |
| Final x Retracted Signal ($\text{Fin}_t \cdot r_t \cdot s_t$) | 0.132 | 0.154 |
| | (0.999) | (0.995) |
| Compressed History FEs | Yes | No |
| Sign History FEs | No | Yes |
| R-Squared | 0.21 | 0.31 |
| Observations | 11213 | 9920 |

Clustered standard errors at the subject level in parentheses.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 7: Retracting Used Evidence (Hypothesis 6)

*Notes*: This table tests whether updating from retractions is different if beliefs have previously been elicited before a signal is retracted. It estimates specifications in Table 2, but interacting its terms with an indicator for whether the subject was in the single-elicitation treatment ($\text{Fin}_t$). The sample includes all subjects, both those in the baseline treatment (beliefs are elicited each period) as well as those in the final elicitation treatment (beliefs are elicited only at the end of the round), but restricted to periods 1 to 3 and to cases in which the truth ball is not disclosed. Column (1) restricts to period 1 and to period 3 when there is a retraction, interacting the specification from column (1) in Table 2—are retractions effective—with a dummy for being in the single-elicitation group ($\text{Fin}_t \cdot r_t$ and $\text{Fin}_t \cdot s_t$ are spanned by the other controls and hence omitted, since period 3 is only in the sample when it is a retraction, making $\text{Fin}_t = \text{Fin}_t \cdot r_t$ within the sample). Column (2) restricts to period 3, interacting the specification from column (2) in Table 2—is updating from retractions different from updating from new observations—with a dummy for being in the single-elicitation group.

### 6.2.2. Retracting Used Evidence (Hypothesis 6)

To conclude this section, we consider another potential explanation for why retractions fail to induce unlearning: that it is difficult to disregard evidence that has been actively used. In other words, retraction failure could be due to something akin to an 'information endowment' effect, with individuals resisting to 'delete' past information that was acted upon, in absence of which retractions would successfully induce 'unlearning.' We test this hypothesis by comparing updating from retractions when beliefs have already been elicited to when they have not, by contrasting belief reports across our between-subject treatments: the baseline treatment—in which beliefs are elicited every period within a round—and the single-elicitation treatment—in which beliefs are elicited only at the end of the round.

The results from this comparison are documented in Table 7. The specifications correspond to equations (6a) and (6b) which we described in Section 5.3, with the addition of the single-elicitation treatment as an interaction term.[38] The result is a null result: having acted upon a piece of information does not change the effect of it being retracted on belief updating. As before, beliefs move in the directions of signals, but less so for retractions relative to new signals. While this does not imply that retractions are as (in)effective when individuals acted upon past information in other contexts, it does strengthen our conviction that our results are not due to design details.

### 6.2.3. Summary: Ruling Out Alternative Explanations

We conclude by taking stock of alternative explanations for retraction failure that we rule out. First, owing to the fact that our design is deliberately abstract, the retraction failure we identify in Section 5.3 is shown to be a general phenomenon, not tied to details of any particular domain. In particular, the fact that motivated reasoning is often at play in political domains might suggest it plays an important role in the limited effectiveness of retractions; in contrast, however, we find this effect even without motivation. The abstract setting also allows us to quantify objectively correct beliefs, which is difficult or impossible to do in domains where beliefs are subjective or, perhaps more problematically, not concretely defined. Issues of whether retractions lead to questioning the source's reliability, while interesting in their own right, are also precluded in our setting.

Second, as Proposition 1 demonstrates, only explanations specific to retractions can rationalize retraction failures. Indeed, we design our experiment such that we can compare retractions to other pieces of equivalent information. We can thus distinguish retraction failures from any explanation that applies to all forms of information processing and belief updating, such as confirmation bias.

significantly smaller than 1 and larger than 0 ($p$-value$<$ .001 in all cases); $\beta_3+\gamma_3$ is always negative and significantly different from zero ($p$-value$<$ .001 for column (3) and .011 for column (4)).

[37] As for Table 1, we reestimate the specifications in Table 6 using probability weights so as to render different histories equally likely. Again, the conclusions are the same and the estimates are very similar. The results can be found in Online Appendix B.4.1.

[38] When beliefs are elicited only at the end of each round, it is not possible to obtain the first difference in beliefs and there is therefore no reasonable way to estimate columns (3) and (4) of Table 2.

Third, since our design nests the classic bookbag-and-poker-chips setup, it allows us to replicate and compare our findings with the existing literature on biases in belief updating, and show that the failure of retractions is a distinct phenomenon. Proposition 1 shows that the biases in updating from new information cannot by themselves explain the failure of retractions, as otherwise there would be no difference in updating from retractions and equivalent new observations. Our results studying such biases further show that they are also *qualitatively* different for retractions as compared to new observations, as shown above in Section 6.2.1: biases in updating from retractions are not simply accentuated versions of pre-existing biases.

Fourth, we tested whether retractions fail due to subjects having used the retracted evidence in the past. As reported above in Section 6.2.2, we fail to detect any effect difference on the (in)effectiveness of retractions when compared to a case in which retracted evidence was never acted upon prior to its retraction. Finally, we note that, as the subjects have access, at all times, to the whole history of observations and (if any) retractions, we rule out memory issues related to imperfect recall of previous draws of which observation is retracted.

In sum, if Section 6.1 tests and provides support for one mechanism—that retractions are treated as more complex than new direct evidence—we rule out a plethora of other mechanisms either by testing them explicitly or by design.

## 7. ROBUSTNESS CHECKS

We subjected our results to a battery of robustness checks. In this section, we discuss two possible concerns: that results are driven by subjects failing to understand the setting, and the extent to which retraction ineffectiveness is a general feature in our sample or due to a small fraction of individuals.

### 7.1. Subject Understanding

We strove to ensure that our results were not driven by inattentive subjects. While behavior of participants in our choice of subject pool (Amazon Mechanical Turk) has been shown to approximate well a representative population sample, it is also the case that behavior is 'noisy' relative to a traditional laboratory subject pool (Snowberg and Yariv, 2021; Gupta et al., 2021). We went to lengths to filter out bots and overly inattentive subjects at the start of the experiment, as described in Section 4.3. We also check robustness to excluding subjects based on different measures of inattentiveness.

The results are robust, and indeed slightly stronger, when restricting the sample to those subjects who appear attentive, as defined in three different ways. First, using the (unincentivized) comprehension questionnaire that followed the presentation of the instructions, we restrict our sample to subjects who answered all questions correctly on their first try, who account for a majority. Second, we further restrict the sample to subjects who, when the state is revealed, correctly report that they know the state. Third, we remove subjects whose belief reports are excessively noisy, which we define as updating in the opposite direction to the signal more than 10% of the time.[39] The robustness and indeed slight strengthening of the results (see Online Appendix B.4) is consistent with noisy subjects if anything attenuating the effect, and shows that inattention is not driving our results. Other robustness checks include checking whether subjects mistake sampling with and without replacement.[40]

Finally, the fact that retractions are more effective when they are arguably easier to interpret—e.g. when the last observation is retraction—suggests that subjects do understand the information retractions convey, but that processing this information is challenging and more so than direct evidence about the state.

### 7.2. Individual Heterogeneity

Underinference from retractions appears to be a robust feature within our subject pool, with our results being driven by a substantial fraction of subjects, not just a small minority. To test this, we estimate the specifications in Table 2 at the *subject*-level. We report summary statics on the subject-level estimates of the coefficient of interest (Retracted signal) in Online Appendix B.5.2. It is difficult to fully decompose the heterogeneity in these estimates into underlying population heterogeneity versus sampling noise, given the small number of belief reports per subject. However, both the mean and the median of these coefficient estimates are similar to our baseline estimates, and the estimates are strictly negative for a substantial majority of the subjects (approx. 70%).

[39] As explained in Online Appendix B.4, we go beyond 10% and consider varying degrees of mistake propensity; our conclusions remain the same. We also note these checks are correlated. For example the first two samples contain a substantially smaller fraction of subjects with excessively noisy reports.

[40] If sampling were without replacement, observing three draws of the same color would reveal the color of the truth ball. Less than 10% of all subjects hold extreme beliefs (close to 1 or 0) in these cases. Dropping these subjects leaves the results virtually unchanged.

We also examine whether retractions are more effective for subjects with higher quantitative ability, proxied for by their scores on incentivized quantitative multiple-choice questions which were asked at the end of the experiment. Expanding our main specifications to account for heterogeneity with respect to quantitative ability, we fail to find any significant effects, as reported in Online Appendix B.5.

## 8. CONCLUSION

This paper has shown that people continue to be influenced by information even once told that it is meaningless. We find that there is a residual impact of information after it is retracted, and that this is a consistent phenomenon across a variety of different kinds of beliefs (extreme vs. moderate) and of retractions (confirming vs. contradicting). We demonstrated this in an abstract setting, where this comparison can be made cleanly and precisely, in an incentivized manner.

In the process of illustrating that retractions are in themselves treated as less informative, we formulated a number of hypotheses relating retraction failure to a number of plausible biases. Table 8 revisits each of these hypotheses, and assesses our findings. Our analysis suggests that retraction failure is due to difficulties in reasoning particular to information about information—rendering retractions harder to interpret—and not just an expression of well-known biases. We also argued that our design is the simplest possible which still enables the desired comparisons in our two main empirical tests illustrated in Figure 2.

While our main goal in this paper was to document that retractions had a differential impact, and to determine any significant sources of variation, our results point to a number of interesting potential directions for future work. We see two as being particularly natural.

First, exploring this phenomenon in particular contexts, where it may interact with other behavioral biases, is likely to generate further insights. By using an abstract design, we provided, to the best of our knowledge, the first identification of retraction failure (or the continued influence effect, as in the psychology literature) as distinct from other biases, such as confirmation bias. But just as substantial work has explored confirmation bias or base-rate neglect in particular domains, we believe it would be valuable for future applied work to determine particular real-world settings where retractions appear relatively more difficult to process. As mentioned in our review of the literature, a significant body of work on political behavior suggests that this class of settings features several factors which interfere with Bayesian reasoning. We noted that past difficulties in

| Hypothesis | Documented (✓) or not detected (×) |
|---|---|
| 1 Subjects (a) fail to fully internalize retractions, and (b) treat retractions as less informative than equivalent new information | ✓ |
| 2: Updating from retractions takes longer and increases belief variance | ✓ |
| 3 Retractions of more recent evidence (a) are more effective, and (b) are faster and induce lower belief variance | ✓ |
| 4: Following retractions, (a) subjects update less from new observations, and (b) updating from new observations takes longer and induces greater belief variance | ✓ |
| 5: Similar belief updating biases are present in updating both from new observations and from retractions | ×† |
| 6: Retractions are ineffective only when subjects have acted on retracted observations | ×‡ |

Table 8: Assessment of Main Hypotheses

*Notes*: See Section 3 for a more complete description of each, as well as the reasoning involved with formulating each one. † Retractions reverse the direction of the biases, resulting in under-inference and anti-confirmation bias. ‡ Null effect.

constructing neutral comparisons with subjective information make it unclear how to interpret existing claims retractions are discounted more than they "should" be in these settings. We believe that our framework to be applicable to these various contexts to study their idiosyncrasies and explore whether different ways of framing retractions influence their relative effectiveness. Any such nuance, we believe, could yield insights which help elucidate the failure or success of retractions and fact-checking in practice.

Second, we have not fully explored the possible heterogeneity in the reactions to retractions. In designing this experiment, we sought to present subjects with a setup that was as minimal and focused as possible. Our experiment was designed to provide the first evidence that retraction failures could be attributed (at least in part) to errors in information processing. Aside from certain tests suggested by our theoretical framework, the richness afforded to us by variation in signal timing allowed us to speak to our proposed mechanism, without altering the fundamental nature of the task at hand. But equipped with the findings of this paper, we believe it is worthwhile to elucidate richer patterns behind cognitive noise in unlearning and the implications thereof. For instance, in settings where fact-checking might be strategic, Levkun (2021) presents a theoretical model where the bias identified in this paper will benefit senders at the expense of receivers. In ongoing work we examine whether beliefs are affected by changes in which information is checked, and how. If only information of a specific kind gets checked and retracted—e.g., only articles that challenge the scientific consensus get checked, only political statements supporting specific agendas—would retractions be less effective? Additionally, if only corrections are announced—as is the case in many circumstances—would people correctly infer when retractions render unretracted evidence more reliable? We believe such results would have substantial practical value, like the ones presented here.

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# Online Appendix for
# Learning versus Unlearning

## A. OMITTED PROOFS

### Proof of Proposition 1

Let $f : [0,1] \to [0,1]$ be a strictly increasing function and $\ell$ denote the logit function $\ell(p) = \log(p/(1-p))$. From Definition 1, $(\ell \circ f^{-1})(b(\theta \mid S_t)) = (\ell \circ f^{-1})(b(\theta)) + \sum_{j=1}^{t} K(s_j)$. Now, let $\alpha(\tau \mid S_t) = \frac{P(\text{Retraction of } s_\tau \mid S_t, \theta=1)}{P(\text{Retraction of } s_\tau \mid S_t, \theta=-1)}$. With symmetric noise, if signal $s_\tau$ is retracted, the Bayesian update should be

$$P(\theta \mid S_t, n_\tau = 1) = \frac{P(\theta) K(s_\tau)^{\eta_t - s_\tau} \alpha(\tau \mid S_t)}{P(-\theta) + P(\theta) K(s_\tau)^{\eta_t - s_\tau} \alpha(\tau \mid S_t)},$$

where $\eta_t := \sum_{\ell=1}^{t} s_\ell$. For a retraction, the log-odds update of a Bayesian decisionmaker is therefore:

$$\ell\left(P(\theta \mid S_t, n_\tau = 1)\right) = \ell\left(P(\theta \mid S_t)\right) - K(s_\tau)\mathbf{1}[s_\tau \text{ retracted}] + \log(\alpha(\tau \mid S_t)). \tag{10}$$

Notice that $\alpha(\tau \mid S_t) = 1$, and hence $\log(\alpha(\tau \mid S_t)) = 0$, for all verifying retractions. Therefore, for any $\tau \in \{1, \dots, t\}$,

$$\begin{aligned}
(\ell \circ f^{-1})(b(\theta \mid S_t, n_\tau = 1)) &= (\ell \circ f^{-1})(b(\theta \mid S_t)) - K(s_\tau) \\
&= (\ell \circ f^{-1})(b(\theta)) + \sum_{j \in \{1,\dots,t\}} K(s_j) - K(s_\tau) \\
&= (\ell \circ f^{-1})(b(\theta)) + \sum_{j \in \{1,\dots,t\} \setminus \tau} K(s_j) \\
&= (\ell \circ f^{-1})(b(\theta \mid S_t \setminus s_\tau)).
\end{aligned}$$

As $(\ell \circ f^{-1})$ is injective, then $b(\theta \mid S_t, n_\tau = 1) = b(\theta \mid S_t \setminus s_\tau)$.

If, moreover, $K(s_{t+1}) = -K(s_\tau)$, then it is immediate that $(\ell \circ f^{-1})(b(\theta \mid S_t, n_\tau = 1)) = (\ell \circ f^{-1})(b(\theta \mid S_t \cup s_{t+1}))$ and therefore $b(\theta \mid S_t, n_\tau = 1) = b(\theta \mid S_t \cup s_{t+1})$. $\square$

# B. TABLES AND FIGURES

## B.1. Updating from Retractions: by Bayesian Posterior

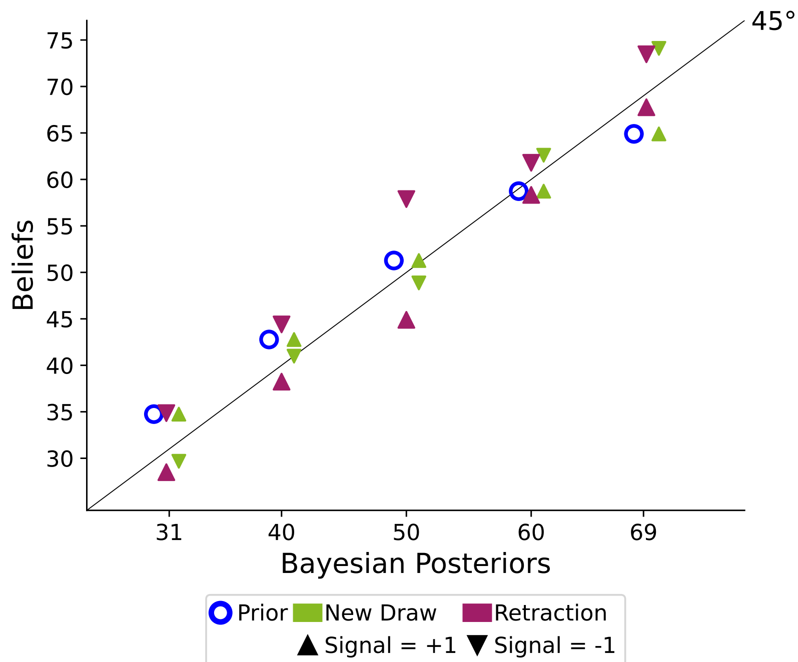



Figure 5: Retractions are Ineffective: Beliefs (Hypothesis 1)

*Notes*: The figure compares mean reported beliefs, disaggregated by histories conducive to the same Bayesian posterior. Blue circles denote mean reported beliefs, for period 1 and 2 histories inducing a given Bayesian posterior. Green triangles denote mean reported beliefs following a new observation, for period 3 and 4 histories inducing a given Bayesian posterior. Magenta triangles denote mean reported beliefs following a retraction, for period 3 and 4 histories inducing a given Bayesian posterior. Triangles pointing up (resp. down) indicate that the last signal was +1 (resp. -1).

### B.2. Comparisons of Belief Reports to Bayesian Posteriors

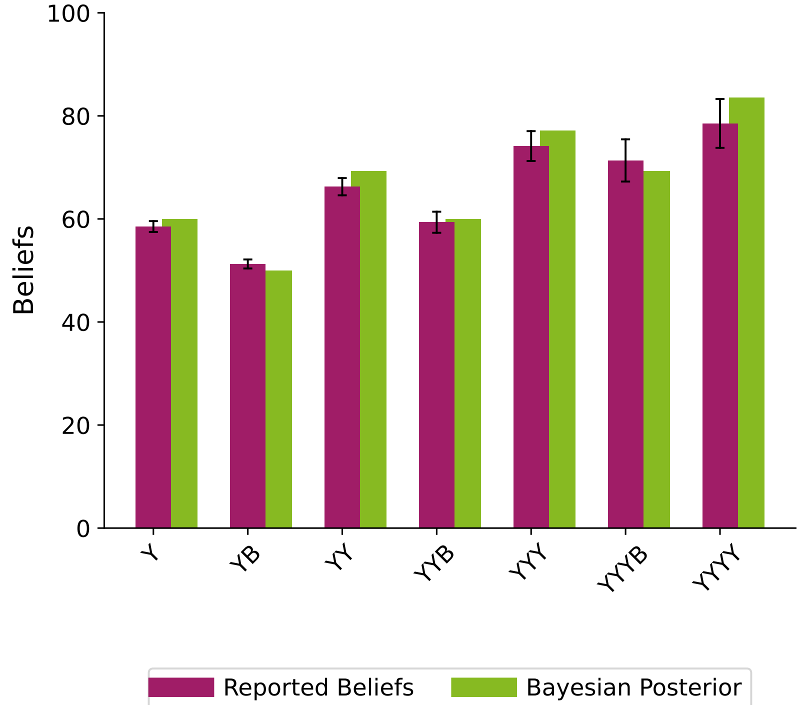



Figure 6: Reported Beliefs and Bayesian Posteriors

*Notes*: The figure compares mean reported beliefs with Bayesian posteriors. Histories and belief reports are symmetrized around 50, e.g. history $BB$ is treated as $YY$ and $b|BB$ is treated as $100 - b|YY$. The sample is restricted to the baseline treatment and to histories in which no observation is retracted. The whiskers denote 95% confidence intervals using standard errors clustered at the subject level.

(a) Difference with respect to Bayesian Posterior

(b) Distance to Bayesian Posterior

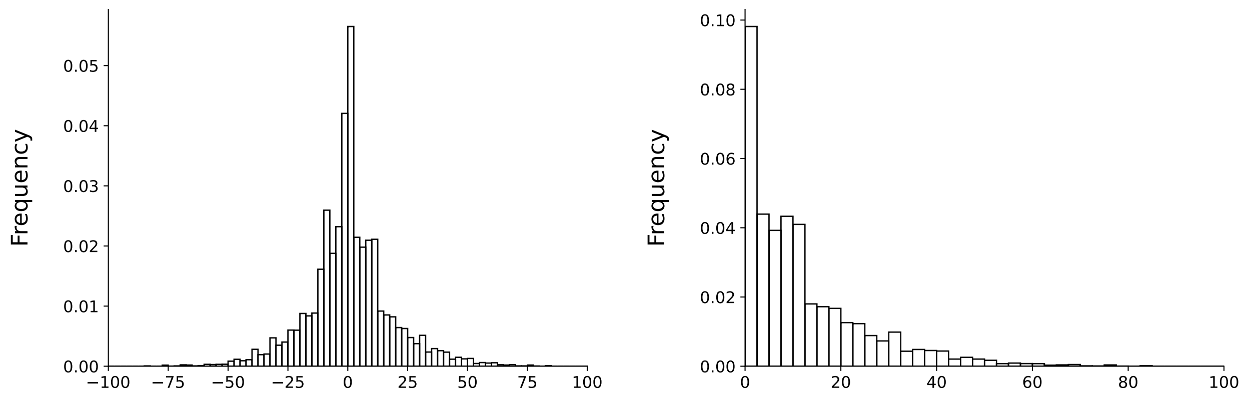



Figure 7: Distribution of Reported Beliefs

*Notes*: The figure shows the distribution of the the difference (a) and the absolute difference (b) of reported beliefs and Bayesian posteriors. The sample is restricted to histories in which no observation is retracted.

### B.3. Sample Characteristics

| | (1) All | (2) Single-Elicitation | (3) Baseline Treatment |
|---|---|---|---|
| Number of Subjects | 419 | 204 | 215 |
| Age | 38.58 | 39.52 | 37.66 |
| | (15.66) | (19.84) | (10.06) |
| Female | 0.40 | 0.40 | 0.40 |
| High School | 0.11 | 0.11 | 0.10 |
| College | 0.21 | 0.23 | 0.19 |
| Bachelor's or equivalent | 0.45 | 0.41 | 0.49 |
| Postgrad or equivalent | 0.18 | 0.19 | 0.18 |
| Other Education level | 0.05 | 0.05 | 0.04 |
| Answered all numeracy questions correctly | 0.57 | 0.55 | 0.59 |
| Total score on numeracy measures | 1.74 | 1.79 | 1.69 |
| | (1.01) | (1.00) | (1.02) |

Table 9: Sample Characteristics

*Notes*: This table provides a comparison of the socio-demographic characteristics of the subjects in our sample. Column (1) considers all subjects and columns (2) and (3) provide summary statics by treatment.

### B.4. Robustness Checks

#### B.4.1. Tables 1 and 6 with Probability Weights

In contrast to Tables 1 and 6, Tables 10 and 11 weight observations so as to make histories equally likely. We note that our conclusions remain the same.

We weight histories of observations according to the relative frequency of the associated permuted sign history within the respective period and taking into account whether or not a retraction occurred. This ensures that observations in each period are made to count equally toward the estimation, and that relative likelihood of permuted sign histories is rendered the same regardless of whether a retraction occurred or not.

| | (1) | (2) |
|---|---|---|
| | $l_t$ | $l_t$ |
| Prior ($l_{t-1}$) | 0.880*** | 0.817*** |
| | (0.050) | (0.052) |
| Signal ($s_t$) | 1.385*** | 1.054*** |
| | (0.088) | (0.085) |
| Signal Confirms Prior ($s_t \cdot c_t$) | – | 0.828*** |
| | | (0.179) |
| R-Squared | 0.47 | 0.47 |
| Observations | 18491 | 18491 |

Clustered standard errors at the subject level in parentheses.
$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 10: Updating from New Draws; with frequency-weighted observations

*Notes*: This table reports updating from new ball draws. It re-estimates the specifications in Table 1, under the same conditions, but using inverse frequency weights.

| | All Periods | | Period 3 | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $l_t$ | $l_t$ | $l_t$ | $l_t$ |
| Prior ($l_{t-1}$) | 0.895*** | 0.817*** | 0.949*** | 0.840*** |
| | (0.054) | (0.058) | (0.064) | (0.067) |
| Signal ($s_t$) | 1.552*** | 1.085*** | 2.143*** | 1.515*** |
| | (0.106) | (0.103) | (0.194) | (0.250) |
| Signal Confirms Prior ($s_t \cdot c_t$) | – | 1.083*** | – | 1.209*** |
| | | (0.222) | | (0.389) |
| Retraction ($r_t$) | -0.040 | -0.032 | -0.030 | -0.017 |
| | (0.028) | (0.028) | (0.047) | (0.048) |
| Retraction x Prior ($r_t \cdot l_{t-1}$) | -0.020 | 0.068 | -0.140** | -0.003 |
| | (0.059) | (0.065) | (0.067) | (0.067) |
| Retracted Signal ($r_t \cdot s_t$) | -1.167*** | -0.622*** | -1.785*** | -1.031*** |
| | (0.126) | (0.125) | (0.210) | (0.262) |
| Retraction x Signal Confirms Prior ($r_t \cdot s_t \cdot c_t$) | – | -1.275*** | – | -1.550*** |
| | | (0.246) | | (0.384) |
| R-Squared | 0.50 | 0.51 | 0.44 | 0.44 |
| Observations | 22578 | 22578 | 6081 | 6081 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 11: Similar Updating Biases (Hypothesis 5); with frequency-weighted observations

*Notes*: This table tests whether biases in belief updating are different when updating from retractions. It re-estimates the specifications in Table 6, under the same conditions, but using inverse frequency weights.

### B.4.2. Log Odds

In this section, we re-estimate the main specifications in the paper, but using log-odds beliefs as the dependent variable instead.

| | Prior vs. Retraction | New Draw vs. Retraction | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $\mathrm{l}_t$ | $\mathrm{l}_t$ | $\Delta \mathrm{l}_t$ | $\Delta \mathrm{l}_t$ |
| Retraction ($r_t$) | 0.005 | -0.015 | -0.044$^{*}$ | -0.043 |
| | (0.023) | (0.034) | (0.025) | (0.029) |
| Retracted Signal ($r_t \cdot s_t$) | -0.233$^{***}$ | -0.235$^{***}$ | -0.249$^{***}$ | -0.276$^{***}$ |
| | (0.039) | (0.050) | (0.046) | (0.048) |
| Signal ($s_t$) | – | – | – | 0.668$^{***}$ |
| | | | | (0.055) |
| Compressed History FEs | Yes | No | No | No |
| Sign History FEs | No | Yes | Yes | No |
| Lagged Sign History FEs | No | No | No | Yes |
| R-Squared | 0.26 | 0.26 | 0.14 | 0.14 |
| Observations | 22578 | 22578 | 22578 | 9074 |

Clustered standard errors at the subject level in parentheses.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 12: Retractions are Ineffective (Hypothesis 1); Log-odds

*Notes*: This table tests whether retractions are effective in inducing 'un-learning' and compares their effectiveness relative to new direct information. It re-estimates the specifications in Table 2, under the same conditions, but utilizing log-odds beliefs rather than beliefs in levels. Log-odds beliefs winsorized at 100-$e$ and $e$, with $e = .1$. The results are robust to changing the winsorization threshold.

### B.4.3. Comprehension Questions Correct at First Try

In this section, we re-estimate the main specifications in the paper, but restricting to subjects who correctly answered all the comprehension questions at first try.

Figure 8 shows the proportion of subjects who successfully answered the comprehension questionnaire by the $n$-th try and compares this with the case in which they would be choosing uniformly at random. In particular, we take the case of a sophisticated randomizer that understands which questions were incorrect and only randomizes among the ones that were not revealed incorrect.

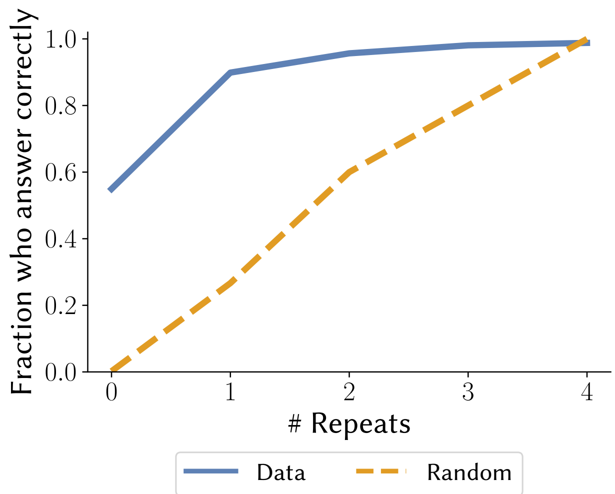



Figure 8: Comprehension Questions

*Notes*: The comparison is to the case in which subjects randomize uniformly over answers that were not previously tried and only in questions that were marked wrong.

| | Prior vs. Retraction | New Draw vs. Retraction | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $b_t$ | $b_t$ | $\Delta b_t$ | $\Delta b_t$ |
| Retraction ($r_t$) | 0.276 | -0.003 | 0.071 | 0.162 |
| | (0.305) | (0.331) | (0.349) | (0.362) |
| Retracted Signal ($r_t \cdot s_t$) | -3.522*** | -4.129*** | -3.779*** | -3.549*** |
| | (0.724) | (0.778) | (0.762) | (0.760) |
| Signal ($s_t$) | – | – | – | 9.712*** |
| | | | | (0.502) |
| Compressed History FEs | Yes | No | No | No |
| Sign History FEs | No | Yes | Yes | No |
| Lagged Sign History FEs | No | No | No | Yes |
| R-Squared | 0.47 | 0.47 | 0.27 | 0.26 |
| Observations | 13574 | 13574 | 13574 | 5446 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Retractions are Ineffective (Hypothesis 1); Robustness Check 1

*Notes*: This table tests whether retractions are effective in inducing 'un-learning' and compares their effectiveness relative to new direct information. It re-estimates the specifications in Table 2, under the same conditions, but restricting the used sample to subjects who correctly answered all comprehension questions at first try.

| | Prior vs. Retraction | | New Draw vs. Retraction | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | log(dt$_t$) | V(b$_t$ $\mid$ $h_t$) | log(dt$_t$) | V(b$_t$ $\mid$ $h_t$) |
| Retraction ($r_t$) | 0.061$^{***}$ | 102.4$^{***}$ | 0.111$^{***}$ | 66.2$^{**}$ |
| | (0.018) | (18.472) | (0.017) | (25.756) |
| Mean Decision Time (secs) | 6.049 | – | 6.049 | – |
| Compressed History FEs | Yes | Yes | No | No |
| Sign History FEs | No | No | Yes | Yes |
| R-Squared | 0.02 | 0.03 | 0.03 | 0.03 |
| Observations | 13574 | 1815 | 13574 | 1815 |

Clustered standard errors at the subject level in parentheses.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 14: Retractions are Harder (Hypothesis 2); Robustness Check 1

*Notes*: This table tests if retractions induce longer decision times and greater belief variance. It re-estimates the specifications in Table 3, under the same conditions, but restricting the used sample to subjects who correctly answered all comprehension questions at first try.

#### B.4.4. Correct Belief Reports when Truth Ball is Revealed

In this section, we re-estimate the main specifications in the paper, not only restricting to subjects who correctly answered all the comprehension questions at first try, but further remove from the sample subjects who failed to correctly report beliefs close to 0 or 100 when the truth ball was revealed. In particular, we remove from the sample any subject who, when state $\theta$ is revealed, failed to report beliefs $|b_t - \theta| \leq \epsilon$. We present the results for $\epsilon = .05$, but the results are robust to the choice of small $\epsilon$.

| | Prior vs. Retraction | New Draw vs. Retraction | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $b_t$ | $b_t$ | $\Delta b_t$ | $\Delta b_t$ |
| Retraction ($r_t$) | 0.174 | -0.399 | -0.069 | -0.010 |
| | (0.327) | (0.351) | (0.320) | (0.306) |
| Retracted Signal ($r_t \cdot s_t$) | -3.647*** | -3.897*** | -3.671*** | -3.526*** |
| | (0.763) | (0.815) | (0.783) | (0.791) |
| Signal ($s_t$) | – | – | – | 10.434*** |
| | | | | (0.476) |
| Compressed History FEs | Yes | No | No | No |
| Sign History FEs | No | Yes | Yes | No |
| Lagged Sign History FEs | No | No | No | Yes |
| R-Squared | 0.61 | 0.62 | 0.44 | 0.45 |
| Observations | 10263 | 10263 | 10263 | 4119 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 15: Retractions are Ineffective (Hypothesis 1); Robustness Check 2

*Notes*: This table tests whether retractions are effective in inducing 'un-learning' and compares their effectiveness relative to new direct information. It re-estimates the specifications in Table 2, under the same conditions, but excluding subjects who did not correctly answer all comprehension questions at first try or who did not correctly report beliefs when the state was revealed.

| | Prior vs. Retraction | | New Draw vs. Retraction | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $\log(\mathrm{dt}_t)$ | $V(b_t \mid h_t)$ | $\log(\mathrm{dt}_t)$ | $V(b_t \mid h_t)$ |
| Retraction ($r_t$) | 0.065*** | 85.7*** | 0.115*** | 60.8** |
| | (0.020) | (19.579) | (0.019) | (27.202) |
| Mean Decision Time (secs) | 5.677 | – | 5.677 | – |
| Compressed History FEs | Yes | Yes | No | No |
| Sign History FEs | No | No | Yes | Yes |
| R-Squared | 0.03 | 0.03 | 0.03 | 0.03 |
| Observations | 10263 | 1378 | 10263 | 1378 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 16: Retractions are Harder (Hypothesis 2); Robustness Check 2

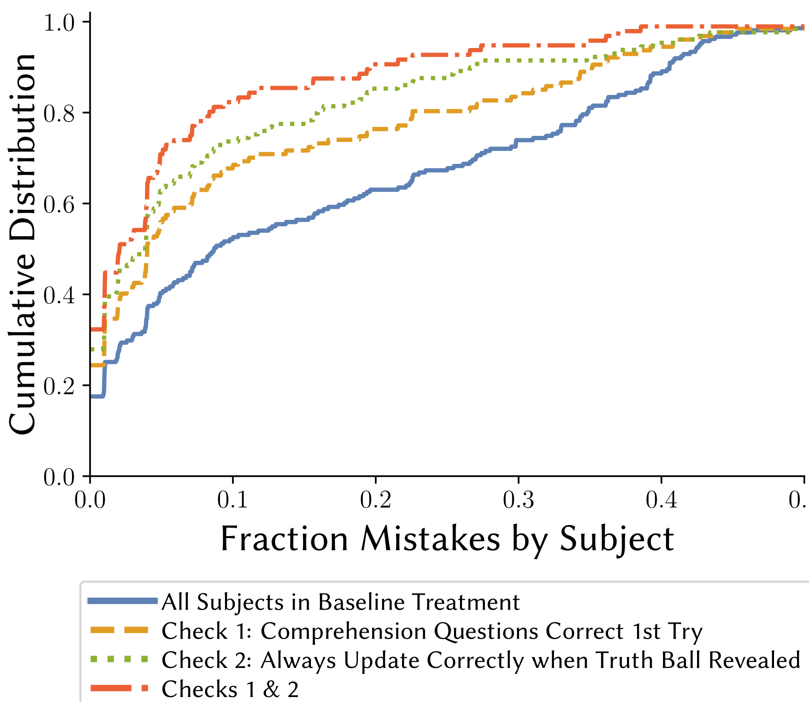
*Notes*: This table tests if retractions induce longer decision times and greater belief variance. It re-estimates the specifications in Table 3, under the same conditions, but excluding subjects who did not correctly answer all comprehension questions at first try or did not correctly report beliefs when the state was revealed.

### B.4.5. Noisy Belief Reports

In this section, we re-estimate the main specifications in the paper, removing subjects who seem to be answering randomly.

For this purpose, we consider that the subject makes a mistake when they update their beliefs in the opposite direction to the signal, i.e. $(b_t - b_{t-1}) \cdot s_t < 0$. For each subject, we compute updating mistakes as a fraction of the total number of belief elicitations and, in Figure 9, we show the distribution of the individual-level mistakes by robustness check. It is immediate that the previous robustness checks do reduce the fraction of subjects who seem to be answering randomly.

Figure 9: Subject-Level Mistakes




In the following tables, we remove from the sample any subject who updates their beliefs in the opposite direction to the signal more than $x\%$ of the time. We present the results for $x = 10\%$; the coefficients are virtually unchanged when considering $5\%$ and $25\%$ instead.

| | Prior vs. Retraction | New Draw vs. Retraction | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $b_t$ | $b_t$ | $\Delta b_t$ | $\Delta b_t$ |
| Retraction ($r_t$) | -0.586** | -0.730** | -0.692** | -0.457 |
| | (0.286) | (0.309) | (0.283) | (0.279) |
| Retracted Signal ($r_t \cdot s_t$) | -5.105*** | -5.912*** | -5.748*** | -5.360*** |
| | (0.726) | (0.802) | (0.760) | (0.766) |
| Signal ($s_t$) | – | – | – | 11.896*** |
| | | | | (0.404) |
| Compressed History FEs | Yes | No | No | No |
| Sign History FEs | No | Yes | Yes | No |
| Lagged Sign History FEs | No | No | No | Yes |
| R-Squared | 0.69 | 0.70 | 0.50 | 0.46 |
| Observations | 11772 | 11772 | 11772 | 4732 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 17: Retractions are Ineffective (Hypothesis 1); Robustness Check 3

*Notes*: This table tests whether retractions are effective in inducing 'un-learning' and compares their effectiveness relative to new direct information. It re-estimates the specifications in Table 2, under the same conditions, but excluding subjects who made mistakes in more than 10% of the periods.

| | Prior vs. Retraction | | New Draw vs. Retraction | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $\log(dt_t)$ | $V(b_t \mid h_t)$ | $\log(dt_t)$ | $V(b_t \mid h_t)$ |
| Retraction ($r_t$) | 0.071*** | 122.0*** | 0.133*** | 102.2*** |
| | (0.022) | (20.368) | (0.020) | (18.116) |
| Mean Decision Time (secs) | 6.016 | – | 6.016 | – |
| Compressed History FEs | Yes | Yes | No | No |
| Sign History FEs | No | No | Yes | Yes |
| R-Squared | 0.03 | 0.06 | 0.04 | 0.06 |
| Observations | 11772 | 1589 | 11772 | 1589 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 18: Retractions are Harder (Hypothesis 2); Robustness Check 2

*Notes*: This table tests if retractions induce longer decision times and greater belief variance. It re-estimates the specifications in Table 3, under the same conditions, but excluding subjects who made mistakes in more than 10% of the periods.

### B.4.6. Sampling with or without Replacement

In this section, we re-estimate the main specifications in the paper, removing subjects who always report extreme beliefs (above 95 and below 5) when observing three draws of the same color.

| | Prior vs. Retraction | New Draw vs. Retraction | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $b_t$ | $b_t$ | $\Delta b_t$ | $\Delta b_t$ |
| Retraction ($r_t$) | 0.273 | -0.229 | -0.281 | -0.153 |
| | (0.292) | (0.395) | (0.385) | (0.392) |
| Retracted Signal ($r_t \cdot s_t$) | -2.722*** | -3.211*** | -3.214*** | -2.792*** |
| | (0.634) | (0.777) | (0.708) | (0.713) |
| Signal ($s_t$) | – | – | – | 7.786*** |
| | | | | (0.521) |
| Compressed History FEs | Yes | No | No | No |
| Sign History FEs | No | Yes | Yes | No |
| Lagged Sign History FEs | No | No | No | Yes |
| R-Squared | 0.31 | 0.31 | 0.16 | 0.13 |
| Observations | 20423 | 20423 | 20423 | 8199 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 19: Retractions are Ineffective (Hypothesis 1); Robustness Check 4

*Notes*: This table tests whether retractions are effective in inducing 'un-learning' and compares their effectiveness relative to new direct information. It re-estimates the specifications in Table 2, under the same conditions, but excluding subjects report extreme beliefs when observing three draws of the same color.

| | Prior vs. Retraction | | New Draw vs. Retraction | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $\log(\text{dt}_t)$ | $\text{V}(\text{b}_t \mid h_t)$ | $\log(\text{dt}_t)$ | $\text{V}(\text{b}_t \mid h_t)$ |
| Retraction ($r_t$) | 0.046*** | 119.4*** | 0.102*** | 70.7*** |
| | (0.017) | (22.190) | (0.016) | (23.199) |
| Mean Decision Time (secs) | 6.759 | – | 6.759 | – |
| Compressed History FEs | Yes | Yes | No | No |
| Sign History FEs | No | No | Yes | Yes |
| R-Squared | 0.01 | 0.02 | 0.02 | 0.02 |
| Observations | 20423 | 2740 | 20423 | 2740 |

Clustered standard errors at the subject level in parentheses.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 20: Retractions are Harder (Hypothesis 2); Robustness Check 4

*Notes*: This table tests if retractions induce longer decision times and greater belief variance. It re-estimates the specifications in Table 3, under the same conditions, but excluding subjects report extreme beliefs when observing three draws of the same color.

### B.4.7. Decision Time Controlling Across Rounds

| | Prior vs. Retraction | New Draw vs. Retraction |
|---|---|---|
| | (1) | (2) |
| | log(dt$_t$) | log(dt$_t$) |
| Retraction ($r_t$) | 0.068$^{**}$ | 0.114$^{***}$ |
| | (0.027) | (0.027) |
| Round | -0.013$^{***}$ | -0.013$^{***}$ |
| | (0.001) | (0.001) |
| Retraction ($r_t$) x Round | -0.001 | -0.001 |
| | (0.001) | (0.001) |
| Mean Decision Time | 6.674 | 6.674 |
| Compressed History FEs | Yes | No |
| Sign History FEs | No | Yes |
| R-Squared | 0.05 | 0.06 |
| Observations | 22578 | 22578 |

Clustered standard errors at the subject level in parentheses.

$^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$

Table 21: Retractions are Harder (Hypothesis 2a): Decision Time Controlling for Round

*Notes*: This table tests if retractions induce longer decision times. Under the same conditions, it re-estimates the specifications in Table 3 pertaining to decision time (columns (1) and (3) therein), but controlling for the round number. This serves as a robustness check to understand whether the effect of retractions on decision time fades away as subjects become more experienced.

### B.4.8. Belief Variance by History

| (1) | (2) | (3) | (4) |
|---|---|---|---|
| Permuted Compressed History | Retraction | Prior | Difference |
| B | 17.54 | 15.34 | 2.21*** |
| Y | 16.91 | 14.20 | 2.71*** |
| BB | 23.51 | 17.41 | 6.10*** |
| YB | 18.30 | 13.76 | 4.54*** |
| YY | 20.59 | 16.31 | 4.27*** |
| Permuted Sign History | Retraction | New Draw | Difference |
| YBB | 17.54 | 17.14 | 0.41* |
| YYB | 16.91 | 14.96 | 1.95*** |
| YBBB | 23.51 | 23.45 | 0.05 |
| YYBB | 18.30 | 15.20 | 3.09*** |
| YYYB | 20.59 | 21.50 | -0.91 |

Table 22: Retractions are Harder (Hypothesis 2b): Belief Variance by History

*Notes*: This table tests if retractions induce greater belief variance. Each row tests for equality of variance of belief reports conditional on permuted compressed/sign histories and on whether or not a retraction was observed using Levene's test centered at the median. The columns (2) and (3) indicate the sample estimate of standard deviation of belief reports conditional on permuted compressed/sign histories and on whether a retraction occurred that period (column (2)) or not column (3). Column (4) shows the difference in sample standard deviations and indicates whether or not it is significant according to the outcome of Levene's test.

## B.5. Heterogeneous Effects

### B.5.1. Quantitative Ability

In this section, we test for the existence of heterogeneous treatment effects relative to the subjects quantitative ability. In the last part of our experiment, we posed three multiple-choice quantitative questions. The median number of correct answers per subject was two. We expand our specifications by interacting all the regressors with a dummy variable that equals 1 if the subject answered all questions correctly and 0 if otherwise.

| | Prior vs. Retraction | New Draw vs. Retraction | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $b_t$ | $b_t$ | $\Delta b_t$ | $\Delta b_t$ |
| Retraction ($r_t$) | 0.353 | 0.008 | -0.408 | -0.309 |
| | (0.347) | (0.436) | (0.444) | (0.484) |
| Retracted Signal ($r_t \cdot s_t$) | -2.958*** | -3.483*** | -4.294*** | -3.038*** |
| | (0.779) | (0.887) | (0.795) | (0.890) |
| Signal ($s_t$) | – | – | – | 7.821*** |
| | | | | (0.659) |
| All correct | -1.273*** | -1.278*** | -0.497*** | -0.586* |
| | (0.406) | (0.407) | (0.170) | (0.340) |
| Retraction ($r_t$) x All correct | -0.531 | -0.643 | 0.213 | 0.257 |
| | (0.489) | (0.475) | (0.534) | (0.600) |
| Retracted signal ($r_t \cdot s_t$) x All correct | -0.644 | -0.536 | 2.129** | -0.975 |
| | (1.098) | (1.095) | (0.983) | (1.224) |
| Signal ($s_t$) x All correct | – | – | – | 3.033*** |
| | | | | (0.868) |
| Compressed History FEs | Yes | No | No | No |
| Sign History FEs | No | Yes | Yes | No |
| Lagged Sign History FEs | No | No | No | Yes |
| R-Squared | 0.34 | 0.34 | 0.18 | 0.16 |
| Observations | 22578 | 22578 | 22578 | 9074 |

Clustered standard errors at the subject level in parentheses.

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 23: Retractions are Ineffective (Hypothesis 1); Heterogeneous Effects

*Notes*: This table tests whether retractions are effective in inducing ‘un-learning’ and compares their effectiveness relative to new direct information. We investigate the existence of heterogeneous effects with respect to quantitative ability by expanding our baseline specifications from Table 2 with interaction terms. ‘All correct’ denotes a dummy variable that equals 1 when the subject answered all quantitative questions correctly, and 0 if otherwise.

### B.5.2. Subject-Level Estimates

| | Prior vs. Retraction | New Draw vs. Retraction | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | $b_t$ | $b_t$ | $\Delta b_t$ | $\Delta b_t$ |
| Mean | -3.285 | -3.727 | -3.892 | -3.448 |
| Median | -2.103 | -2.602 | -2.797 | -3.236 |
| Fraction Coeff $< 0$ | 0.68 | 0.71 | 0.73 | 0.72 |
| Mean std error | 2.867 | 3.77 | 4.294 | 4.777 |
| Compressed History FEs | Yes | No | No | No |
| Sign History FEs | No | Yes | Yes | No |
| Lagged Sign History FEs | No | No | No | Yes |

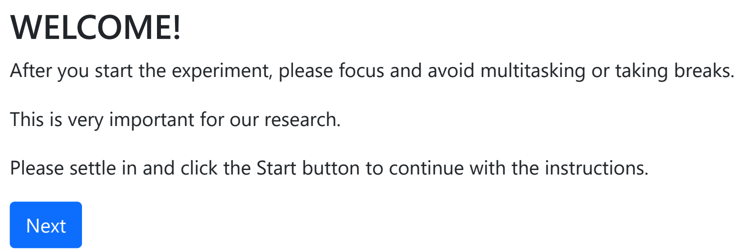
Table 24: Retractions are Ineffective (Hypothesis 1); Subject-Level Estimates

*Notes*: This table provides summary statistics on distribution of subject-level estimates of the coefficient of interest in the main specification of interest in this paper. We investigate the existence of individual-level heterogeneity by estimating the specifications in Table 2 for each subject. The sample consists of subjects in the baseline treatment (beliefs are elicited each period) who observed at least 8 retractions in the 32 rounds.

# C. INSTRUCTIONS AND SCREENSHOTS

## C.1. Start Screen and Instructions

Below are screenshots of the start screen and the instructions as presented to the subjects.



# Outline

You are about to participate in an experiment on the economics of decision-making. In the experiment you can earn up to $12.50 if you do well, which will be paid to you at the end of the experiment.

You will begin, on the next screen, with the instructions. Please read them carefully.

At the end of the instructions there will be questions to check that you understand how the experiment works. Upon answering these questions correctly, you will proceed to the experiment.

The experiment contains 32 rounds, and we expect it to take **shorter than one hour** to complete. Your payment will depend on your performance in the experiment. The goal of the experiment is to study how people process new information.

Before the experiment begins there will be two practice rounds for you to familiarize yourself with the interface. After the experiment, the final part of the task is a brief survey.

You will be **guaranteed a payment of $6.00** by completing the experiment, of which $2.00 will be paid immediately afterwards and $4.00 paid together with the bonus. In addition to this, you can get a **bonus of $6.00**, which depends on your performance.

We estimate an **average hourly payment of above $9.00.**

## 'Bot'-Detection

This task is designed for humans and cannot be fulfilled using automated answers.

You will be asked to prove you are complying with this requirement by transcribing words at random points in this task. The text will be as legible as the text in these instructions. Any human able to read this text will be able to read the words for transcription, but a 'bot' will not. You will be allowed 3 attempts and 2 minutes per attempt. If you fail to transcribe a word three times, the task will be immediately terminated and you will automatically get no payment. You will not be able to perform the task again.

## Quitting the Task

You can quit the task at any time. However, if you do so, the task is immediately terminated and you will automatically get no payment. You will not be able to perform the task again.

## Additional Information

In the experiment you will answer questions which ask you to choose between different options. Your responses to this experiment will be used to study how people process information. No identifying data about you will be made available and all data we store will be anonymized. All data and published work resulting from this experiment will maintain your individual privacy.

Next

# Instructions

## Welcome!

In the experiment you will be asked to estimate the probability that a given ball in a box is blue or yellow.
The experiment is divided into 32 rounds, each round with up to 4 periods, plus two practice rounds before you start for you to get familiar with the interface.
We expect the overall experiment to last for less than 1 hour, although you are free to move at your own pace.
We also expect that, with an adequate amount of effort, participants get on average $9.00, of which $6.00 depends only on completing the task.

## Truth Balls and Noise Balls

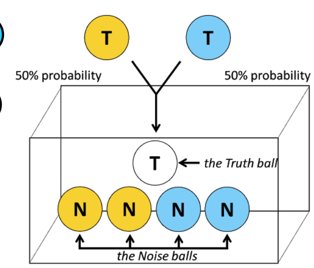
At the beginning of each round, 5 balls are put inside a box.
The balls in that box are of two kinds:

- 4 Noise Balls N, of which 2 are yellow N and 2 are blue N; and
- 1 Truth Ball T, which can be either yellow T or blue T.

Your task is to estimate the probability that the Truth Ball T is yellow T or blue T, upon observing random draws from the selected box in each round.

## Your Task

### A Round




At the beginning of each round, the Truth Ball T is chosen to be either T or T with equal probability.

The Truth Ball T is then put inside the box with all 4 Noise Balls, 2 N and 2 N
.

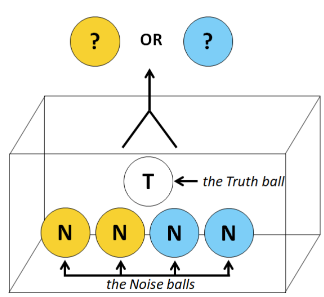
All balls remain inside the box throughout the round.

The round lasts for 4 periods, each of which may help you to guess the color of the Truth Ball T.

Note that the Truth Ball remains the same throughout the round but changes across different rounds.

This means that the draws you observe from a particular round are not helpful to estimate the color of a Truth Ball in another round and every round you need to start afresh.

### Periods 1 and 2

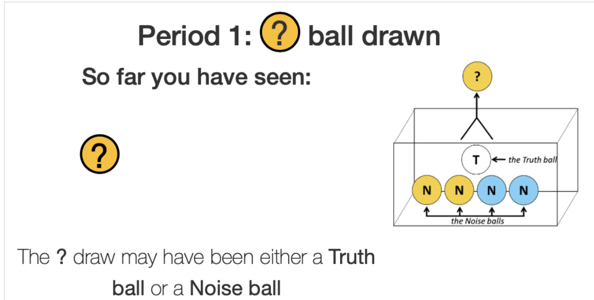



In periods 1 and 2, a ball is drawn from the box at random and you are told its color, or .
The ball is then placed back into the box.
You will not be told whether it is a Noise Ball N or the Truth Ball T. Because of this, the ball will be labelled with a question mark ?.
Since the balls are drawn at random, the drawn ball ?:

- is the Truth Ball T with 20% probability;
- is a Noise Ball N with 80% probability.

Naturally, the more draws you observe, the more likely that one of them is the Truth Ball, and the more balls of one color you observe, the more likely it is that the Truth Ball is of that color. However, because in each period the ball you are shown is placed back into the box, it can be that you are shown the Truth Ball multiple times or even that you are only shown Noise Balls.

This is an example of what you can see at period 1:




Periods 3 and 4

At the beginning of period 3, a coin is flipped, and

- (i) with 50% probability it lands heads and you will observe a new draw from the box;
- (ii) with 50% probability it lands tails and you will observe a validation, learning whether one of the balls is a Noise Ball or the Truth Ball.

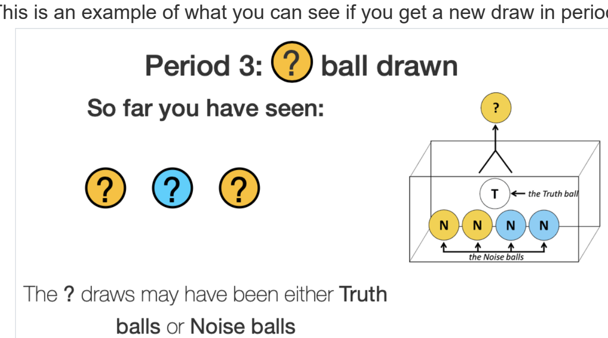
(i) New Draw

If you get a new draw, it will be exactly as before: a ball is drawn from the box and its color is shown to you, but not whether it is the Truth Ball or the Noise Ball.

Since the balls are drawn at random, the drawn ball ?:

- is the Truth Ball T with 20% probability;
- is a Noise Ball N with 80% probability.

This is an example of what you can see if you get a new draw in period 3:



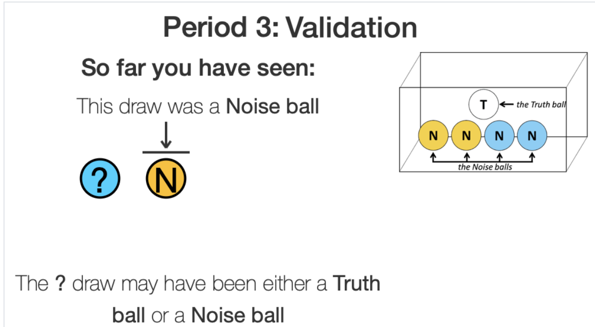

(ii) Validation

If you get a validation

| |
|---|
| one of the ? draws is chosen at random with equal probability, regardless of whether they were draws of the Truth T or Noise N Balls. |

You are then showed whether that draw was a Noise Ball N or the Truth Ball T itself.

This is an example of what you can see if you get a validation in period 3:




New Round

After these 4 periods, a new round begins.

Each round, a new color for the Truth Ball T is selected the same way and independently.

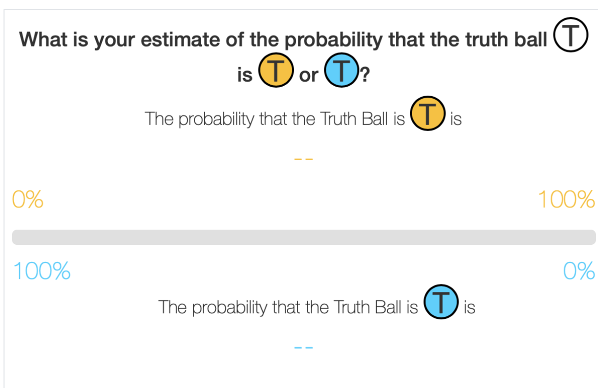
This means that whether the Truth Ball is T or T in one round has no influence on whether the Truth Ball is T or T in another round.
It will be clearly indicated when a new round begins.

## Estimates

Every period and every round you will be asked to provide your estimate of the probability that the Truth Ball T is yellow T or blue T.

Unless it is shown to you in a validation, you will not be able to know the color of the Truth Ball for sure, but you will be able to make inferences based on the draws you have seen. You will be paid based on how accurate your estimate is.

You can enter your estimate using the slider.




## Payment

By completing the experiment, you can secure $6.00 for sure.
You can get a bonus of an additional $6.00 depending on your performance.

At each period, you will receive a number of points which depends on your estimate and on the color of the Truth Ball T in that round.

The higher the probability you assign to the correct color, the more points you get at each round.

If your estimate in a given period is that the Truth Ball is  with probability $q$ $(\times 100\%)$ and an  with probability $(1-q)$ $(\times 100\%)$, then you will receive

- $100 \times (1-(1-q)^2)$ points if the Truth Ball is ; and
- $100 \times (1-q^2)$ points if the Truth Ball is .

So if your estimate completely correctly the color of the Truth Ball, you get 100 points and if you estimate completely incorrectly you get 0 points.

The lower probability you assign to the correct color, the fewer points you receive.

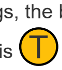
For instance, if you estimate that the Truth Ball is  with 89% probability and  with 11% probability, you receive 98.79 points if the Truth Ball is indeed  and 20.79 if the Truth Ball is instead .

The points you get determine the probability of you getting the bonus.

In order to determine the probability of you getting the bonus, at the end of the experiment, one of the rounds is picked randomly with equal probability and, in this round, one of the periods is then chosen randomly, with equal probability.

The points you got = probability of getting the $6.00 bonus.
This means that if in the selected round/period you have 99.84 points you have 99.84% probability of getting the $6.00 bonus. If you have 36 points you only have 36% probability.

There is, of course, an element of chance in the task, but the more you pay attention, the more you increase the probability of getting the bonus.

All in all, the implication of the reward rule is straightforward: To maximize your expected earnings, the best thing you can do in each period is to always report your best estimate of the probability that the Truth Ball is  or .

This reward system has been designed to encourage you to provide your best estimates.

## Questionnaire

After you have completed all rounds, we will ask you some quantitative reasoning questions, for which you can get an extra $0.50 in bonus and then generic demographic questions.
We will not be collecting any information that allows us to identify you.
The data will be anonymized and your MTurk ID will not be available.
This data will be used for scientific research purposes only.

Only after you answer these questions will the task be completed and we will proceed to implement payments.

# Questions

You must answer the following questions correctly before you can proceed.

1. There are 32 rounds and each round has up to 4 periods.

   ○ The statement is true.

   ○ The statement is false.

2. How many Noise Balls are there?

   ○ 0

   ○ 1

   ○ 2

○ 3

○ 4

3.

How many of the Noise Balls are N and N?

○ 1 N and 3 N

○ 3 N and 1 N

○ 2 N and 2 N

4.

It is possible that you see a ? ball 3 times and the Truth Ball is T.

○ The statement is true.

○ The statement is false.

5.

Even if in a given round the Truth Ball is T, in the following round the Truth Ball can be either T or T with equal (50% - 50%) probability.

○ The statement is true.

○ The statement is false.

6.

If a draw you were shown ? corresponded to a Noise Ball N then the Truth Ball has to be T and not T.

○ The statement is true.

○ The statement is false.

7.

If a draw you were shown ? corresponded to a Noise Ball N then the Truth Ball T may or may not be of a different color.

○ The statement is true.

○ The statement is false.

Check Answers

## C.2. Practice Round

Subjects played had two practice rounds before starting the task. It was explicitly mentioned that these would not count toward their payment.

Practice Rounds

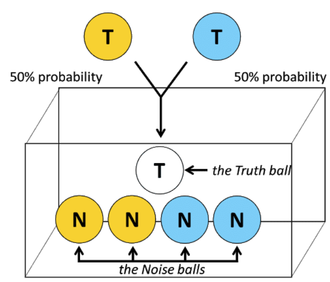
You will now play two practice rounds.
These rounds do not count towards your payment.
They are meant for you to familiarize yourself with the interface and the task.

Start Practice Rounds

Practice Round 1 of 2

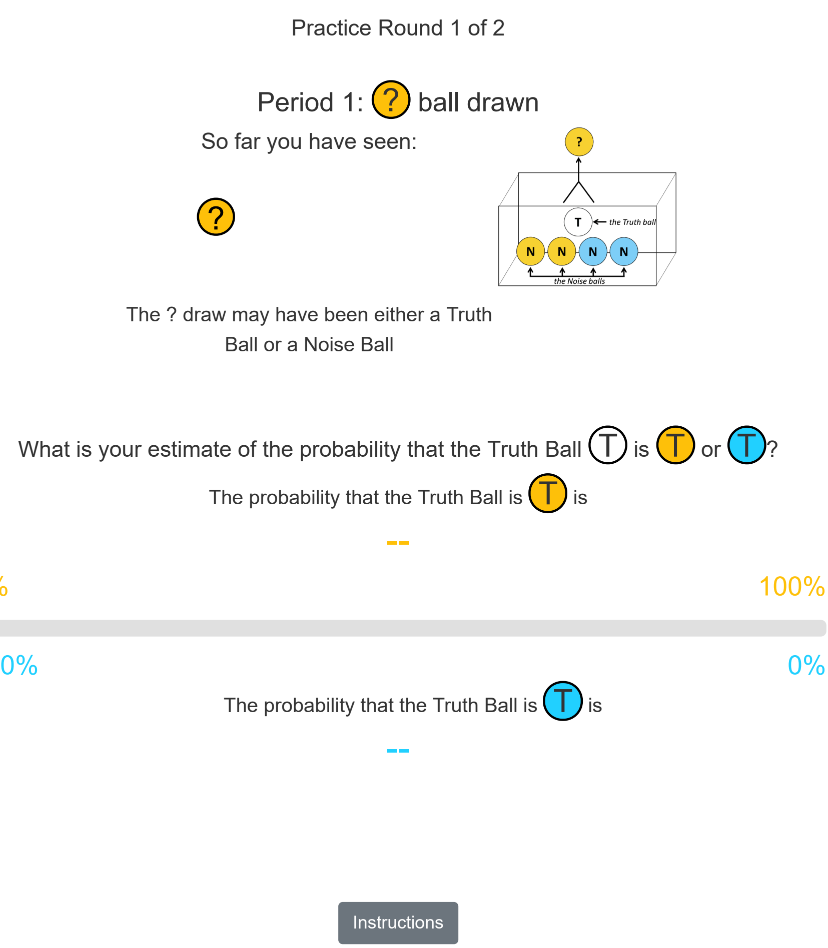
New Round

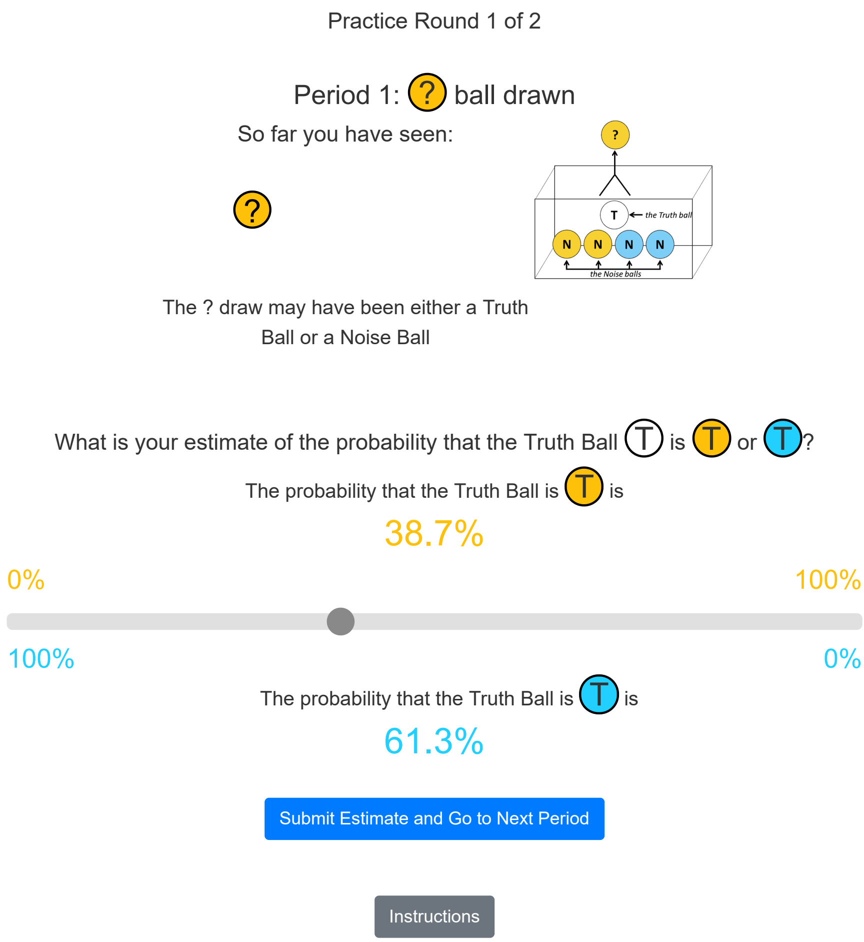
The truth ball is drawn and placed in the box




Start New Round

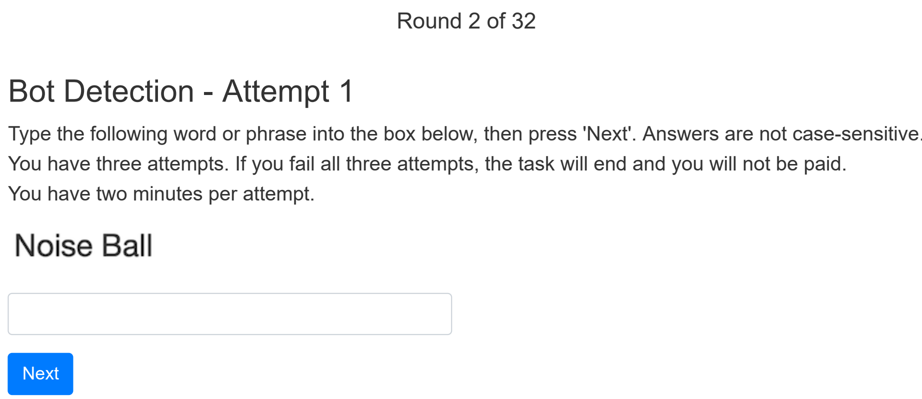
One the page loaded, the slider was blank and only activated once the subjects clicked on it.






### C.3. Captchas

Subjects face five different captchas at different rounds. They had 3 tries and one minute to submit for each try. Were they to fail the 3 tries, the task ended and they would not receive any bonus.




### C.4. Rounds

The rounds were described in Section 4.

Start the Task

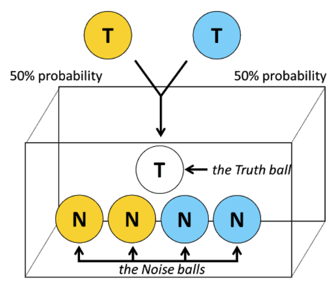
From now on, rounds matter towards your payment.

Start the Task

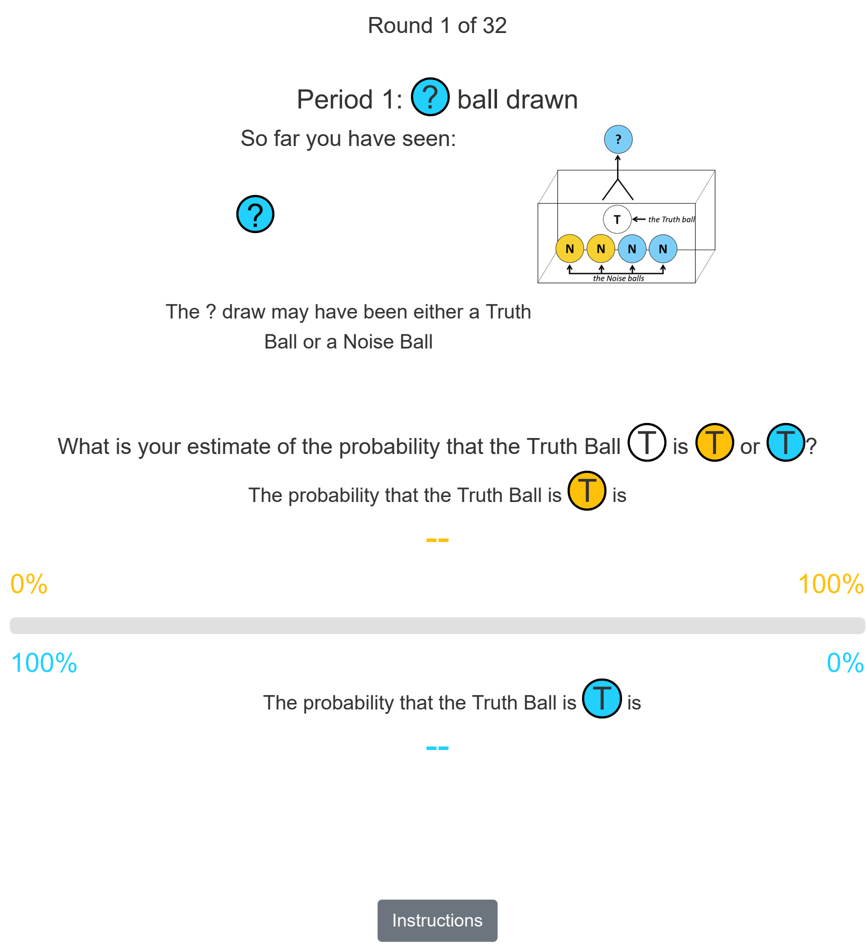
Round 1 of 32

New Round

The truth ball is drawn and placed in the box

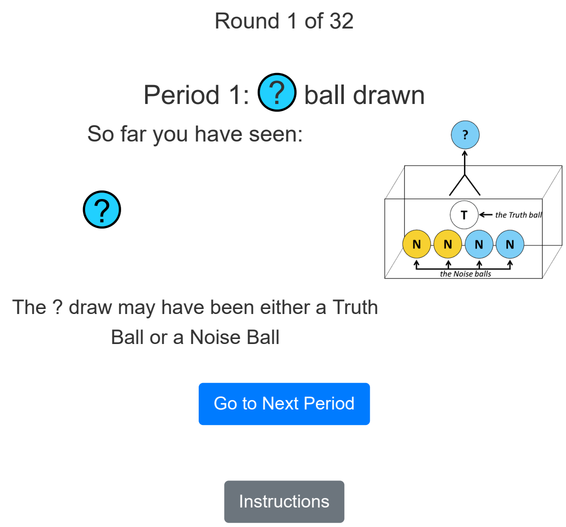



Start New Round



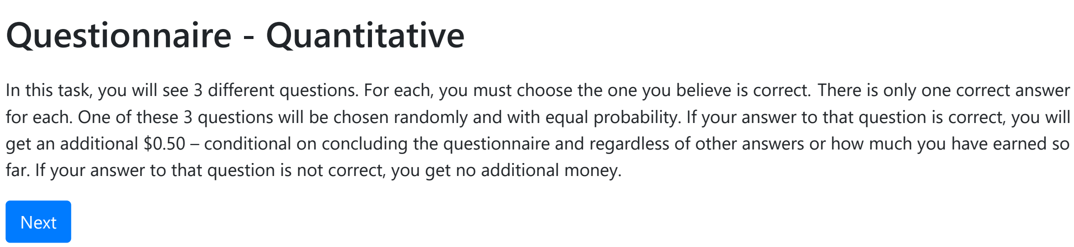

## C.5. Final Period Elicitation Only

Were the subjects to be in the treatment arm in which beliefs were elicited only at the last period of each round, the last period would be just as before. In periods in which there was no belief elicitation, they would observe just the ball draw:




### C.6. Quantitative Questions

After the main task, the subjects had to answer three questions meant to assess their quantitative ability; these were incentivized.



## Questionnaire - Quantitative

Read each question and choose the answer that you believe is correct.

A picture was reduced on a copier to 90% of its original size and this copy was then reduced by 10%. What percentage of the size of the original picture was the final copy?

- ○ 10%
- ○ 81%
- ○ 90%
- ○ 99%
- ○ 100%

Friends Albert, Bruce and Caroline agree to buy $7 worth of lottery tickets, with Albert contributing $3, Bruce contributing $2 and Caroline contributing $2. They agree that if they win anything with any of these tickets, the winnings are to be shared out in the same ratio as their contributions. They win $175. How much does each get?

- ○ Albert gets $105, Bruce gets $35 and Caroline gets $35
- ○ Albert gets $85, Bruce gets $40 and Caroline gets $40
- ○ Albert gets $85, Bruce gets $45 and Caroline gets $45
- ○ Albert gets $75, Bruce gets $50 and Caroline gets $50
- ○ Albert gets $65, Bruce gets $55 and Caroline gets $55

In order to make 1 liter of stone paint, Navin needs to mix 3 parts (30%) of red paint, 5 parts (50%) of yellow paint and 2 parts (20%) of blue paint. If Navin has 24 liters of red paint, 40 liters of yellow paint and 6 liters of blue paint, how many liters of stone paint can Navin make?

- ○ 6 liters
- ○ 24 liters
- ○ 30 liters
- ○ 120 liters
- ○ 200 liters

Next

You must answer each question before you can continue.

### C.7. Debrief and Payments

Following the task, we gathered subjects comments, socio-demographic information, and informed them of the payment they would receive.

## Questionnaire - Comments

If you have any comments for the experimenters running this HIT, please leave them below. This question is optional.

Click 'Next' to complete the task.

Next

# Questionnaire - Socio-Demographics

Please enter your age:

Please state your sex:

- ○ Male
- ○ Female

What is the HIGHEST LEVEL OF EDUCATION that you COMPLETED in school?

- ○ None or Primary Education: Primary School (grades 1-6)
- ○ Lower Secondary Education: Middle School or some High School incomplete
- ○ Upper Secondary Education: High School
- ○ Business, technical, or vocational school AFTER High School
- ○ Some college or university qualification, but not a Bachelor
- ○ Bachelor or equivalent
- ○ Master or Post-graduate training or professional schooling after college (e.g. law or medical school')
- ○ Ph.D or equivalent

Choose the field that best describes your PRIMARY FIELD OF EDUCATION.

- ○ Generic
- ○ Arts and Humanities
- ○ Social Sciences and Journalism
- ○ Education
- ○ Business, Administration and Law
- ○ Computer Science, Information and Communication Technologies
- ○ Natural Sciences, Mathematics and Statistics
- ○ Engineering, Manufacturing and Construction
- ○ Agriculture, Forestry, Fisheries and Veterinary
- ○ Health and Welfare
- ○ Services (Transport, Hygiene and Health, Security and Other)

Next

You must answer each question before you can continue.

## Payouts

You earned $12.50.
This consists of the automatic $2.00 payment for completing the HIT, and $10.50 that will be paid to you as a bonus.

Click 'Next' to continue to the comments section. You must do this to complete the task and receive your payment.

Next

## Task Complete

You have completed the HIT. Your completion code is:

9c64c7c9-cbc7-42eb-ad25-d798af4ba97f

Please copy/paste this into the space provided on the initial HIT page. You must do this in order to receive your payment.