Learning versus Unlearning:
An Experiment on Retractions

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Abstract. Widely discredited ideas nevertheless persist. Why do people fail to "unlearn"? We study one explanation: beliefs are resistant to retractions (the revoking of earlier information). Our experimental design identifies unlearning—i.e., updating from retractions—and enables its comparison with learning from equivalent new information. Across different kinds of retractions—for instance, those consistent or contradictory with the prior, or those occurring when prior beliefs are either extreme or moderate—subjects do not fully unlearn from retractions and update less from them than from equivalent new information. This phenomenon is not explained by most of the well-studied violations of Bayesian updating, which yield differing predictions in our design. However, it is consistent with difficulties in conditional reasoning, which have been documented in other domains and circumstances.

Keywords. Belief updating, retractions.

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1. INTRODUCTION

Retracted information often continues to influence beliefs, even once widely discredited. The claim of a connection between vaccines and autism was made in an infamous study in *The Lancet* in 1998. While the study was subsequently retracted in 2010, due to fraud, 29% of adults continue to believe in the connection (National Consumer League, 2014), with prominent adherents fully aware of the retraction. In another example, early in the COVID pandemic, multiple health organizations announced that there was no need to wear masks—incorrect information, conveyed to the public to ensure that health professionals would have access to supplies. Once the recommendation was later reversed, many continued to believe the original, incorrect message.

Why, in these cases, was it easier to learn the (incorrect) information than it was to subsequently unlearn it? Misinformation is inevitable and influences beliefs. Even in science, information thought to be true is sometimes subsequently found to be false. How do people unlearn information when it is shown to be incorrect? More generally, people can learn either from new information, or from the correction of previous misinformation. While there are many studies of the former, we know little about the latter, and whether the two work differently. Understanding how people unlearn matters for the debate about misinformation, both its harm as well as how to combat it: should we emphasize the error of the original information, or instead the correct alternative information?

This paper identifies and analyzes one hypothesis for this asymmetry between learning and unlearning: beliefs display greater inertia to information in the form of a retraction—an amendment of earlier information—rather than information which is *directly* informative about the state. As an illustrative example, imagine hearing gossip that an acquaintance committed scientific fraud, leading you to doubt their findings. Our hypothesis would imply that, upon learning that the gossip was baseless, lingering doubts about their work may remain; you may not fully "unlearn" what you heard, in response to the retraction.

To test this hypothesis, we present an experimental design which allows us to measure (un)learning from retractions, and to compare it to learning from informationally equivalent new signals about a state. We show that, across a broad set of cases, information still has residual impact even once retracted—retractions are not fully effective—and also that retractions are treated as less informative than equivalent new signals.[1] Our results are consistent with "information about past information" being more difficult to interpret and internalize than information that is directly informative of the state, even if the informational content is otherwise identical.

Our design is deliberately abstract, for reasons described below, and is a variation on a classic balls-from-urns experiment. We present subjects draws of colored balls (blue or yellow) from a

[1] By *equivalent* we mean yielding identical Bayesian posterior about the state given the same prior.

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, the color of the truth ball, allowing us to make two comparisons of particular interest: (1) beliefs following retractions versus beliefs without observing the retracted signal in the first place—testing whether retractions work; and (2) beliefs following retractions versus beliefs following new draws which yield identical Bayes updates (i.e. a draw of the opposite color to that which is retracted)—testing retractions versus equivalent new signals.

Our first result is that subjects fail to fully unlearn from retractions. Comparing beliefs after an earlier signal has been retracted, to beliefs after a comparable history in which the retracted signal was not observed to begin with, we find that beliefs consistently display a residual effect of the retracted signal—they are biased in the direction of the signal which was retracted. Moreover, retractions have less of an effect on beliefs than equivalent new information: comparing beliefs after a retraction, to beliefs after a new draw of the opposite color—to events which are informationally equivalent—beliefs update more in response to the new draw. 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 moderate or strong.[2] As a result, we do not believe these results are overly sensitive to particular design details.

This bias in updating from retractions interacts with other biases. When updating from new draws subjects (slightly) over-infer from signals and do more so when signals confirm the prior, whereas when updating from retractions they under-infer and exhibit anti-confirmation bias. Belief updating from retractions exhibits the opposite biases when compared to updating from new draws, a conclusion which is robust across all specifications.

Why are retractions less effective? As a first take, we report decision times when updating from retractions versus equivalent new information, which show that subjects take 10% longer on average after retractions, suggesting that they are more difficult. To dig further into why, we do three further analyses. First, in one treatment arm of the experiment, we randomly select subjects to only elicit their beliefs after the retractions, not after each draw. We then compare beliefs

[2]We broadly replicate the key findings from the literature on belief updating from new information, and thus feel comfortable using prior results to aid in interpreting some of our findings (despite the fact that our design accommodated retractions, the novel feature).

after retractions in this arm to those in the main arm, where beliefs were also elicited before the retraction. The hypothesis is that the residual effect of retracted information may be stronger if it has previously been acted upon and hence potentially internalized, for which we identify a precise null effect. Second, we conjecture that the effectiveness of retracting a signal may be different for a signal received earlier rather than later. By comparing when the first draw is retracted to when the second draw is retracted, we find that retractions are slightly more effective for information which has been more recently received. Third, we hypothesize that retractions may have different effect when they are received earlier rather than later. Comparing across similar histories of draws, permuting the timing of the retractions, we find no effect retractions happening earlier rather than later.

The design also enables us to study updating after retractions. Our results—consistent across all specifications—suggest that beliefs are more sensitive to new signals after a retraction, compared to both had the retracted signal never been received and to after an equivalent new signal. Moreover, decision times are slightly longer updating from new information after a retraction, indicating that it requires more effort in processing information in the form of retractions.

While abstract, we believe that our design represents the kinds of situations described in introductory examples. Moreover, the design allows us to speak to many practically relevant questions which we would not be able to replicate in a less abstract environment. First, and most importantly, we wish to show that the diminished effectiveness of retractions is a general phenomenon, not tied to the details of any particular domain (which typically motivates economists' interest in similar designs). As we discuss below, the closest precedent for our experiment comes from the voting literature. 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; by contrast, we find this effect even without motivation. Second, we leverage the fact that we can quantify objectively correct beliefs, which is difficult or impossible to do directly in domains where beliefs are subjective or, perhaps more problematically, not well-formed. Third, we can compare retractions to other pieces of equivalent information; again, when information lends itself to a more subject interpretation, as in the examples above, it is difficult to determine what pieces of information should be equivalent to a retraction. Fourth, our design allows us to replicate and compare with many other findings from the literature on biases in belief updating, showing that the failure of retractions to be a distinct phenomenon (e.g., not a simple case of base-rate neglect). Fifth, we are able to incentivize responses, which typically improves accuracy and reliability. While we acknowledge some of these could be addressed in other creative designs, doing all simultaneously seems important, and doing this elegantly seems infeasible without abstraction.

What could explain our experimental results? Our experiment is one of a small number which study how beliefs respond to *information about information*. Readers familiar with the experimental literature on belief formation could arrive at a variety of conjectures regarding whether and why retractions are distinct from otherwise equivalent information. Nevertheless, as our theoretical discussion illuminates, the failure of retractions cannot be seen as simply being due to other biases which do not make use of this kind of conditional reasoning which retractions necessitates. Instead, our analysis shows that the bias is due to retractions *in themselves* being more difficult to internalize. Our findings are consistent with additional thinking costs emerging when information is presented as a retraction. That said, our analysis goes beyond simply positing that these updating frictions exist, as we exploit that our design also allows us to compare different kinds of retractions. While we believe documenting that retractions are treated as distinct is a contribution in itself, we also seek to reconcile this finding with other potential explanations one may arrive at from extrapolating from other experiments and models. And indeed, in the course of the analysis, we do uncover other determinants which appear (or do not appear) to influence the effectiveness of retractions.

We believe these results validate our focus and show the power of our design to speak to practically relevant issues. And we believe that the observation the retractions are *fundamentally less effective* has significant practical value as well. Taken together, we believe our results provide important guidelines regarding how individuals can be expected to update about information about information, and we hope these patterns will be helpful for those who are regularly involved in communicating information to the public. In particular, our results show that this finding is a general one, not tied to any particular domain. This last point has enormous practical relevance. A policymaker deciding whether to provide guidance they may want to correct later should understand that this may not be so easy, even if "this time seems different from other cases." This paper documents that typically it is unreasonable to expect a retraction to simply involve a "deletion" of a piece of information. Indeed, our suspicion is that in many instances the inability to correct retractions ex-post would have changed the calculus regarding the value of disseminating information. By showing that it is harder to update from retractions relative to other kinds of information, our hope is that communicators will be better able to convey information and thereby limit the channels through which incorrect beliefs propagate.

1.1. Literature Review of Related Experimental Evidence

Our paper contributes to an extensive literature in experimental economics on errors in belief updating; an authoritative and comprehensive survey on this vast literature can be found in Benjamin (2019). As we discuss, we replicate many of the key findings from this literature which he documents, such as base-rate neglect and confirmation bias.[3] However, most related to the

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

present study are experiments on the failure of contingent reasoning. Charness and Levin (2005) was among the first papers to study the influence of such contingent reasoning in winners curse settings, one of the first documented failures of contingent reasoning. They find that transforming the problem to one where contingent reasoning is not necessary improves participant performance. Esponda and Vespa (2014) find, in a strategic voting experiment, that subjects have difficulty extracting information from hypothetical scenarios. Martínez-Marquina et al. (2019), like Charness and Levin (2005), study the "acquire a company" game and decompose difficulties with contingent reasoning into a complexity component and a uncertainty component, finding evidence for the existence of the latter by comparing to a deterministic treatment. Enke (2020) documents that many subjects consistently fail to account for the informational content from the *absence* of a signal, suggesting a failure of contingent reasoning. We note that, with the exception of Enke (2020), these papers all study cases where information has some instrumental use, whereas in our design information is only helpful in terms of forming beliefs in a *pure prediction* setting.[4]

As alluded to above, an experimental literature on reactions to news has similar motivation as ours. However, it is crucial that our main treatment does not contain motivated reasoning, as may be entailed by political stories. Therefore, this suggests a different channel than the focus of these papers. Substantial evidence exists that motivated reasoning is important in political settings; Thaler (2020) shows that political beliefs strongly predict that political identity influences how subjects react to information about pieces of news.[5] His design, however, explicitly seeks to eliminate inferences about source veracity, in contrast to ours.[6]

Above, we discussed how our results are consistent with added difficulties when updating from "information about information" compared to "direct information." This phenomenon is similar to what Miller and Sanjurjo (2019) refer to as the *Principle of Restricted Choice*, whereby subjects fail to condition on the data generating process behind the *source* of a piece of information. They draw a connection between this bias and the mistakes subjects make when facing

[4]Of course, information is arguably instrumental in our case as well, insofar as payments depend on reports through the scoring rule. Nevertheless, our focus on a pure prediction setting is a distinguishing feature.

[5]Other experimental papers in political science study the extent to which voters are able to rationally update beliefs include Huber et al. (2012) and Taber and Lodge (2006). These replicate certain biases in information processing, but are not about retractions per se. Angelucci and Prat (2020) studies memory of news in a long-term survey and find evidence that political leanings influence which stories are likely to be remembered.

[6]For other papers studying motivated reasoning in different domains outside of politics, see Eil and Rao (2011), Mobius et al. (2013), Coutts (2019b), Grossman and Owens (2012) and Oprea and Yuksel (2020). Conlon et al. (2021) use a similar design to study whether husbands and wives update their beliefs in the same way following information from each other versus information from strangers. A seminal theoretical contribution in this area is Brunnermeier and Parker (2005), which studies a decisionmaker who faces consumption utility, as well as *anticipatory utility* from future consumption, and show it may be optimal for a decisionmaker to induce biased beliefs in order to maximize the latter. From a decision theoretic perspective, Kovach (2020) studies preferences over acts and derives axioms which characterize wishful thinking, under an assumption which focuses the setting on cases where subjective expected utility holds. See Benabou and Tirole (2016) for a survey of this literature more generally.

the *Monty Hall Problem* (Friedman, 1998), a classic problem where subjects consistently guess mistaken probabilities.[7] Biases of this form have been found anecdotally in many cases.[8] From this perspective, our results can be interpreted as exhibiting that, when related to this kind of conditional reasoning, belief updating involves additional costs not necessarily present when an initial signal is received. Furthermore, while this bias can indeed explain the updating pattern we identify—that is, the dampening effect with respect to retractions—one of our contributions is to show how the effectiveness of retractions varies with other parameters of the environment, something this theory does not directly speak to; for instance, whether a retraction is contradictory or confirmatory, or whether beliefs are extreme or moderate. We are not aware of any designs which have exogenously varied the extent to which a decisionmaker faces a problem of the Monty Hall form; instead, this literature typically takes the mathematical problem as given, and instead varies other circumstances around it such as incentives (Palacios-Huerta, 2003) or how it is presented and explained to participants (James et al., 2018); thus, it is not clear how to reconcile this effect against others, a concern which would be practically relevant in many cases.

It is also worth mentioning that other biases have been documented in belief updating that we view as distinct from the phenomenon identified and which our design allows us to avoid studying. One such bias is *base-rate neglect*, whereby agents underweight the prior when updating their beliefs; see, for instance, Esponda et al. (2020). We do not vary the prior directly in order to keep our design symmetric; on the other hand, are *are* able to determine how much an initial guess influences subsequent beliefs. Another is *confirmation bias*, whereby subjects interpret information more favorably when it coincides with their initial beliefs. Such a bias is modelled theoretically by Rabin and Schrag (1999). Charness et al. (2020) experimentally study how players choose among potentially biased sources of information; however, our design features exogenous information, in contrast to theirs. We note that we *are* able to replicate several of these results in our sample; however, they are not are primary focus.

[7] In the Monty Hall Problem, a subject is asked to choose one of three doors, with one of the three hiding a prize. After making an initial choice, subjects learn which of the doors they did *not* select has *no* prize behind it. Subjects are then offered to switch their choice. Friedman (1998) shows that subjects—with striking consistency—choose to keep their choices, even though this has a lower probability of a prize. We note that many factors are at play in this setting, such as an illusion of control. Indeed, as far as we have been able to tell, the relevance of this problem is entirely through the Principle of Restricted Choice. See also Borhani and Green (2018), who study Monty Hall settings and characterize where an inferential bias may lead to information processing errors.

[8] See also the Bertrand Box paradox and the Boy-or-Girl Paradox for similar cases where individuals with significant probability training can arrive at the incorrect answers.

2. FRAMEWORK

This Section presents our formal definition of a retraction, and includes our main framework and hypotheses.

2.1. Defining Retractions

Consider a Bayesian decision maker learning about a state of the world, which for simplicity we take to be binary, say $\theta \in \{-1, 1\}$, with prior probability p_θ; in the experiment, we take $p_\theta(1) = 1/2$. Our interest is in cases where the decisionmaker repeatedly observes independently and identically distributed signals. In particular, a signal s_t can either be *truth* or *noise*, that is,

$$s_t = n_t \cdot \epsilon_t + (1 - n_t) \cdot \theta.$$

Here, $\epsilon_t \in \{-1, 1\}$ and equals 1 with probability p_ϵ, and $n_t = 1$ whenever the signal is given by the independent "noise" ϵ_t, an event which occurs with probability p_n. When $n_t = 0$ (which occurs with with complementary probability), we have that $s_t = \theta$ and we refer to the signal as "truth;" in our experiment, we take $p_\epsilon = 1/2$ to maintain simplicity. Putting this together, the probability of observing a signal s_t given the state θ is given by

$$p_s(s_t \mid \theta) = p_n \cdot p_\epsilon(s_t) + (1 - p_n) \cdot \mathbf{1}[s_t = \theta]$$

Letting s^t denote the set of signals $\{s_1, \ldots, s_t\}$, we have that the posterior of a Bayesian decision maker about the state of the world θ is given by

$$p_\theta(\theta \mid s^t) = \frac{p_\theta(\theta) p_s(\theta \mid \theta)^{\#\{s_t = \theta\}} p_s(-\theta \mid \theta)^{\#\{s_t = -\theta\}}}{p_\theta(\theta) p_s(\theta \mid \theta)^{\#\{s_t = \theta\}} p_s(-\theta \mid \theta)^{\#\{s_t = -\theta\}} + p_\theta(-\theta) p_s(\theta \mid -\theta)^{\#\{s_t = \theta\}} p_s(-\theta \mid -\theta)^{\#\{s_t = -\theta\}}}$$

Definition 1. *A retraction is any signal informing the agent that a past observation s_t was noise.*

We believe this definition is in line with colloquial usage. While we specialize the definition to our main information arrival process of interest, it is straightforward to extend to the case where the data generating process does not reveal the *truth* of a state; it simply involves informing that the decisionmaker that a past signal was not reflective of the state.

In order to update beliefs as a Bayesian following a retraction, the decisionmaker must know how the retraction is generated, that is, how the retracted signal was chosen. Let τ be the period corresponding to the retracted signal s_τ. In this paper, we will focus on *verifying retractions*: these are retractions which involve first selecting a signal s_τ, and subsequently revealing to the decisionmaker whether this signal is noise or not.[9] Formally, verifying retractions are those such that $\{\tau = t\}$ and $\{n_t = 1\}$ are independent events. Note that a Bayesian decisionmaker should be able to follow Bayes rule and update beliefs following retractions without any ambiguity.[10] Also note that, for verifying retraction, we have that updating from a retraction is equivalent (as per Bayes rule) to "disregarding" the retracted signal:

$$p_\theta(\theta \mid s^t, n_\tau = 1) = p_\theta(\theta \mid s^t \setminus \{s_\tau\})$$

2.2. Belief Updating Biases

A number of studies referenced above find that subjects underweight information. If b_t is a subject's belief in period t about θ in period t, then the Bayesian belief update would yield a constant change in the log odds ratio. Let us write:

$$\log\left(\frac{b_{t+1}(s)}{b_{t+1}(-s)}\right) = \log\left(\frac{b_t(s)}{b_t(-s)}\right) + K_1 \mathbf{1}[s = s_t] - K_2 \mathbf{1}[s = -s_t]. \tag{1}$$

Given symmetric noise ($p_\epsilon = 1/2$), for a Bayesian under our information arrival process, $K_1 = K_2 = \log\left(\frac{2 - p_n}{p_n}\right) =: K$. The literature mentioned above, however, has found that:

- $K > K_1, K_2$, and
- $K_1 > K_2$ if the subject obtains additional utility when $\theta = s_t$ (and visa versa).

Various microfoundations have been proposed which would yield these biases. The first can be rationalized, for instance, by paying a "thinking cost" to choosing K, which would prevent K_ℓ from equaling K. A model of belief-based utility could induce the asymmetry in K_ℓ. To understand the theoretical difference of a retraction, note that a subject being informed that a signal were noise would not have any differential update. So, the first effect of a retraction is simply to "disregard" such noisy signals. However, there is also a second effect, caused by the need to infer from the retraction itself. Let $\alpha(\tau \mid s^t) = \frac{\mathbb{P}[\text{Retraction of } s_\tau \mid s^t, \theta = 1]}{\mathbb{P}[\text{Retraction of } s_\tau \mid s^t, \theta = -1]}$. With symmetric noise, if signal s_τ is retracted, the Bayesian update should be:

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

[9] Note that this kind of retraction may indeed lead to the subjects learning that their past information was actually *accurate*; however, these signal realizations are (in principle) degenerate at certainty, and so we do not use any such reports in our analysis in any significant way. Our companion paper studies a version of this design which *does* allow us to study such positive verification directly.

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

where $\eta_t := \sum_{\ell=1}^{t} s_\ell$. So, for a retraction, the log odds update is now:

$$\log\left(\frac{b_{t+1}(s)}{b_{t+1}(\neg s)}\right) = \log\left(\frac{b_t(s)}{b_t(\neg s)}\right) + K_1 \mathbf{1}[\neg s \text{ retracted}] - K_2 \mathbf{1}[s \text{ retracted}] + \log(\alpha(\tau \mid s^t)), \quad (2)$$

which is identical except for the $\log(\alpha(\tau \mid s^t))$ term. Now, $\alpha(\tau \mid s^t) = 1$ (and hence $\log(\alpha(\tau \mid s^t)) = 0$) for all verifying retractions. The purpose of our experiment is to understand whether subjects treat $\alpha(\tau \mid s^t) = 1$. We also wish to understand whether the *reasons* for any departure are due to the same biases which would lead to any of those biases identified in prior work.

One final comment prior to the analysis: We will be interested in both belief levels, as well as log odds, in our analysis, and think each measurement has merits. However, studying whether $\log(\alpha(\tau \mid s^t)) = 0$ in (2) to infer whether retractions are different does not require the decision-maker to be a Bayesian. This observation is due to a result of Cripps (2019). The author shows that as long as the decisionmaker's rule is "divisible" (which roughly states signals are treated as exchangeable), the updated belief must be found via a transformation of the prior, Bayes rule applied to the transformation, and an inverse transformation. Thus under any updating rule satisfying that paper's axioms, this result therefore implies that the reported belief b_t is a function of the Bayesian belief, and that b_{t+1} is the inverse of the Bayesian update of the transformed belief; that is, we have for some monotonic f, belief updates can be determined via the following relationship:

$$\log\left(\frac{f(b_{t+1}(s))}{1 - f(b_{t+1}(s))}\right) = \log\left(\frac{f(b_t(s))}{1 - f(b_t(s))}\right) + K_1 \mathbf{1}[\neg s \text{ retracted}] - K_2 \mathbf{1}[s \text{ retracted}] + \log(\alpha(\tau \mid s^t)), \quad (3)$$

The key observation from this expression is that, since $\log(\alpha(\tau \mid s^t)) = 0$, no model of "as-if" Bayesian updating would explain any difference due to retractions. Indeed, this observation would hold for *any* f—including those that may rationalize under-updating, for instance. We record this result as follows:

Proposition 1. *Consider any (possibly non-Bayesian) updating rule, which emerges as the result of "Bayesian updating under a transformation." Any such decisionmaker would have an equivalent* log *odds update following a retraction of a signal* s *compared to a new signal* $\neg s$*, provided* $\alpha(\tau \mid s^t) = 1$.

Even when focusing on levels, however, this result strengthens the interest in our design as stepping beyond the normal boundaries of Bayesian updating. Any differential effect we find would not be consistent with any updating rule that satisfies exchangeability. This will be helpful in distinguishing our results from similar analyses in the literature.

2.3. Hypotheses

The purpose of this paper, simply put, is to understand patterns in $\alpha(\tau \mid s^t)$. Insofar as our view, informed by the anecdotal evidence mentioned in the introduction, is that retractions are indeed more difficult to process, this suggests our first hypothesis:

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

Despite the fact that our anecdotal evidence is highly suggestive that this hypothesis should hold, it should not be immediately apparent that we should be guaranteed to find this in our design. A priori, it could very well be that our setting is *too* stripped down and has therefore eliminated whatever aspects of these applications is responsible for the ineffectiveness of retractions. If subjects are always more likely to make mistakes following more signals, for instance, then a retraction may be easier to internalize since it suggests that a subject only need to consider a smaller number of signals.

The substance of the hypothesis, then, is that we attribute a distinctive effect to retractions themselves relative to new signals. One expects, from prior work, that $K_\ell < K$ when regressing log odd updates on log odd initial beliefs. However, we are not interested in testing Bayesianism per se; we expect to find various biases in our samples. Our point is that retractions lead to less aggressive belief updating.

On the other hand, the complexity of belief updating is also often used in order to explain the emergence of other commonly studied biases commonly. Insofar as retractions might be harder to internalize, this suggests our second hypothesis:

Hypothesis 2 (Retractions accentuate biases)**.** *Updating from retractions accentuates biases in updating already present in updating from new information—for instance, excessive conservatism.*

As part of testing this hypothesis, it will be important to show that we do in fact find the same kinds of biases in our sample as in other work, and indeed this will be the case.

We then test for the above-mentioned explanation as to why retractions may work differently from new signals. More specifically, we examine whether more time is spent in updating from retractions relative to new signals, considering decision time as a proxy for difficulty in processing the information.

Hypothesis 3 (Retractions are harder to process)**.** *Updating from retractions takes longer.*

The remainder of our hypothesis relate to the *relative* effectiveness of retractions. A natural question, assuming retractions are indeed less effective, is to understand when this is more likely to be the case. One natural question depends on whether it depends on how well-formed beliefs are. Does the fact that subjects are required to report their beliefs after every signal make their beliefs harder to move? Given that we posit retractions are harder to update, a conjecture would be that our elicitation step increases these updating costs even further.

Hypothesis 4 (Retracting internalized signals)**.** *The effect of retractions on belief updating is weaker when agents acted upon the observed signals.*

Relatedly, insofar as our experiment involves dynamic information arrival and correspondingly may involve dynamics in terms of the difficulty of updating, one might expect there to be some interaction with the timing of retractions (as well as the timing of the signal that is retracted). Our first hypothesis says that which *signal* is retracted (i.e., one that was observed early on versus one that was observed later). Our second hypothesis says that a retraction that is experienced earlier (i.e., after fewer signals are observed) should have a different impact on belief updates compared to a retraction that is observed later (i.e., after more signals are observed):

Hypothesis 5 (Timing of retractions)**.** *Experiencing a retraction later leads to a different impact on beliefs compared to when the agent experiences it earlier, fixing the same history of events, up to order.*

Hypothesis 6 (Timing of retracted signals)**.** *The effect of retractions on belief updating differs depending on when the retracted signal was originally observed.*

Our last hypothesis posits that, insofar as a retraction is more difficult to process, this might would suggest that it is more difficult to update following a retraction—for instance, if there is some non-linearity in updating costs which makes updating more difficult if more costs have been incurred:

Hypothesis 7 (Signals after retractions)**.** *Subjects update from signals differently, depending on whether or not a signal has been retracted.*

Taken together, our first set of hypotheses posit that subjects may display something similar to an "endowment effect for past signals." That is, a decisionmaker observing a particular signal s_t, having incurred a cost to update their beliefs, would be resistant to deleting or internalizing it. In this light, several of our subsequent hypotheses hinge on what the shape of these costs may be (e.g., is it even more costly to update after some costs have been incurred). Accordingly, a natural question is whether the effectiveness of retractions depends only on the beliefs themselves, or how the decisionmaker has used information, or preferences over beliefs. While these elements are (to varying degrees) known to influence Bayesian updating, as Proposition 1 makes clear, this is orthogonal to the question of whether it influences the effectiveness of retractions.

2.4. Outline for Analysis of the Hypotheses

We describe our basic design first, which describes the basic method of drawing retractions which subjects faced during the experiment. Subsequently, we describe additional details of each round of the experiment, including exact variations considered and other details. Subjects were provided all information regarding how observations would be drawn and compensation would be provided. See Appendix B for the instructions as presented to the subjects in the experiment.

3. EXPERIMENTAL DESIGN

The purpose of the experiment is to study how people learn from retractions. There are two main questions. First, are retractions effective: once information is retracted, do people behave as if they never received the information to begin with? Second, is learning from retractions inherently different from learning from new signals? In answering each of these two questions, we also aim to provide evidence on when and why.

We purposefully test these questions in an abstract setting, one of drawing colored balls from urns. Existing experiments on retractions are in settings where many possible mechanisms are at play, for example the retraction of politically polarizing newspaper articles, where motivated reasoning and complicated inference regarding the strategic incentives of others are likely to be at play. We wish to test whether there is an underlying, cognitive difference in processing retractions, free of such confounding mechanisms.

The basic data generating process in the experiment is as described in the previous Section. We implemented it as follows. Subjects play multiple rounds of the experiment. At the start of each round, a state is drawn, which we refer to either as "yellow" or "blue," with each state being equally likely. The state refers to the color of a particular ball, which throughout the experiment we refer to as the *truth ball*. Subjects are told that the truth ball is placed in a box which additionally contains *noise balls*; these are yellow and blue in equal proportion.

Rounds consist of multiple *periods*. In each period, subjects receive a new piece of information, either a new signal or a retraction. A *new signal* corresponds to a ball being drawn from the box, with replacement, with subject told the color of the ball but not whether it was the truth ball or a noise ball. A *retraction* corresponds to informing the subject of whether a past draw was a noise ball, in which case it (generally) contained no information regarding the state.[11]

At the end of each period—that is, after each new piece of information—subjects report their belief regarding the probability of the truth ball being blue or yellow with these reports

[11] In another experiment, in which we tested an alternative type of retraction, in certain conditions the retraction did provide some information about the state, via a Monty Hall argument. The results are largely similar and available from the authors upon request.

incentivized as detailed below. Comparing beliefs after draws are retracted, to beliefs before those draws happened, allows us to test whether retractions are effective. Comparing changes in beliefs in response to retractions, to changes in beliefs in response to equivalent new signals, allows us to test whether learning from retractions is different from learning from new signals. Indeed, a key aspect of our design is that a new draw of one color is informationally equivalent, for a Bayesian, as a retraction of the opposite color—what matters is the net number of balls of one color.

We ran the experiment on Amazon Mechanical Turk (henceforth MTurk) in June 2020.[12] The experiment had several sub-treatments, as described in detail below, focused on verifying retractions. Subjects were provided all information regarding how observations would be drawn and compensation would be provided. See Appendix B for the instructions as presented to the subjects.

In the rest of this section, we describe additional details of each round of the experiment, which are also explained in figures 1 and 2.

3.1. Baseline Setup

Our experiment focused on verifying retractions.[13] In the main treatment, each round had four periods, with beliefs elicited at the end of each round. The sequence of events was the following:

- At the start of the round a truth ball is chosen at random (with probability .5 it is yellow, .5 it is blue) and placed into the box with two yellow noise balls and two blue noise balls (corresponding to $p_n = 4/5$ in the information arrival process).
- In each of periods one and two, the subjects observes a draw from the box, with replacement, as described above. They are told the color of the ball but not whether it is the truth ball or a noise ball.

[12] All our subjects were recruited in this way. By now, using MTurk itself does not appear to be a distinguishing feature; however, our scale is somewhat larger than a typical study, and our design replicates certain documented phenomenon from lab experiments using MTurk. In our case, the plethora of possible belief paths implies that a lab experiment would be subject to a nontrivial amount of additional sampling noise, in that certain paths may not be observed with sufficient frequency given a smaller participant pool. Given existing evidence, it is hard to document cases in the experimental economics literature where the results were driven by the use of venue; see, for instance, Landier et al. (2020) and Martínez-Marquina et al. (2019). However, we acknowledge, as Snowberg and Yariv (2020) note, there may nevertheless be a tradeoff between the noise in the MTurk participant pool and other commonly used participants such as university students. We determined that for our design, the ability to easily recruit additional subjects outweighed the costs. While perhaps less novel in terms of economic implications, we nevertheless believe that these replications are important, given the anticipated growth of this platform in future work.

[13] As we mentioned, in ongoing research we also ran a version of this experiment using falsifying 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 $\log(\alpha(\tau \ j \ s^t)) = 0$. These results are available from the authors upon request.

- In each of periods three and four, subjects are provided a new draw (as above) with probability .5, or a verifying retraction with probability .5. A verifying retraction worked by choosing one of the prior draws at random and informing the subject whether it was a noise ball—a "retraction"—or a truth ball. If it was revealed that the ball was a truth ball, the round was stopped, as at that point the state was fully revealed. The probability that they would receive each of these signals was determined independent across periods.

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 Appendix B.1. 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.

Comparing beliefs after a retraction in period 3, to the equivalent beliefs in period 1, allows us to test whether retractions are effective. Since a retraction of one color in period 3 is equivalent, from a Bayesian perspective, to a new draw of the other color in period 3, comparing updating under each of these events allows us to test whether learning from retractions is somehow different from learning from new signals. Finally, comparing updating in period 4 after a retraction in period 3 to updating after a new draw in period 3 (and also to updating in period 2) allows us to test whether retractions affect subsequent updating.

3.2. Single-Elicitation Design Treatment

Our experiment also featured an across-subject treatment. At the start of the experiment, each subject was randomly allocated to one of two treatments. With 2/3 probability they were allocated to the *intermediate-elicitation treatment*, exactly as described above. With 1/3 probability, they were allocated to the *single-elicitation treatment*, which differed in several ways.

In the single-elicitation treatment, subjects would observe two signals with probability $1/3$ and three signals with probability $2/3$. Most importantly, beliefs were only elicited at the end of each round, not at the end of each period. The sequence of events, summarized in figure 2, was as follows:

- At the start of the round a truth ball is chosen at random.
- The first two periods would always be new draws. Beliefs are not elicited after period 1. With probability 1/3, beliefs are elicited after period two, after which the round ends. With probability 2/3, beliefs are not elicited after period two and the round continues to period 3.
- If reached, period 3 is a new draw with probability 0.5 and a (verifying) retraction with probability 0.5.[14] Beliefs are elicited at the end of the period, after which the round ends.

[14]This motivates the asymmetry in whether subjects would observe two or three signals; this ensures we have an

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 signal.

This variant of the design tests whether requiring that subjects provide reports in *every* period affects the efficacy if retractions. One hypothesis is that forgetting information which has already been internalized or acted upon is difficult. If so, we may expect retractions to be more effective when beliefs have not already been elicited. This treatment has the obvious downside that we have substantially less data to populate belief paths, and so more subjects are needed to obtain similarly precise estimates. Furthermore, since we only wanted to obtain *one* report, we needed to vary the number of periods which comprised a round.

3.3. Other Experimental Design Details

Subjects needed to answer comprehension questions in the instructions correctly in order to proceed with the experiments. The questions summarized the key points the subjects needed to understand. We also asked additional questions on mathematical ability, which were incentivized by providing a 50 cent reward for every question answered right. Lastly, subjects were given two rounds of "practice" to familiarize themselves with the interface. These rounds simply showed the subjects examples of what they would do during the experiment, and were not incentivized.

We incentivized subjects to report truthfully using a binarized scoring rule (see Hossain and Okui (2013) and Mobius et al. (2013)). It is well known that elicitation methods are often difficult for participants in experiments to understand or appreciate; in the interest of transparency, we displayed in the interface exactly how a report would correspond to payoffs, which subjects would be able to see if they wished to. This avoided the need for subjects to do any computation on their own, since they would see exactly what the payouts would be as a function of the truth ball's color. However, in the instructions, we simply mentioned to subjects that we believed it would be in their best interest to report the truth, and did not require them to understand details of the payment scheme directly (or reasons behind its incentive properties). In order to determine the agent's payments, we used one of their reports within a single randomly selected period.

4. RESULTS

There are four sets of principle results. First, before turning to the contribution of our paper, we show that updating from new draws is similar in our experiment to what has been found in the existing literature, to validate our experimental setting. Second, we turn to updating from

equal number of subjects reporting after two new draws, after three new draws, and after two new draws following a retraction.

retractions: do retractions work; do people update differently from retractions versus new draws; and how do retractions interact with existing deviations from Bayesian updating documented in ball-draw experiments? Third, why do retractions fail? Fourth, how do retractions affect *subsequent* updating?

4.1. Methodology Overview

Our analysis leans on the simplicity of our experimental design to make the analysis as non-parametric as possible; however, at times we will add slightly more structure, following specifications used in the literature on belief updating. We broadly perform two different kinds of comparisons, explained visually in figure 3:

- Are subjects' beliefs after seeing a retraction the same as if the retracted signal had never been observed in the first place? We refer to these as *tests of unlearning*
- Do subjects treat retractions as having similar informational content as equivalent (in terms of Bayesian belief updates) new information? We refer to these as *comparisons to new information.*

When analyzing the first question, we compare the belief reports themselves, as only the level comparisons are relevant. By contrast, the second question relates to how beliefs *move* in response to retractions; for this, we report both differences in log odds, as well as levels. Levels has the advantage that extreme beliefs, near 0 or 1, 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.

Before presenting our analysis, it is worth discussing our identifying variation and how we aggregate the histories of signals in our analysis, explained visually in figure 3. For our tests of unlearning, when aggregating across histories we look at what we refer to as a *compressed history*; this involves removing any retracted ball draws, as if those events had never occurred (so, for instance, a history of *blue-blue-retraction* would be equivalent to *blue*). We included fixed effects for compressed history in our analysis, and hence compare beliefs within the same compressed history, before a ball draw was received compared to after it was retracted.

When comparing to novel information, we include fixed effects at the level of the lagged history (where the lag is compared to one draw before, so that we control for the initial beliefs prior to the observation of the signal). That is, this dummy groups histories which would lead to the same Bayesian belief updates prior to observing the new information (which may either be a retraction or a new draw). In addition, we include dummy variables for whether the signal is evidence for yellow or blue, whether the signal observed is a retraction, and an interaction term (i.e., whether the signal observed was evidence of yellow or evidence of blue). Our main interest for the comparison to novel information is in the sign of the interaction term. Insofar as we have no reason to suspect a retraction to be interpreted more favorably as evidence for one color over the other, we expect this dummy to be a 0 (given that we are controlling for history, and the symmetry of our design). However, a negative coefficient on the interaction term suggests that beliefs move less when a given signal is a retraction, controlling for history and for the sign of the signal.

4.2. Preliminary Observations on Belief Paths

As a first step, in part as a test of validity of experimental setting, we examine the belief paths of subjects when they are not shown retractions. In the absence of a retraction, the design is very similar to many others surveyed by Benjamin (2019). To start, 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.

One concern about our design is that the complexity would make it difficult for subjects to understand the instructions. On the other hand, we do find that subjects tend give reports that are consistent with predictions one would expect with the literature. Figures 4 and 5 in the Online Appendix present the distance the belief reports are from the truth in real and absolute terms; in both cases, we see that reports tend to be fairly close to the truth on average, despite the aforementioned lower effect. We note that while many subjects appear to misinterpret information by giving reports in the opposite direction on would expect,[15] the overwhelming majority of subjects do not make this mistake (or at least do not make it consistently).

We present an even more explicit comparison to this literature in Table 1, as well as Table 12 in the Online Appendix. Table 1 shows regressions of the reported log odd on the Bayesian results. Specifically, it shows the following specification, restricted to the cases where there has not been a prior retraction in the round (so only new draws):

$$l_t = \beta_0 + \beta_1 \quad l_{t \quad 1} + \beta_2 \quad s_t \quad K \tag{4}$$

and

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

where t is the period, l_t is the log-odds of the beliefs reported at t, s_t is the signal in round t (+1 or -1), $c_t := \mathbf{1} f \text{sign}(l_{t \quad 1}) = \text{sign}(s_t) g$ is an indicator function that equals 1 when the signal at t

[15] Subjects' change in belief reports goes against the signal 19.5% of the time.

confirms the prior at $t - 1$, and $K > 0$ is a constant factor of Bayesian updating.

The power of using a log odds framework is that a perfect Bayesian updater would move log odds by a constant amount, which depends only on the likelihood of each signal. Hence the above regression, regressing log-odds of belief reports on this log odds ratio would yield a coefficient $\beta_2 = 1$ for a Bayesian updater. Benjamin (2019) notes that this tends not to be the case: subjects tend to under-react to new information. For the two incentivized studies he reviews with sequential observations, the estimate on this coefficient is $.528$ times the likelihood. In contrast, in the most parsimonious of our regressions, we find this signal to be 1.321, indicating over-updating from new information.

Once we include the effect of confirmatory information, we uncover an interesting finding: the estimated coefficient on the likelihood becomes 1.042 (not significantly different from 1), and $\beta_3 > 0$. Together, this suggests that our subjects over-react to new information 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. Thus, while most of the studies report $\beta_2 < 1$, strict over-inference resulting from confirmatory information—that is, $\beta_2 + \beta_3 > 1$— has been previously documented (e.g. Charness and Dave, 2017).

The regressions also verify another deviation from Bayesian updating identified in the literature: subjects exhibit base-rate neglect. Or, in other words, they underweight the prior, as evidenced by $\beta_1 < 1$.

It is useful to keep these general patterns in mind below when interpreting our results; we emphasize that these findings we mentioned are essentially what one would expect based on the literature. It also suggests that, since we do find these biases in the "new information" treatment, any departure due to retractions cannot be attributed to explanations that do not use the nature of the information source.

We also run the following specification, interacting prior beliefs with both the signal and the signal interacted with whether it is confirmatory.

$$l_t = \beta_0 + \beta_1 \, l_{t-1} + \beta_2 \, s_t \, K + \beta_3 \, s_t \, K \, c_t + \beta_4 \, l_{t-1} \, s_t + \beta_5 \, l_{t-1} \, s_t \, c_t \tag{6}$$

Under this specification, $\beta_4 < 0 < \beta_5$ represents belief entrenchment, i.e. resisting revising beliefs according to information that goes against prior; this phenomenon is more expressive the more extreme the prior is.

To summarize, in our analysis of this data, we do not see any consistent departure from the prior literature on belief updating. Subjects do display under reaction to new information in general and typically move in the correct direction. We do not find any significant departures from the main conclusions of Benjamin (2019) in either experiment, and therefore do not have strong reasons to suspect our results are driven by, for instance, the choice of venue.

4.3. Updating from Retractions

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 signals. While we begin with the aggregate results, the richness of the design also allows us to break down the comparison of retractions to new signals across various belief paths, and hence to study how retractions interact with existing deviations from Bayesian updating.

4.3.1. Failure to "Unlearn" and Retractions Versus New Signals

Our first result, and the key finding of the paper, is that retractions are ineffective, in that (1) retracted signals are not fully disregarded and (2) beliefs are less responsive to retractions than new signals. The results on this are presented in Table 3. We run three tests, which look at how retractions both change the absolute magnitudes of the belief reports, as well as the change in the belief reports in response to the new information. The former can be seen in the first two columns, and the latter can be seen in columns three.

Our basic strategy for identifying the effects of retractions on belief updating is simple, as summarized in figure 3. In order to aggregate these simple effects across different histories, let us introduce some notation. Let H_t be the history up to time t, that is, the set of all the draws observed as well as which ones were retracted, fixing the order. We call the compressed history $C(H_t)$ the set of all draws observed excluding the retracted ones, but keeping the order fixed. For example, if at period 4 the history is $H_4 = (s_1, s_2, n_2 = 1, s_4)$—which $n_2 = 1$ corresponding to a retraction of the second draw—then the compressed history $C(H_4)$ is given by (s_1, s_4). We will denote F_{H_t} and $F_{C(H_t)}$ the fixed effects associated with the history up to time t and those associated with the compressed history.

The first of these tests assesses whether updating from retractions is equivalent to deleting the retracted piece of information. We do this by estimating the following equation:

$$b_t = \beta_0 + \beta_1 \quad r_t + \beta_2 \quad r_t \quad s_t + F_{C(H_t)}, \tag{7}$$

where r_t is an indicator variable for the signal being a retraction, and s_t gives the direction implied by the signal or retraction observed.[16] Controlling for the compressed history allows us to compare, for example, the beliefs after observing $\{s_1, s_2, n_2 = 1\}$ to those reported when only signal s_1 was seen.

[16]To be precise, if a signal with value s is retracted, then $s_t = \quad s$.

We then look at whether belief updating is the same for a retraction of a signal s as for an informationally-equivalent new signal $-s$. We do this by estimating two regressions. First:

$$b_t = \beta_0 + \beta_1 r_t + \beta_2 r_t \times s_t + \beta_3 s_t + F_{H_{t-1} \times \{s_t\}}, \tag{8}$$

where $F_{H_{t-1} \times \{s_t\}}$ denote the fixed effects associated with the lagged history interacted with the direction of the signal or retraction observed. Thus, simply from changing the fixed-effects, we can now compare beliefs reported after $\{s_1, s_2, n_2 = 1\}$ to those reported after $\{s_1, s_2, s_3 = -s_2\}$.

The third specification also compares retractions to new signals, but we instead consider the *change* in beliefs by estimating the following equation:

$$b_t = \beta_0 + \beta_1 r_t + \beta_2 r_t \times s_t + \beta_3 s_t + F_{H_{t-1} \times \{s_t\}}. \tag{9}$$

The key finding is that β_2, across all of the specifications we study, is negative—while beliefs do move following retractions, their effectiveness is dampened, since they move less strongly in the direction of the signal. This implies not only that retracted signals are not fully disregarded (column 1), but also that providing informationally-equivalent new signals is more effective in inducing changes in beliefs (columns 2 and 3). In short, retractions are treated *differently*, and in particular as if they were less informative.

4.3.2. Retractions are Harder to Process

We then look into whether retractions are harder to process as failures of contingent reasoning may result in $\log(\alpha(\tau \mid s^t)) \neq 0$. A natural proxy for processing difficulty is the subjects' decision time d_t, under the assumption that the greater the difficulty the longer the time taken to interpret the information being provided. To test this hypothesis we regress decision time on a dummy variable indicating whether or not a retraction occurs in that period. As before, we control for the lagged history interacted with the direction of the signal or retraction observed, such that we compare decision times for retraction to those of informationally equivalent new signals. We estimate

$$d_t = \beta_0 + \beta_1 r_t + F_{H_{t-1} \times \{s_t\}}, \tag{10}$$

as well as another variant of this equation using log decision time as the independent variable instead.

The results, which can be seen in Table 4, confirm our conjecture: subjects take longer to report their beliefs when updating from retractions. Thus, the data suggests that retractions are not only treated differently, they are harder to process.

4.3.3. Variation Across Paths: Retractions Accentuate Biases in Updating

Having illustrated that retractions are treated differently, with beliefs reacting less on average, we then seek to determine when this effect is relatively more or less pronounced. Specifically, we now adopt a similar regression specification to prior ball-draw experiments, and examine heterogeneities in the relative effect of retractions across different belief paths.[17] Specifically, we run variants of the following regression, which uses log odds as the dependent variable to replicate existing papers on deviations from Bayesian updating:

$$\begin{aligned} l_t = \beta_0 + \beta_1 \;\; l_{t \;\; 1} + \beta_2 \;\; s_t \;\; K + \beta_3 \;\; s_t \;\; K \;\; c_t + \beta_4 \;\; l_{t \;\; 1} \;\; s_t + \beta_5 \;\; l_{t \;\; 1} \;\; s_t \;\; c_t + \\ + r_t \;\; [\gamma_0 + \gamma_1 \;\; l_{t \;\; 1} + \gamma_2 \;\; s_t \;\; K + \gamma_3 \;\; s_t \;\; K \;\; c_t + \gamma_4 \;\; l_{t \;\; 1} \;\; s_t + \gamma_5 \;\; l_{t \;\; 1} \;\; s_t \;\; c_t] \end{aligned} \tag{11}$$

We first note that the second line of this equation replicates the first, except that the second line includes an interaction with the signal being in the form of a retraction. Otherwise, the first line is identical to the equation (6), which was the focus of Section 4.2 where we showed our results are consistent with the literature. Hence the presence of γ_ℓ allows us to detect how these patterns vary, depending on whether or not the signal is a retraction. In other words, the added terms provided a flexible functional form in order to capture the effect of retractions as discussed in Section 2.2, $\log(\alpha(\tau \; j \; s^t))$.

The results can be found in Table 5. A striking pattern emerges: while when updating from new draws we have that subjects (slightly) over-infer from signals ($\beta_2 > 1$) and do more so when signals confirm the prior ($\beta_3 > 0$), when updating from retractions they *under*-infer ($0 < \beta_2 + \gamma_2 < 1$) and exhibit *anti*-confirmation bias ($\beta_3 + \gamma_3 < 0$). In sum, belief updating from retractions exhibits the opposite biases when compared to updating from new draws, a conclusion which is robust across all specifications.

4.4. Why Retractions Fail

We now turn to the question of why retractions are not effective, and especially why they affect beliefs less than equivalent new draws. We present three sets of results. First, we consider the question of whether retractions are less effective once signals have been acted upon, and hence internalized. We test this by comparing the effect of retractions when beliefs have already been elicited versus when they have not, holding constant the history of signals. Second, we analyze the heterogeneous effect of retractions based upon which signal was retracted. Lastly, we discuss

[17] These regressions involve weighting the error terms based on the round they are emerging from. This is because certain rounds—particularly those involving a mix of blue and yellow draws—will mechanically be oversampled by our design. Our weighting adjusts for this in order order to ensure this oversampling does not yield bias in the direction of the most likely paths.

whether the timing of retractions themselves affects their effectiveness by considering whether beliefs are updated differently when retractions occur after the second or the third signals.

4.4.1. Interpreting vs. Reinterpreting Past Information

Assuming that our results on the ineffectivness of retractions is a cognitive effect, a reasonable hypothesis is that this residual effect is stronger if the earlier draw has been acted upon and hence potentially internalized. We test this hypothesis by comparing updating from retractions when beliefs have already been elicited versus when they have not, by comparing beliefs across intermediate versus final elicitation treatments.

The results from this comparison are documented in Table 6. The specifications here correspond to equations 7 and 8 which we have described in Section 4.3.1, but including the intermediate elicitation treatment as an interaction term.[18] The result is a well-identified null result: having acted upon a piece of information has no effect on retractions.

The broad messages from this table are identical to those we have discussed in the previous sections; beliefs move in the directions of signals, but remain diminished when they are retractions relative to new signals. As far as we are able to tell, none of the lessons we have described so far are changed in this treatment. This observation is reassuring, as it suggests that the fact that we are eliciting beliefs does not yield significant changes to the phenomenon

4.4.2. The Timing of Retracted Signals

One feature of our design is that, while the signals subjects receive are exchangeable, they are observed in sequence. If subjects belief updating process displays either primacy or recency effects (which have been documented by Benjamin (2019)), then it is not a priori clear whether the timing of the observed retraction should make a difference.

The aggregated implication of timing of retracted signals is presented in Table 7. This table considers the same specifications described in Section 4.3.1, which described the diminished effectiveness of retractions, adding an indicator variable for whether the last signal observed was retracted (rf_t), as well as an interaction with the direction of the signal itself.

We find that the absolute effectiveness of retractions is slightly increased: it is easier to disregard a piece of information if it arrived more recently, as can be seen from the fact that, in column 1, the estimated coefficient on $r_t \quad s_t \quad rf_t$ being strictly positive and statistically significant. However, subjects still fail to fully disregard the retracted signals, even when these were these are the most recent ones, as the mentioned coefficient is half the size of the coefficient on $r_t \quad s_t$.

[18]Naturally, as when beliefs are elicited only in one period, there is no way to estimate a version of equation 9.

As for the relative effectiveness of retractions, we find that retractions remain equally less effective than informationally-equivalent new signals in inducing changes in beliefs (columns 2 and 3). Thus, our results suggest that, while the timing of the retracted signal may matter for the effectiveness of retractions, it does not play a major role in driving our results.

We also show disaggregated (i.e., history-by-history) versions of these results in Tables 13 through 18. The differences in the belief updates tend to be very small, even when they are significant, leading us to conclude that any differences along these dimensions are reasonably negligible; none of the comparisons are significant at the 5 % level. While there are certainly differences in the average belief reports in each case, we attribute this to the fact that these comparisons have a significantly smaller number of observations. The closest we obtain to determining a difference is when the retraction is observed after two signals, which has roughly 6 times as many observations as the other comparisons. These results suggest the possibility that the retraction may be more effective when it relates to the signal that is more recent, but this difference is quite small and still not significant at the 5% level (though just barely). We conclude that in this design, timing does not have an impact; however, we emphasize that this was in part by design, since signals are observed in close succession to one another. We do not speak to whether these effects may or may not be present when information arrives over a longer timescale, and leave this to future experiments.

Similarly, we also fail to detect any particular meaning to the timing that information is *received*. In Section A.2 in the Online Appendix, we present belief reports for identical histories, which differ only on whether the retraction of a given ball color was the first or the second one observed. We fail to detect any noticeable patterns in these cases. One conclusion this observation is supportive of is that memory has a highly limited role in driving any of our results. This would be expected given our design, since subjects observe all the draws they had seen previously in every round. Still, as our design was not intended to focus on the role of memory (while this may be an interesting avenue for future work), we find it reassuring that the results do not seem to be driven by this variable. In particular, this supports or theoretical formulation, which views signals as exchangeable; as far as we can tell, this assumption seems plausible.

4.4.3. The Timing of Retractions

By having the possibility of observing either a new draw or a retraction in both periods 3 and 4, we can assess whether the timing of a retraction itself has any bearing on its effectiveness. To do so, and similarly to before, we expand our specifications described in Section 4.3.1 with a binary term denoting whether or not the retraction occurred in period 4.

In order to provide a clear identification of the effect of the timing of the retraction itself, we consider only situations where retractions occurred only in either period 3 or in period 4. Furthermore, in order to compare the effectiveness of retractions relative to new draws, we redefine the fixed effects for equation 8 (column 2) on $H_{t-2} \cup \{s_{t-1}\} \cup \{s_t\}$. This implies we are restricting attention to comparing the cases where the compressed history at period 4 is the same but in one the retraction took place in period 3 and in the other in period 4 and both retracted the same signal. That is, we are comparing beliefs after histories $(s_1, s_2, n_\tau = 1, s_4)$ with those following $(s_1, s_2, s_3 = s_4, n_\tau = 1)$, where $\tau \in \{1, 2\}$. We do not estimate equation 9 as we cannot both control for the histories as desired and compare $(s_1, s_2, s_3, n_\tau = 1)$ to the case of obtaining an informationally-equivalent new signal $(s_1, s_2, s_3, s_4 = -s_\tau)$.

As can be seen in Table 8, the timing of retractions has no noticeable nor significant impact on the effectiveness of retractions in leading subjects to disregard particular pieces of information, nor does it impact the relative (in)efficiency of retractions vis-à-vis equivalent new signals.

4.5. Updating After Retractions

Our design also allowed us to see how subjects would respond to belief updates *following* the retraction of information. A natural conjecture is that the differences with retractions is simply due to the added mathematical difficulty due to a more complicated updating process. If this were the case, then one might also expect the subjects to update differently *after* observing a retraction. The hypothesis is that the lack of certainty over the meaning of a retraction would therefore lead to an increase in the relative weight subjects would put on a subsequent signal. Alternatively, observing a retraction may simply undermine confidence in the source of the signal. Our results speaking to this can be found in Table 9. We test whether observing a retraction affects *subsequent* updating in two ways. First, we maximize our use of the experimental variation, plus fixed effects, in Columns 1-4 of Table 9, by estimating two variants of the following equation:

$$b_t = \beta_0 + \beta_1 s_t \times K + \beta_2 r_{t-1} + \beta_3 s_t \times K \times r_{t-1} + \beta_4 s_{t-1} \times K \times r_{t-1} + \beta_5 s_t \times K \times s_{t-1} \times r_{t-1} + F.$$

In columns 1 and 3, the sample is restricted to periods 2 and 4 and fixed effects F correspond to the lagged compressed history, $F_{C(H_{t-1})}$. This enables us to compare changes beliefs after observing, e.g. $\{s_1, s_2\}$ and $\{s_1, s_2, n_2 = 1, s_4 = s_2\}$. In columns 2 and 4, we restrict the sample to period 4 and we have fixed effects F corresponding to the history at period 2 interacted with the sign of the signal in period 3, i.e. $F_{H_{t-2} \times \{s_{t-1}\}}$. As such, we can compare the change in belief reports at histories $\{s_1, s_2, n_2 = 1, s_4\}$ and $\{s_1, s_2, s_3 = -s_2, s_4\}$. Our results—consistent across all specifications—suggest that beliefs are more sensitive to new signals after a retraction.

We conclude our analysis by checking whether experiencing a retraction in the past results in an increased level of effort or attention by the subjects when considering new signals. Similarly to before, we estimate the effect of a retraction on how long subjects take in subsequent updating from new signals by estimating

$$d_t = \beta_0 + \beta_1 \, r_{t-1} + F_{H_{t-2} \cup \{s_{t-1}\} \cup \{s_t\}}, \tag{12}$$

where we control for twice-lagged history and the signs of the signals in the previous and in the current periods. Analogously to Section 4.3.2, we also report on the effect on log decision time. The estimated coefficients—reported in Table 10—suggest that a retraction in the previous period does lead to an increase in decision time, albeit a mild one and statistically significant only when considering log decision time.

5. CONCLUSION

This paper has shown that people do not fully disregard information after being told that it is meaningless. In particular, 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 (i.e., extreme vs. moderate) and kinds of retractions (i.e., confirming vs. contradicting). We demonstrated this in an abstract setting, where this comparison can be made cleanly and precisely, in an incentivized manner.

Toward this end, we formulated a number of hypotheses regarding how subjects would internalize retractions, inspired by the anecdotal evidence that retractions tend to be treated as differently and relatively less effective. We also explicitly considered the notion that there were some additional (non-prohibitive) difficulties associated with updating beliefs from retractions. Table 11 revisits each of these hypotheses, and assesses whether we documented them in our experiment, or failed to find support for them. Overall, the patterns we find are consistent with retractions being more difficult relative to otherwise equivalent information. We emphasize that subjects did still update from the retractions, suggesting that they did convey some informational content. On the other hand, we did not detect any patterns related to how we implemented retractions. This leads us to conclude that particular design choices did not drive the results; on dimensions where relatively difficulty would not seem to vary much, we correspondingly do not detect significant differences in belief updating from retractions.

Our results point to a number of interesting potential directions for future work. As mentioned, our main goal in this paper was to document that retractions had a differential impact, and to determine any significant sources of variation. We therefore see two main directions for follow-on work given these observations.

First, we doubt that we have fully explored the possible heterogeneity in the reactions to retractions. In particular, our design is extremely limited in how strongly it can address memory, or in the impact of the timing of retractions. The uniformity of our results is somewhat striking, but we also suspect that more targeted designs addressed on these questions may yield interesting and useful results. A natural question is whether the way retractions are explained can make them more or less effective. Understanding this is important insofar as it provides suggests for how to more effectively retract information, a question with a high degree of policy relevance.

Second, exploring this phenomenon in particular contexts seems important as well. As mentioned in our review of the literature, a significant body of work on political behavior suggests that many factors are at play which interfere with Bayesian reasoning. We have consciously eliminated many of these. But that said, an interesting question is whether the politicization of certain beliefs makes retractions more or less effective. Or whether the same holds when information is about individual skill, for instance. Our results are certainly suggestive of a broader behavioral phenomenon at play, but leave open the question of how they are operationalized in specific domains. Any such nuances, we believe, could yield insights which help understand the failure or success of retractions in practice.

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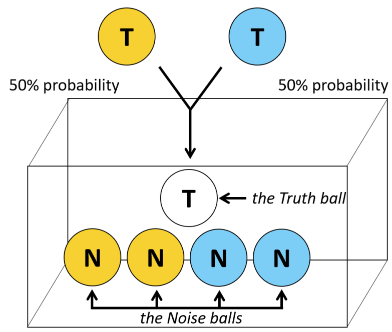
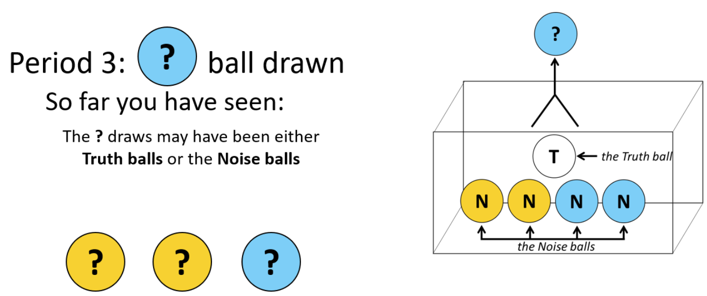
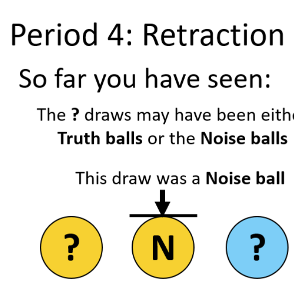
TABLES AND FIGURES



(a) 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. Rounds consisted of (up to) four periods, in each of which there was either a new draw, or a verification, as explained below. At the end of each period, subjects' beliefs were elicited over the color of the truth ball.

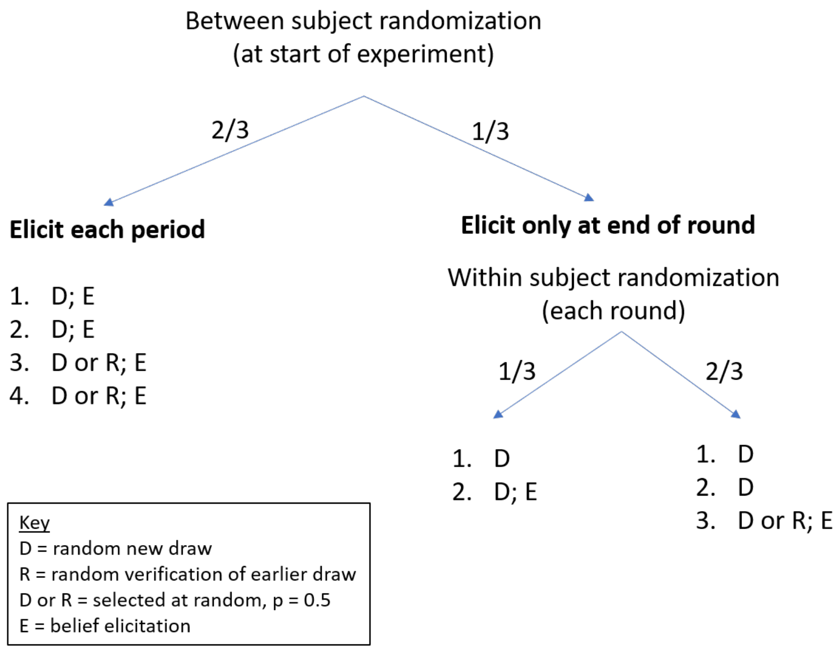


(b) In periods where there was a new draw, a ball was drawn from the box (with replacement), and the color of the drawn ball was disclosed, but whether it was the truth ball or a noise ball was not. The history of the round was displayed throughout.

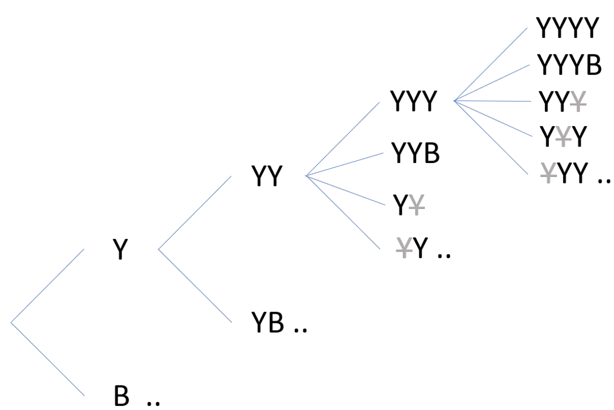


(c) In periods where there was a verification, an earlier draw was chosen at random, and it was disclosed whether that ball was a noise ball (a retraction) or the truth ball. If it was the truth ball the round ended.

Figure 1: Summary of experimental visuals



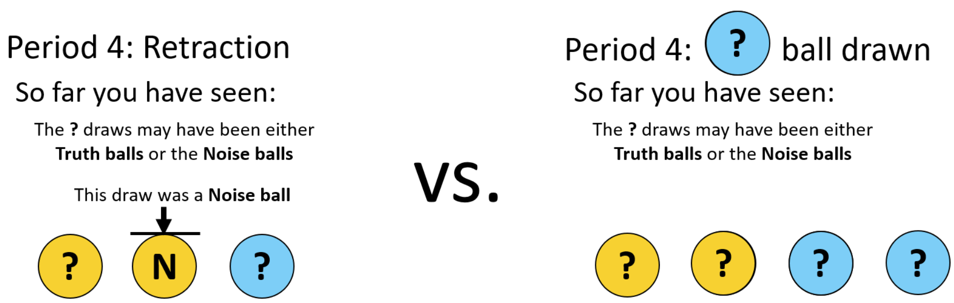
(a) Design of Experiment. At the start of the experiment, subjects are randomly assigned to one of two treatments, "elicit each period" or "elicit at end". Subjects then play 16 rounds of the game. Each round comprises of 3 or 4 periods, in each of which the subject receives a signal - either a draw (D) or a retraction (R) - possibly followed by a belief elicitation (E). In some periods, "D or R", whether the subject observes a new draw or a retraction is randomized, each with probability 0.5. Retractions are verifications, so that one prior draw is revealed to be either the truth or a noise ball (if it is the truth ball the round ends). Subjects assigned to "elicit at end" face a further, round-level, randomization - rounds have either two periods or three, with beliefs only elicited at the end of the round. In this experiment there are four noise balls, two yellow and two blue.



(b) A trimmed event tree of the histories which subjects could see, by period, in "elicit each period" treatment. The 'trimming' is that we only expand the top branch after each period in the figure. Y represents a yellow ball, B represents a blue ball, strike-through represents a retraction on an earlier draw (we exclude verifications which reveal the truth ball, as the round ends at that point.

Figure 2: Experimental design

(a) Do retractions work? We compare beliefs after a retraction, in period t (where t is 3 or 4) to beliefs after the (equivalent) ”compressed history” in period t 2 (although not necessarily in the same round). The compressed history is the history with any retracted balls removed. Thus, in the example illustrated, beliefs elicited after the retraction in period 4 are compared to beliefs in period 2 when there has been a yellow and then a blue draw.



(b) Are retractions treated differently from equivalent new signals? We compare beliefs after a retraction, in period t (where t is 3 or 4) to beliefs after an equivalent new draw (of opposite color to the draw which was retracted), also in period t, but necessarily in a different round. Thus, in the example illustrated, beliefs elicited after the retraction of the yellow ball in period 4 are compared to beliefs elicited in period 4 when the history through period 3 is the same, but then period 4 is a draw of a blue ball.

Figure 3: Illustrative examples to explain the two main identification approaches

	(1)	(2)	(3)
	l_t	l_t	l_t
Prior (l_{t-1})	0.883	0.838	0.825
	(0.0233)	(0.0274)	(0.0298)
Signal (s_t)	1.321	1.042	1.042
	(0.0463)	(0.0611)	(0.0642)
Signal Confirms Prior ($s_t \times c_t$)		0.561	0.583
		(0.110)	(0.111)
Signal x Prior ($s_t \times l_{t-1}$)			-0.229
			(0.0908)
Prior x Signal Confirms Prior ($s_t \times l_{t-1} \times c_t$)			0.274
			(0.148)
Observations	11739	11739	11739
R-Squared	0.425	0.427	0.429

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 1: Updating from new draws. This table represents updating from standard new ball draws. The sample is those subjects assigned to the intermediate elicitation treatment (i.e. beliefs are elicited each period). We include beliefs of all periods (1-4) but, within a given round, we exclude any beliefs which are elicited after a verification. Thus, for example, if there is a retraction in period 3, we exclude beliefs in both period 3 and 4. The regressions correspond to Equations 4, 5, and 6. Inverse probability weights are used to make each history equally likely. 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 by K, a constant factor of Bayesian updating, such that the coefficient on s_t would be 1 under Bayesian updating), $c_t := \mathbb{1}\{\text{sign}(l_{t-1}) = \text{sign}(s_t)\}$ is an indicator function that equals 1 when the signal at t confirms the prior at $t-1$.

	(1)	(2)	(3)	(4)	(5)
	l$_t$	l$_t$	l$_t$	l$_t$	l$_t$
Prior (l_{t-1})	0.883		0.716	0.883	0.938
	(0.0233)		(0.0257)	(0.0371)	(0.0353)
Signal (s_t)	1.321	0.621	0.709	1.417	1.566
	(0.0463)	(0.0331)	(0.0259)	(0.0628)	(0.117)
Observations	11739	6752	6752	3317	1670
R-Squared	0.425	0.049	0.498	0.385	0.507

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 2: Updating from new draws – by period. This table represents updating from standard new ball draws across different periods. The sample is those subjects assigned to the intermediate elicitation treatment (i.e. beliefs are elicited each period). We include beliefs of all periods (1-4) but, within a given round, we exclude any beliefs which are elicited after a verification. Thus, for example, if there is a retraction in period 3, we exclude beliefs in both period 3 and 4. Column 1 uses data of all periods. Columns 2 through 5 use a sample restricted to periods 1 through 4, respectively. The regressions correspond to Equations 4, 5, and 6. Inverse probability weights are used to make each history equally likely. 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 by K, a constant factor of Bayesian updating, such that the coefficient on s_t would be 1 under Bayesian updating), $c_t := 1\{\text{sign}(l_{t-1}) = \text{sign}(s_t)\}$ is an indicator function that equals 1 when the signal at t confirms the prior at $t-1$. .

	(1)	(2)	(3)
	b_t	b_t	Δ b_t
Retraction (r_t)	-0.000492	-0.00367	-0.00471
	(0.00341)	(0.00430)	(0.00663)
Retracted signal ($r_t \quad s_t$)	-0.0220	-0.0322	-0.0304
	(0.00305)	(0.00430)	(0.00663)
Compressed history FEs	Yes	No	No
Lagged history x Sign of Signal FEs	No	Yes	Yes
Observations	17591	9074	2993
R-Squared	0.154	0.255	0.115

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 3: Updating from retractions: do they work and how do they compare to equivalent new signals. The sample is those subjects assigned to the intermediate elicitation treatment (i.e. beliefs are elicited each period). Column (1) tests whether retractions work, by comparing beliefs after a retraction to beliefs after the equivalent compressed history. We include beliefs of all periods (1-4) but, within a given round, we exclude any beliefs which are elicited after a verification and we exclude period 4 beliefs if there was a retraction in period 3. In periods 3 and 4 we only include beliefs when there was a retraction in that period. The outcome is the beliefs in period t, b_t. $r_t \quad s_t$ is the retracted signal in round t (+1 or -1). The regression includes fixed effects for the compressed history of draws. Columns (2) and (3) test whether people update more or less from retractions compared to equivalent new signals. The sample is restricted to beliefs in periods 3 and 4, once again dropping beliefs after validations or in period 4 if there is a retraction in period 3. The specifications include fixed effects for the history of the previous period interacted with the signal. In Column 2, the outcome is the beliefs in period t, b_t. In Column 3, the outcome is the first difference in beliefs.

	(1)	(2)
	d_t	$\log(d_t)$
Retraction (r_t)	0.493	0.101
	(0.0908)	(0.0142)
Lagged history * Sign of Signal FEs	Yes	Yes
Mean of dep. variable	5.57	1.57
Observations	8986	9074
R-Squared	0.010	0.014

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 4: Decision times. This table tests whether the time taken to report beliefs is different after retractions compared to equivalent new signals. The sample is those subjects assigned to the intermediate elicitation treatment (i.e. beliefs are elicited each period). The specifications compare updating from retractions versus from an equivalent new signal in periods 3 and 4 (we drop period 4 if there was a retraction in period 3).

	(1)	(2)	(3)
	l_t	l_t	l_t
Prior ($l_{t\ 1}$)	0.883	0.840	0.820
	(0.0371)	(0.0438)	(0.0497)
Signal (s_t)	1.417	1.185	1.163
	(0.0628)	(0.0834)	(0.0937)
Retraction (r_t)	-0.0274	-0.0135	-0.000210
	(0.0322)	(0.0326)	(0.0423)
Retraction x Prior ($r_t\ \ l_{t\ 1}$)	-0.0793	-0.00832	-0.0188
	(0.0534)	(0.0637)	(0.0681)
Retraction x Signal ($r_t\ \ s_t$)	-1.027	-0.656	-0.693
	(0.0798)	(0.114)	(0.122)
Signal Confirms Prior ($s_t\ \ c_t$)		0.475	0.534
		(0.156)	(0.165)
Retraction x Signal Confirms Prior ($r_t\ \ s_t\ \ c_t$)		-0.809	-0.817
		(0.210)	(0.210)
Signal x Prior ($s_t\ \ l_{t\ 1}$)			-0.234
			(0.159)
Prior x Signal Confirms Prior ($s_t\ \ l_{t\ 1}\ \ c_t$)			0.316
			(0.248)
Retraction x Signal x Prior ($r_t\ \ s_t\ \ l_{t\ 1}$)			0.209
			(0.205)
Retraction x Prior x Signal Confirms Prior ($r_t\ \ s_t\ \ l_{t\ 1}\ \ c_t$)			0.0423
			(0.320)
Observations	6081	6081	6081
R-Squared	0.413	0.414	0.416

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 5: How do retractions interact with other biases in updating? The sample is those subjects assigned to the intermediate elicitation treatment. The regressions correspond to Equation 11, using data from periods 3. The sample excludes the cases where the state of the world is fully disclosed. Inverse probability weights are used to make each history equally likely. 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 by K, a constant factor of Bayesian updating, such that the coefficient on s_t would be 1 under Bayesian updating), r_t is an indicator variable for whether the signal in period t can from a retraction, $c_t := 1\{\text{sign}(l_{t\ 1}) = \text{sign}(s_t)\}$ is an indicator function that equals 1 when the signal at t confirms the prior at $t\ \ 1$.

	(1)	(2)
	b_t	b_t
Final (Fin_t)	0.00991	0.0117
	(0.0106)	(0.0114)
Retraction (r_t)	-0.00424	-0.00704
	(0.00343)	(0.00458)
Retracted signal (r_t s_t)	-0.0161	-0.0308
	(0.00717)	(0.00903)
Final x Retracted Signal (Fin_t r_t s_t)	0.00447	0.00399
	(0.0109)	(0.0170)
Final x Retraction (Fin_t r_t)		-0.00134
		(0.00806)
Final x Signal (Fin_t s_t)		0.000464
		(0.0101)
Compressed history FEs	Yes	No
Lagged history x Sign of Signal FEs	No	Yes
Observations	11213	9920
R-Squared	0.097	0.209

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 6: Intermediate versus final belief elicitation. This table tests whether updating from retractions is different whether or not beliefs have previously been elicited before a signal is retracted. The sample is all subjects. Column 1 restricts to period 1 and to period 3 when there is a retraction, interacting the specification from Column 1 in Table 3 with a dummy for being in the final period only elicitation group. Column 2 restricts to period 3 and 1, 2, and 3 restrict to period 3, when there was a retraction, and regresses beliefs on the period 3 signal interacted with whether the subject was in the intermediate elicitation group and period 3 retracted signal interacted with whether the subject was in the intermediate elicitation group. Column 1 includes fixed effects for the history at period 3. Column 2 includes fixed effects for the history at period 2 interacted with the sign of the signal.

	(1)	(2)	(3)
	b_t	b_t	Δ b_t
Retraction (r_t)	0.000777	-0.00321	-0.00843
	(0.00434)	(0.00537)	(0.00755)
Retracted signal (r_t s_t)	-0.0289	-0.0381	-0.0320
	(0.00408)	(0.00537)	(0.00755)
Last Draw Retracted (rf_t)	-0.00289	-0.00112	0.0108
	(0.00615)	(0.00714)	(0.0111)
Retracted signal x Last Draw Retracted (r_t s_t rf_t)	0.0156	0.0135	0.00444
	(0.00613)	(0.00714)	(0.0111)
Compressed history FEs	Yes	No	No
Lagged history x Sign of Signal FEs	No	Yes	Yes
Observations	17591	9074	2993
R-Squared	0.154	0.255	0.115

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 7: Timing of the signal which is retracted. The idea is to test whether there is a difference in responding to retractions depending on whether the last signal was retracted or an earlier signal was retracted. The sample is those subjects assigned to the intermediate elicitation treatment in experiment (i.e. beliefs are elicited each period). In Column 1, we include beliefs of all periods (1-4) but, within a given round, we exclude any beliefs which are elicited after a verification. We also exclude any beliefs if there was no retraction in period 3 or 4. In Column 2 and 3, we further restrict the beliefs to periods 3 and 4. In Column 2, the outcome is the beliefs in period t, b_t. In Column 3, the outcome is the first difference in beliefs.

	(1)	(2)
	b_t	b_t
Retraction (r_t)	0.00773	0.00737
	(0.00630)	(0.00850)
Retraction in Period 4 (r_4)	-0.00104	-0.00194
	(0.00836)	(0.00838)
Retracted signal ($r_t \quad s_t$)	-0.0368	-0.0417
	(0.00596)	(0.00742)
Retracted signal x Retraction in Period 4 ($r_t \quad s_t \quad r_4$)	0.00218	0.0154
	(0.00836)	(0.00982)
Compressed history FEs	Yes	No
L2 History x L Signal x Signal FEs	No	Yes
Observations	9432	4350
R-Squared	0.203	0.274

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 8: Timing of retraction: does the timing at which a retraction is received matter for its effect? The idea is to test whether retractions have a different effect if they come in period 3 or period 4, based on beliefs in period 4 and holding the history fixed up to order. The sample is those subjects assigned to the intermediate elicitation treatment in experiment (i.e. beliefs are elicited each period). In Column (1) period 4 is compared to period 2. Beliefs in period 4 are included if there was a retraction in period 3 or 4, but not both. In Column (2) only beliefs in period 4 are considered, and they are dropped if there is retractions in both periods 3 and 4. The comparison is a retraction in period 4 versus an equivalent new signal in period 4, compared to a retraction in period 3 versus an equivalent new signal in period 3.

	(1)	(2)	(3)	(4)
	Δ b$_t$	Δ b$_t$	Δ b$_t$	Δ b$_t$
Signal (s_t)	0.0574	0.0579	0.0574	0.0578
	(0.00172)	(0.00343)	(0.00172)	(0.00343)
Retraction in Previous Period (r_{t-1})	0.00826	0.00950	0.00816	0.00989
	(0.00395)	(0.00546)	(0.00395)	(0.00546)
Signal x Retraction in Previous Period ($(s_t) \times (r_{t-1})$)	0.00900	0.0154	0.00901	0.0157
	(0.00389)	(0.00513)	(0.00389)	(0.00513)
Retracted Signal in Previous Period ($r_{t-1} \times s_{t-1}$)			0.00127	0.0131
			(0.00339)	(0.00546)
Signal x Retracted Signal in Previous Period ($(s_t) \times (r_{t-1} \times s_{t-1})$)			0.00386	0.00249
			(0.00339)	(0.00391)
Lagged Compressed History FEs	Yes	No	Yes	No
Lagged Lagged History x Lagged Sign of Signal FEs	No	Yes	No	Yes
Observations	12209	5457	12209	5457
R-Squared	0.112	0.103	0.112	0.104

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 9: Signals after retractions. This tests updating from new signals after retractions, compared to after the equivalent compressed history, and also compared to after the equivalent new signal. The sample is those subjects assigned to the intermediate elicitation treatment (i.e. beliefs are elicited each period). In Columns 1 and 3, we restrict the sample to period 2 and 4. In Columns 2 and 4, we restrict the sample to period 4.

	(1)	(2)
	d_t	$\log(d_t)$
Lagged Retraction (r_{t-1})	0.224	0.0515
	(0.130)	(0.0192)
Lagged lagged history x Lagged Sign of Signal x Sign of Signal FEs	Yes	Yes
Mean of dep. variable	5.93	1.63
Observations	5405	5457
R-Squared	0.010	0.013

Standard errors in parentheses
$p < 0.1$, $p < 0.05$, $p < 0.01$

Table 10: Decision times. This table tests whether the time taken to report beliefs is different after retractions compared to equivalent new signals. The sample is those subjects assigned to the intermediate elicitation treatment (i.e. beliefs are elicited each period). The specifications consider updating from new signals in period 4, and compare the decision time based on whether or not there was a retraction in period 3. That is, we compare updating from a given new signal, based upon whether in the previous period there was a new signal or an equivalent retraction.

Hypothesis	Documented (✓) or not detected (x)	Other Comments
Hypothesis 1, Part (a): Subjects fail to fully internalize retractions	✓	
Hypothesis 1, Part (b): Subjects treat retractions as less informative than equivalent new information	✓	
Hypothesis 2: Updating from retractions accentuates biases in updating already present in updating	✓	Additional under-inference and anti-confirmation bias
Hypothesis 3: Updating from retractions takes longer	✓	
Hypothesis 4: Retractions have less of an effect when subjects have acted upon observed signals	x	Precise null
Hypothesis 5: Later retractions have a different impact on beliefs compared to earlier retractions	x	Imprecise null; possible that retractions are slightly more effective for recent signals
Hypothesis 6: Retractions influence beliefs differently depending on when the retracted signal was observed.	x	
Hypothesis 7: Subjects update differently after observing retractions	✓	More sensitivity to signals after retractions

Table 11: Summary of our assessment of our 7 main hypotheses. See Section 2.3 for a more complete description of each, as well as the reasoning involved with formulating each one.

Online Appendix for Learning versus Unlearning

A. TABLES AND FIGURES

A.1. Basic Data on Beliefs

Figure 4: Distribution of reported beliefs

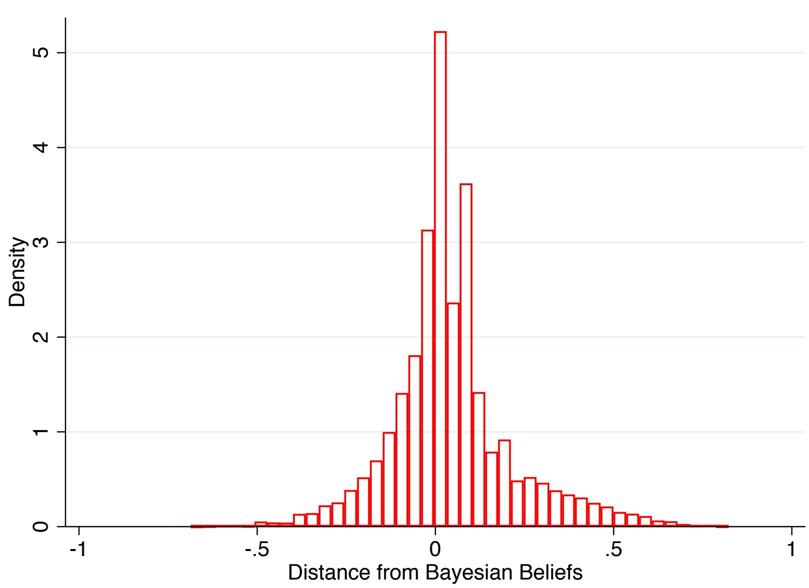
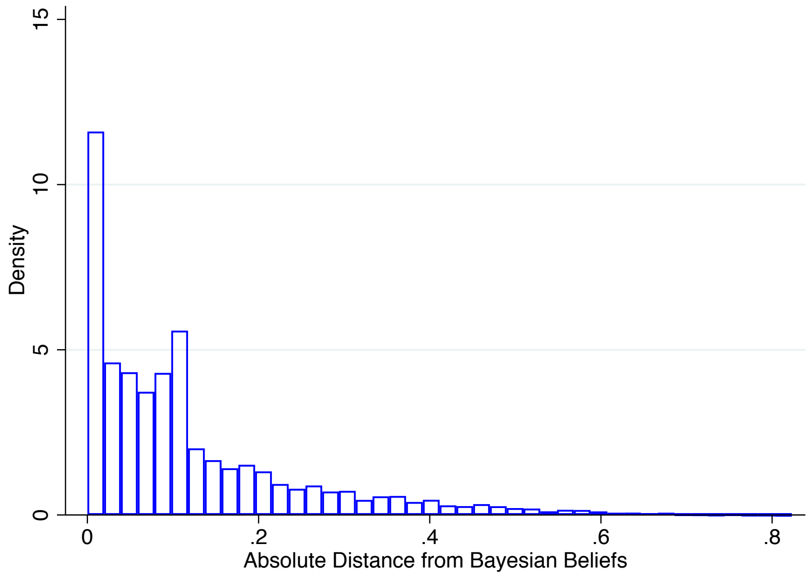


Figure 5: Distribution of reported beliefs



	(1)	(2)
Bayesian Beliefs	0.614	0.745
	(0.0163)	(0.0163)
Constant	0.187	0.129
	(0.00956)	(0.00968)
N	17896	15521

Standard errors in parentheses

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

Table 12: Sanity check. Regressing stated beliefs against their Bayesian benchmark. This compares belief reports to Bayesian beliefs. In the second specification, we exclude participants who updated in the wrong direction.

A.2. Comparisons on Time of the Retracted Signal

VARIABLES	(1) Beliefs
Case 1: 11, Retracting signal from period 1	0.00336
	(0.00788)
Case 2: 11, Retracting signal from period 2	-0.0162**
	(0.00775)
Constant	0.576***
	(0.00321)
Observations	4,495
R-squared	0.001
Retracting signal from period 1 = Retracting signal from period 2	0.0522

Table 13: Case 1.1: 11. Comparing belief updating with retraction of the first signal vs. the second signal.

VARIABLES	(1) Beliefs
Case 1: 111, Retracting signal from period 1	0.00555
	(0.0288)
Case 2: 111, Retracting signal from period 2	0.0482*
	(0.0259)
Constant	0.585***
	(0.00901)
Observations	777
R-squared	0.005
Retracting signal from period 1 = Retracting signal from period 2	0.244

Table 14: Case 1.2: 111. Comparing belief updating with retraction of the first signal vs. the second signal.

VARIABLES	(1) Beliefs
Case 1: 110, Retracting signal from period 1	-0.0600***
	(0.0121)
Case 2: 110, Retracting signal from period 2	-0.0679***
	(0.0123)
Constant	0.588***
	(0.00768)
Observations	1,339
R-squared	0.027
Retracting signal from period 1 = Retracting signal from period 2	0.556

Table 15: Case 1.3: 110. Comparing belief updating with retraction of the first signal vs. the second signal.

VARIABLES	(1) Beliefs
Case 2: 111, Retracting signal from period 1	0.0163
	(0.0288)
Case 3: 111, Retracting signal from period 3	0.0438*
	(0.0259)
Constant	0.574***
	(0.00895)
Observations	824
R-squared	0.004
Retracting signal from period 1 = Retracting signal from period 3	0.453

Table 16: Case 2.1: 111. Comparing belief updating with retraction of the first signal vs. the third signal.

VARIABLES	(1) Beliefs
Case 1: 101, Retracting signal from period 1	-0.00405
	(0.0102)
Case 3: 101, Retracting signal from period 3	-0.00362
	(0.0158)
Constant	0.533***
	(0.00681)
Observations	945
R-squared	0.000
Retracting signal from period 1 = Retracting signal from period 3	0.979

Table 17: Case 2.2: 101. Comparing belief updating with retraction of the first signal vs. the third signal.

VARIABLES	(1) Beliefs
Case 2: 100, Retracting signal from period 2	0.0113
	(0.0116)
Case 3: 100, Retracting signal from period 3	0.00112
	(0.0215)
Constant	0.467***
	(0.00879)
Observations	933
R-squared	0.001
Retracting signal from period 2 = Retracting signal from period 3	0.627

Table 18: Case 3.1: 100. Comparing belief updating with retraction of the second signal vs. the third signal.

B. INSTRUCTIONS

B.1. Instructions (June 2020), not including preamble

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 **{{total_rounds}} rounds**, each round with 4 **periods**, plus a practice round 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 ${{avg_payment}}**, of which ${{min_payment}} depends only on completing the task.

Truth Balls and Noise Balls

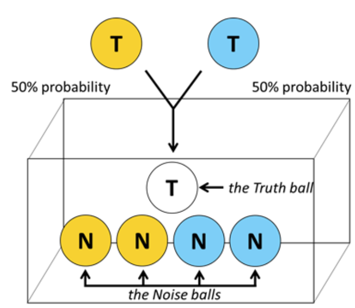
At the **beginning of each round**, **5 balls** are put inside a box.
The balls in that box are of two kinds:
- **4 Noise balls** [N], of which **2 are yellow [NY] and 2 are blue [NB]**; and
- **1 Truth ball** [T], which can be either **yellow [TY] or blue [TB]**.

Your task is to estimate the probability that the Truth ball [T] is yellow [TY] or blue [TB], 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 [TY] or [TB] with **equal probability**.
The Truth ball [T] is then put inside the box with all 4 Noise balls, 2 [NY] and 2 [NB].
All balls remain inside the box throughout the round.

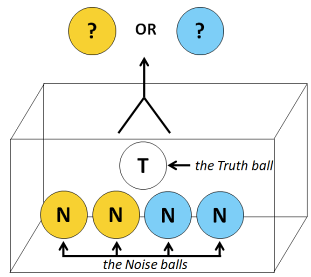


The round lasts for 4 periods, each of which may help you to guess the color of the Truth ball [T].

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**.

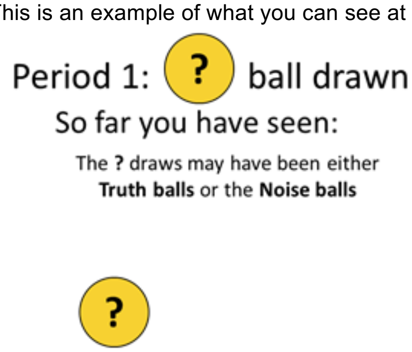
Periods 1 and 2

In periods 1 and 2, **a ball is drawn** from the box **at random** and you are told its **color**, [Y] or [B].
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:



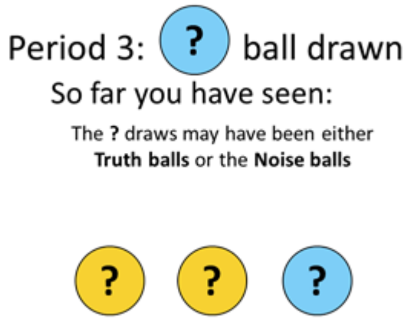
Periods 3 and 4

At the beginning of periods 3 and 4, 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 **retraction**, 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:



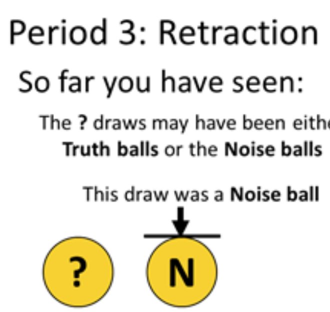
(ii) Retraction

If you get a retraction,

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 retraction in period 3:



New Round

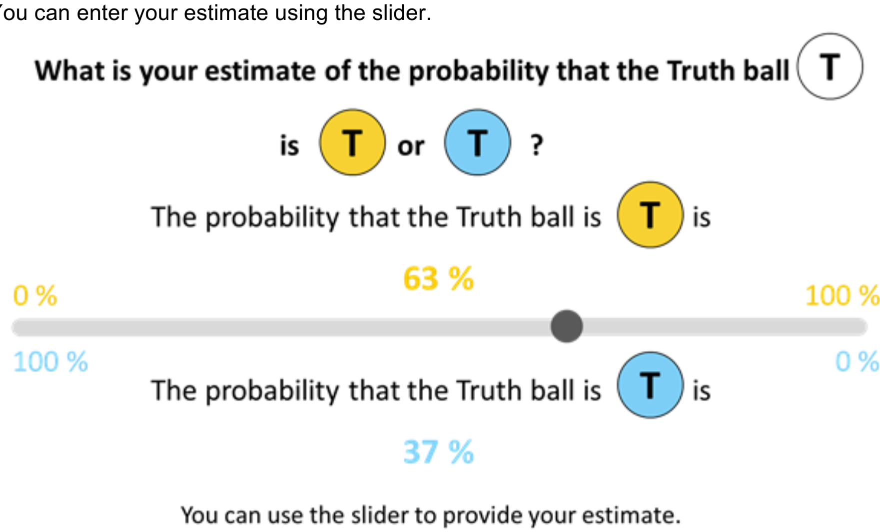
After these 4 periods, a new round begins.
Each round, a new color for the Truth ball [T] is selected the same way and independently.
This means that **whether the Truth ball is [TY] or [TB] in one round has no influence on whether the Truth ball is [TY] or [TB] in another round**.
It will be clearly 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 [TY] or blue [TB].

Unless it is shown to you in a retraction, 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 ${{min_payment}} for sure.

You can get a bonus of an additional ${{bonus_payment}} 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 [TY] with probability q (x 100%) and an [TB] with probability 1-q (x 100%), then you will receive
100 x (1 - (1-q)^2) points if the Truth ball is [TY], and
100 x (1 - q^2) points if the Truth ball is [TB].

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

For instance, if you estimate that the Truth ball is [TY] with 89% probability and [TB] with 11% probability, you receive 98.79 points if the Truth ball is indeed [TY] and 20.79 if the Truth ball is instead [TB].

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 bonus**.
This means that if in the selected round/period you have 99.84 points you have 99.84% probability of getting the $6 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 [TY] or [TB].

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 ${{quant_bonus}} 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

Q1:
How many Noise Balls are there?
0, 1, 2, 3, 4

Q2:
How many of the Noise Balls are [NY] and [NB]?
1 [NY] and 3 [NB]
3 [NY] and 1 [NB]
2 [NY] and 2 [NB]

Q3:
It is possible that you see a [?Y] ball 4 times and the Truth ball is [TB].
The statement is true.
The statement is false.

Q4:
Even if in a given round the Truth Ball is [TY], in the following round the Truth ball can either be [TY] or [TB] with equal (50 % -- 50 %) probability.
The statement is true.
The statement is false.

Q5:
If a draw you were shown [?Y] corresponded to a Noise ball [NY], then it means the Truth ball has to be [TB] and not [TY].
The statement is true.
The statement is false.

Q6:

If a draw you were shown [?Y] corresponded to a Noise ball [NY], then it means the Truth ball [T] may or may not be of a different color.
The statement is true.
The statement is false.

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 **{{total_rounds}} rounds**, each round with either 2 or 3 **periods**, plus a practice round 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 ${{avg_payment}}**, of which ${{min_payment}} depends only on completing the task.

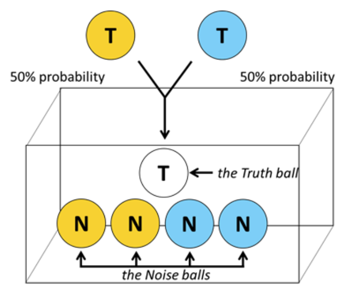
Truth Balls and Noise Balls

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 [NY] and 2 are blue [NB]**; and
- **1 Truth ball** [T], which can be either **yellow [TY] or blue [TB]**.

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

Your task

A Round

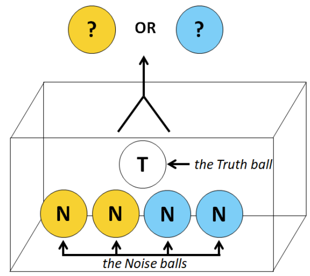


At the **beginning of each round**, the **Truth ball** [T] is chosen to be either [TY] or [TB] with **equal probability**.
The Truth ball [T] is then put inside the box with all 4 Noise balls, 2 [NY] and 2 [NB].
All balls remain inside the box throughout the round.

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**.

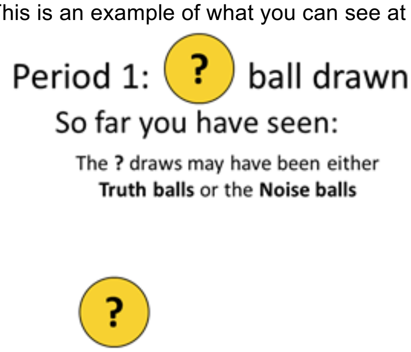
Periods 1 and 2



In periods 1 and 2, **a ball is drawn** from the box **at random** and you are told its **color**, [Y] or [B].
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:



At the end of period 2, with 33% probability the round ends, and with 66% probability you will move to period 3.

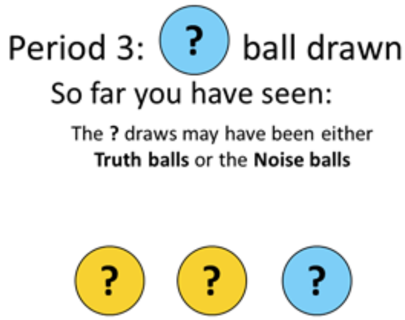
Period 3

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 **retraction**, 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:



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If you get a retraction,

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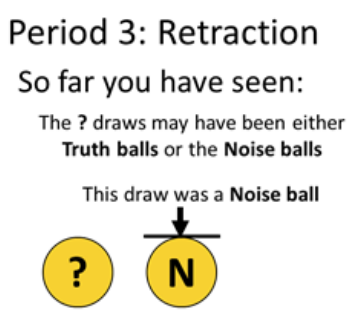

New Round

The round can end after period 3, with 2/3 probability (66.7%), or after period 2, with 1/3 probability (33.3%), in which case you will skip period 3.

After the round ends, 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 [TY] or [TB] in one round has no influence on whether the Truth ball is [TY] or [TB] in another round**.
It will be clearly indicated when a new round begins.

Estimates

At the end of every round you will be asked to provide your estimate of the probability that the Truth ball [T] is yellow [TY] or blue [TB].

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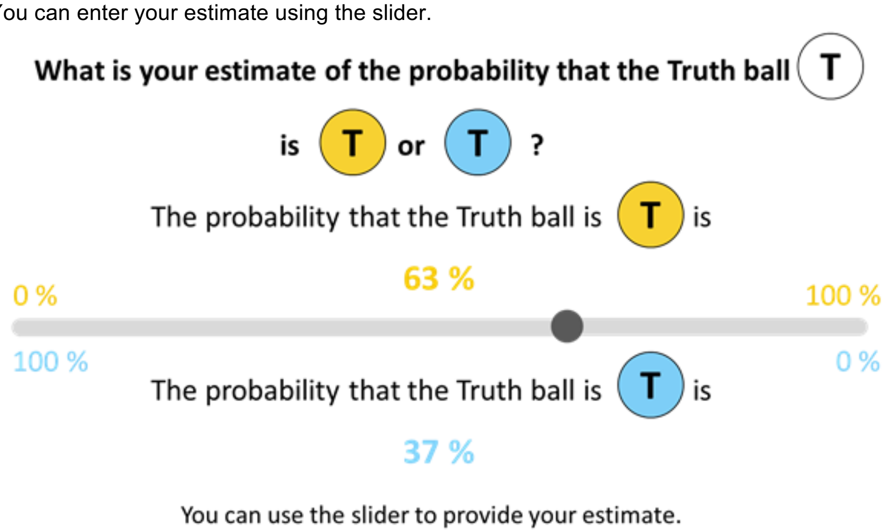

Payment

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100 x (1 - (1-q)^2) points if the Truth ball is [TY], and
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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.

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How many Noise Balls are there?
0, 1, 2, 3, 4

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How many of the Noise Balls are [NY] and [NB]?
1 [NY] and 3 [NB]
3 [NY] and 1 [NB]
2 [NY] and 2 [NB]

Q3:
It is possible that you see a [?Y] ball 3 times and the Truth ball is [TB].
The statement is true.
The statement is false.

Q4:
Even if in a given round the Truth Ball is [TY], in the following round the Truth ball can either be [TY] or [TB] with equal (50 % -- 50 %) probability.
The statement is true.
The statement is false.

Q5:
If a draw you were shown [?Y] corresponded to a Noise ball [NY], then it means the Truth ball has to be [TB] and not [TY].
The statement is true.
The statement is false.

Q6:
If a draw you were shown [?Y] corresponded to a Noise ball [NY], then it means the Truth ball [T] may or may not be of a different color.
The statement is true.
The statement is false.

Instructions: Quantitative Questions

Below you can see 3 different questions.
For each question, choose the option that you think is correct.
There is only one correct answer for each question.

One of these 3 questions will be chosen randomly with equal probability.
If your answer to the chosen question is correct, you will get an addition ${{quant_bonus}}.
If your answer is not correct, you get no additional money.

Q1

Q2

Q3