

Belief-Based Utility and Signal Interpretation^{*}

Marta Kozakiewicz[†]
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Abstract

Do people perceive favorable feedback in a different way than unfavorable one? After a decade of research, there is no definite answer. The existing literature disagrees not only on the magnitude but also the direction of the bias (Benjamin, 2019). In this paper, I propose a new experimental paradigm to identify motivated distortion of beliefs about signal informativeness. The new design allows me to better capture the asymmetry in response to “good” and “bad” news. The data reveals that participants perceive favorable signals as much more likely to be informative. Furthermore, I introduce a new control condition to uncover the underlying mechanism. Participants in the control group evaluated informativeness of a signal ex-ante, conditioned on possible signal realizations. By comparing beliefs reported after a signal to the reports stated ex-ante, I show that participants distort their perception in a motivated way *after* receiving a signal. The results cast a new light on the origins of overconfidence, pointing towards the role of affect (or utility from beliefs shifted by the signal) in asymmetric updating.

Keywords: overconfidence, belief formation, learning, experiment

JEL classification: C91, D83

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[†]Bonn Graduate School of Economics; email: martkozakiewicz@gmail.com

1 Introduction

People tend to overestimate their abilities and chances of success, making costly mistakes as they hold on to their biased beliefs at the expense of accuracy. This tendency, commonly referred to as *overconfidence*, generates significant costs for both the individual and society¹. A long-standing question in behavioral economics is how it can persist in environments with frequent feedback. In this paper, I explore one possible explanation.² I consider an agent who does not know his ability and receives a signal that either reveals it or not. The agent forms beliefs about both his ability *and* informativeness of the signal. Importantly, he values his beliefs about his ability, so that any change in these beliefs directly affects his utility function (Brunnermeier and Parker, 2005; Caplin and Leahy, 2019; Kőszegi, 2006). I attempt to answer the following questions: Does the agent perceive a favorable signal to be more informative than an unfavorable one? Would he perceive the signal differently if the signal did not affect his utility function?

To this end, I designed a simple experiment in which participants learn about their performance in an IQ test.³ In a treatment condition, participants received a signal about their performance and reported their beliefs about the signal’s informativeness. I incorporate several changes to the classical design that allow me to better capture asymmetry in response to “good” and “bad” news. Moreover, I introduce a new control condition, in which participants decide about hypothetical signal realizations. They faced *the same* decision as subjects in the treatment condition but *without* receiving an actual signal. The difference between reports in the treatment and the control condition reveals the extent of belief manipulation in response to favorable and unfavorable signals, and pins down a causal effect of signal valence on updating. Moreover, it informs us about the underlying mechanism by showing how a change in beliefs (triggered by a signal) and the ensuing belief-based utility affect signal interpretation.

¹Negative consequences of overconfidence include excessive selection into competitive environments (Camerer and Lovo, 1999; Niederle and Vesterlund, 2007), excessive trading (Barber and Odean, 2001), suboptimal investment decisions (Malmendier and Tate, 2005, 2008), and political polarization (Ortoleva and Snowberg, 2015).

²Other explanations that are similar to my work (as they consider motivated reasoning rather than cognitive processes) can be divided into three categories: information avoidance (see Golman et al., 2017, for a comprehensive literature review), selective recall (Chew et al., 2019; Huffman et al., 2019; Zimmermann, 2020), and asymmetric updating. The last point mentioned comes the closest to my work and I review it in detail in the following section.

³The experiment was pre-registered in the AEA RCT Registry (Registration Number AEARCTR-0006233). Details of the registration are provided in Appendix H.

The data from the treatment condition shows that subjects perceive favorable signals as more likely to be informative. The average difference in the reported probability after a “good” versus a “bad” signal amounts to 13 percentage points and is significant at the 1% level. The result holds after controlling for potential selection. Moreover, the comparison between the treatment and the control condition indicates that the perception of a signal is significantly altered after receiving it. In the treatment condition, participants reported a 10.6 percentage points higher (a 27.9% increase) probability of a favorable signal being entirely informative about their performance. There is no significant difference after unfavorable signals. The inference about the signal has a lasting effect on subjects’ beliefs about their ability. We observe additional asymmetry in how participants translate their beliefs about the signal into beliefs about ability. As a result, although signals significantly shifted subjects’ beliefs, they did it selectively, and the aggregate overconfidence level remained virtually unchanged.

My study provides the first clear evidence of a causal effect of belief-based utility on signal interpretation. While the research on updating beliefs about ego-relevant traits has a long tradition, establishing causality has always been challenging. One difficulty lies in introducing exogenous variation in “ego-relevance”: the way signals affect belief-based utility. Ideally, we would like subjects to receive the same feedback, but the feedback would have no *valence* – it would not be “positive” or “negative” in the sense that it would not bring participants additional belief-based utility. But how to separate feedback from its valence? Previous work focused on comparing how people update their beliefs about some ego-relevant characteristic (e.g. one’s performance in an IQ test) and how they update beliefs about some ego-neutral parameter (e.g. performance of a robot).⁴ However, this comparison involves not only learning about ego-relevant and ego-neutral parameters, but also updating subjective beliefs, possibly multiple priors, and updating objective probabilities given by the experimenter. The experimental manipulation affects more than one aspect of the study undermining causal inference.

⁴See, for instance, Coutts (2019), Eil and Rao (2011), and Möbius et al. (2014). One exception is a study by Buser et al. (2018), which compares how participants update beliefs about their performance in various tasks that differ in how relevant they are to the subject’s self-esteem. However, in their set-up, it is not possible to introduce exogenous variation in ego-relevance. Grossman and Owens (2012) propose a control condition in which participants learn about the test result of another subject. In this case, subjects update their subjective beliefs about an unknown, ego-neutral variable.

In this paper, I propose a novel experiment in which both the treatment and the control condition are based on the same subjective beliefs over the same ego-relevant characteristic. However, I introduce exogenous variation in how signals affect subjects' beliefs and their belief-based utility: in the control condition, a signal is not realized, hence it does not affect subjects' beliefs nor their belief-based utility. Thereby, I separate feedback from its valence without changing other decision-relevant aspects of the design.

The study was conducted in August 2020 in the BonnEconLab at the University of Bonn. In total, I collected data from 222 participants. The experiment consisted of several parts. Firstly, participants were given an IQ test and incentivized to do their best. After the test, they were asked to report their beliefs about their relative performance. Using an incentive compatible mechanism, I elicited subjective beliefs about one's test score falling into the 1st, 2nd, ..., 10th decile of the score distribution. I referred to the deciles as "ranks", with 1 denoting the highest and 10 denoting the lowest rank.

After the belief elicitation, we described the framework to the subjects as follows: "There are two boxes. Box 1 contains 10 balls with numbers 1 to 10 written on them (each number occurs exactly once). Box 2 contains 10 balls with the same number written on every one of them. That number is equal to your rank." For example, if a subject's rank is 4, Box 2 contains 10 balls with the number "4" written on them.

In the main task, one ball was randomly drawn from one of the boxes (either box could be selected with equal probability) and presented to the subject. After seeing the ball, the participant reported his beliefs about the event that the ball came from Box 2 (with his rank). The report was made by dividing 100 points between the two boxes. I incentivized truthful reporting with the Binarized Scoring Rule (Hossain and Okui, 2013). The method was explained to the participants and they were informed that their chances to win the highest reward were maximized when they divided their points in a way that corresponded to their true beliefs about the box. We explained in intuitive terms how one can arrive at a Bayesian update given one's prior beliefs about the rank.

The design described above differs from experiments on belief updating in several ways. Firstly, I shift the focus from beliefs updating to subjects' inferences about the signal. I argue that updating takes two steps: assessing the information bore into a

signal and incorporating it into prior beliefs.⁵ I aimed at disentangling the effect of signal valence on the first step from the way agents are aggregating information.⁶ For this reason, I restricted the number of signals that participants receive to one.

Secondly, I use a richer state and signal space compared to previous studies. To understand why it is important, imagine a participant who believes that he is in the 80th percentile of the IQ test score distribution. Receiving a coarser signal, e.g. a signal indicating that his score was above the median, would not influence his beliefs as it merely confirms what he already knows. However, if the signal was more precise, e.g. it revealed that his score was only in the 60th percentile, it would affect his beliefs and, according to my hypothesis, induce a stronger reaction.

Last but not least, I define signal valence with respect to subjects’ expectations. Being among 40% best performers is hardly good news if you expect to be among the top 10%. I incorporate this idea by defining a “good” signal to be the one above or equal to the median of individual belief distribution, which I elicited before the main task.⁷

An ideal counterfactual to the treatment condition would include a subject who has the same prior belief distribution (or the same set of prior belief distributions if the agent had multiple priors) and observes the same signal, but the signal has no effect on his belief-based utility function. To come as close as possible to the ideal counterfactual, I designed a control condition, which I describe below.

In the control condition, subjects do not see a ball being drawn, but are asked to report their beliefs about signal informativeness ex-ante, for every possible signal realization. The procedure, known as the Strategy Method, is commonly used in experiments investigating strategic interactions in games (Brandts and Charness, 2009). To alleviate concerns about the non-comparability of the two treatments, I adopted procedures that specifically targeted the issues raised in the literature.⁸ I argue that a participant in the

⁵This holds true even in experiments that give participants a signal that is accurate with a certain probability (e.g. 75%). Before forming a posterior belief, one needs to answer the question of whether the observed realization reveals the state or should be attributed to noise.

⁶Thus, my study is also related to the literature on self-serving attribution bias. It has been extensively studied by psychologists (see Mezulis et al., 2004, for a meta-analysis of the existing studies) and, more recently, by economists (Coutts et al., 2020; Hestermann and Yaouanq, 2020; Van den Steen, 2004). None of the studies, however, consider the counterfactual discussed in my paper.

⁷As a robustness check, I use different definitions of a “good” signal relative to beliefs: considering only signals that are strictly better than the median belief or replacing median with the mean.

⁸One concern raised in the experimental game theory literature is that players may gain a better understanding of the game if they are induced to think about the best strategies from the perspective

control condition faces the same decision as a subject in the treatment condition but without the signal affecting his beliefs and belief-based utility.⁹

The results lend support to the hypothesis that asymmetry in updating is due to an instantaneous reaction to signals. While there is a 6 percentage point difference in the beliefs reported after “good” versus “bad” signals in the control condition, the additional effect of a “good” signal in the treatment condition is almost twice as large (10 pp). I show that the effect strongly depends on the subjects’ expectations. It is no longer present if a subject assigned zero prior probability to the rank indicated by the signal. Moreover, asymmetric updating about the box is followed by asymmetric updating about the rank. In the last part of the study, we again elicited subjects’ beliefs about their rank (the entire belief distribution). The data reveal that participants translate their beliefs about the signal into beliefs about the rank in a motivated way, with those who received “good” signals being more consistent in their final reports. In the end, even though more participants received signals that were below their median beliefs, the average posterior belief in the sample was not significantly different from the average (overconfident) prior.

Using subjects’ responses in questionnaires, I provide additional evidence to support my interpretation of the results as being driven by changes in belief-based utility. In the treatment condition, those participants who report experiencing hopelessness (a negative anticipatory emotion) tend to deviate more from the Bayesian benchmark. The effect is counteracted by the habitual use of emotion regulation strategies. Subjects who reported using more emotion regulation in their daily life tend to deviate less from Bayesian updating, even if they admit to feeling more hopeless. While only suggestive, the evidence supports the view that the treatment effect is stemming from the visceral, emotion-based reaction to signals that are indicative of a belief-based utility.

My work is based on the theoretical literature on overconfidence and belief formation. That literature postulates that people derive utility not only from physical outcomes

of other players. One can imagine that considering every possible signal in the control condition could influence subjects’ beliefs. I address this issue by presenting participants in the treatment condition with the screenshots from the control condition and asking them to consider every possible draw before they proceed to the main task. Moreover, I hope to alleviate another concern, the problem of framing the answers in the strategy method with the order of options, by randomizing the order of the signals presented to the subjects in both conditions.

⁹It is reasonable to assume that only realized signals induce subjects to revise their beliefs and bring them additional belief-based utility. The gain in utility can be sustained (or, in the case of unfavorable signals, mitigated) by distorting one’s beliefs about signal informativeness.

but also from their beliefs about the current or future state (Brunnermeier and Parker, 2005; Caplin and Leahy, 2019; Kőszegi, 2006). The individual can choose his beliefs but faces a trade-off between their accuracy (necessary to take the optimal action) and their desirability (a consequence of the non-monetary value beliefs bring to the agent). The tension is resolved by the agent manipulating his beliefs to the extent that he is not losing too much from actions taken based on those beliefs. Several studies demonstrated that agents significantly deviate from Bayes' rule when forming beliefs about their own intelligence or beauty (Buser et al., 2018; Coutts, 2019; Eil and Rao, 2011; Ertac, 2011; Grossman and Owens, 2012; Möbius et al., 2014; Schwardmann and Van der Weele, 2019). The main conclusion emerging from this strand of literature is that belief formation over ego-relevant characteristics significantly differs from learning about ego-neutral variables. At the same time, the direction of the effect and its magnitude vary across studies. The idea presented in this paper is related to research on emotions and decision-making (Lerner et al., 2015). One conclusion from the psychological literature is that emotions may influence decisions via changes in the content of thought, and vice versa. A similar hypothesis has been tested in a recent study of Engelmann et al. (2019) who investigate the impact of anxiety on wishful thinking. Using data from a carefully designed experiment, they show a causal effect of anticipatory anxiety on belief formation. Although I cannot argue about the causal impact of anticipatory emotions in my experiment, the suggestive evidence is in line with their findings.

The paper is organized as follows. The next section outlines the experimental design. In Section 3, I describe the main results. Section 4 presents the data from the final belief elicitation, and Section 5 describes the additional evidence. Section 6 concludes.

2 Experimental Design

The experiment consisted of two parts and is outlined in Figure 1. In the first part, subjects completed an IQ test intended to assess their cognitive ability. The second part included the elicitation of prior and posterior beliefs and a stage in which subjects received signals (or considered every possible signal realization in the control condition). I describe the procedures in detail in the following subsections.

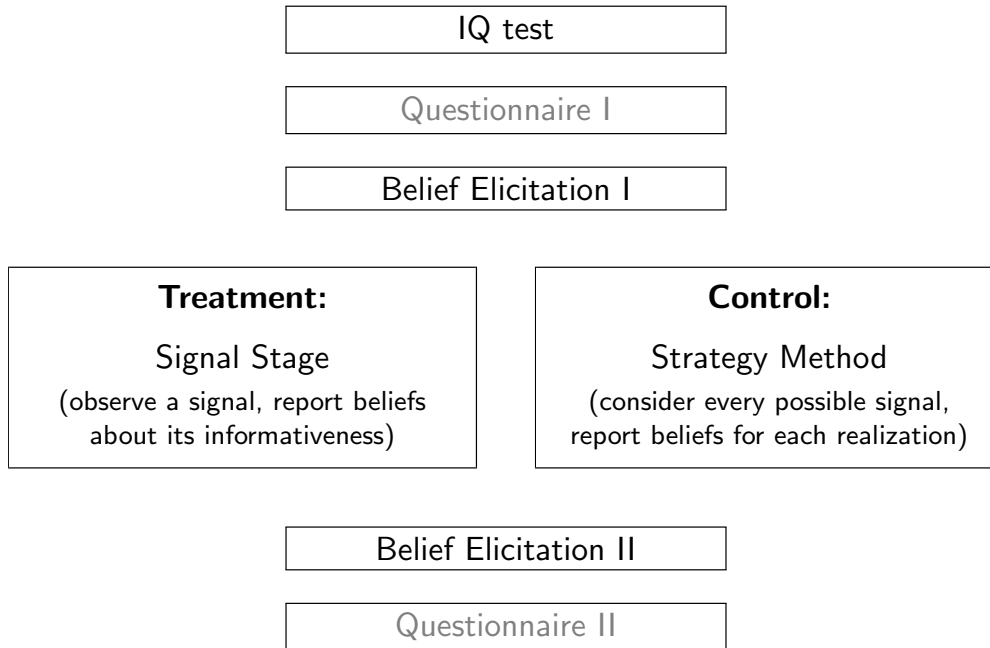


Figure 1: The outline of the experiment.

2.1 IQ Test

In the first part of the experiment, I evaluated the subjects' cognitive ability using an IQ test.¹⁰ The test consisted of 29 standard logic questions and participants were asked to solve as many of them as possible in 10 minutes. Individual scores were calculated based on the number of correctly answered questions minus the number of incorrect answers, and subjects were paid 0.75 Euro for every point they obtained.

¹⁰I decided to use intelligence as a basis for the learning exercise for several reasons. Firstly, it is known that intelligence correlates strongly with educational achievement, success in the labor market, and income. Because of that, I expect people to care deeply about their cognitive ability. Therefore, IQ measure seems to be a good candidate for a genuine ego-relevant parameter. Secondly, the literature provides evidence that people have biased beliefs about their cognitive ability (with overconfidence prevailing among men), which suggests that learning about one's cognitive ability may be one of natural settings in which the mechanism is in play.

Participants were informed that their earnings from the IQ test will be added to their earnings from the remaining parts of the experiment and paid at the end of the session. They were also informed that, although they will receive the entire sum of money at the end of the study, they will not learn immediately the exact number of points they obtained in the IQ test, nor how much money they earned in each part. Participants were informed that their IQ test results and the details of their payoffs will be available to them in one week after the session. Every participant received a personal link to a website on which his individual information was posted one week later.¹¹

2.2 Belief Elicitation

At the beginning of the second part, participants were told that they have to complete 3 tasks, for which they can earn up to 12 Euro. They were informed that *one task* will be drawn at random at the end of the session, and they will be paid only for that task.

In the first task, I elicited subjects' beliefs about their test scores being in the 1st, 2nd, ..., 9th and 10th deciles of the distribution of the test scores of 300 participants who took the same test in the BonnEconLab in previous sessions. I introduced 10 "ranks", with Rank 1 denoting the highest rank (assigned to participants whose IQ test scores were higher than or equal to the test scores of 90 – 100% of all participants), and Rank 10 denoting the lowest rank (defined analogously). The first task was to allocate 100 points among the ranks in a way that reflects one's beliefs about the relative performance in the IQ test.

The screen-shot of the computer interface used by subjects is presented in Figure 2. Participants were allocating points by dragging blue arrows to selected positions. They were informed that they can move the arrows back and forth to correct their choices. The text below the scales informed a participant how many points are being allocated to a given rank and the allocation was immediately appearing on the graph to the right.

¹¹This procedure served two purposes. First of all, I wanted to minimize dynamic concerns (e.g. subjects may adopt overly pessimistic beliefs to prepare themselves for the arrival of "bad news"). Secondly, this feature of the design enables me to collect data on who decided to check the test results. I describe the data on information acquisition in Appendix G.

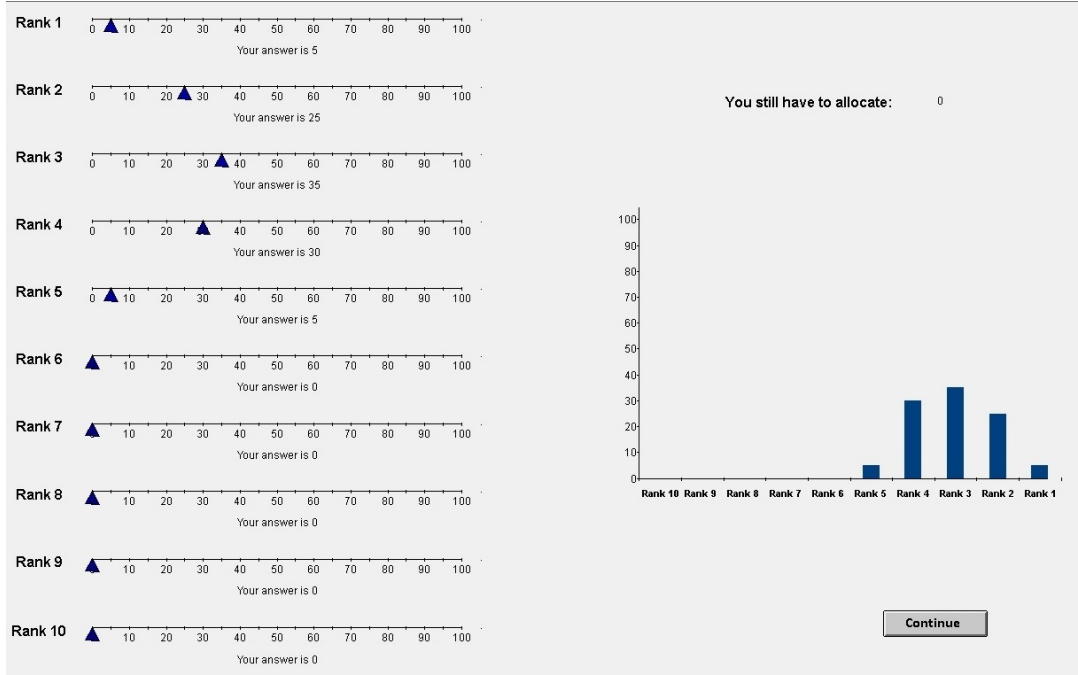


Figure 2: The screen-shot of the interface used by subjects in belief elicitation.

The number above the graph indicated how many points the participant still has to allocate before he can proceed to the next task.

To incentivize truthful reports, I used the Binarized Scoring Rule following Hos-sain and Okui (2013). The random variable X can take one of 10 values: $(1,0,\dots,0,0)$, $(0,1,\dots,0,0)$, ..., $(0,0,\dots,1,0)$, $(0,0,\dots,0,1)$; the position of 1 indicates in which decile subject's IQ test score fell. After receiving agent's report $x = (x_1, \dots, x_{10})$, where x_i denotes the share of points allocated to decile $i \in \{1, \dots, 10\}$, I observed his IQ test score in the k^{th} decile, and the agent won the prize if the QSR for multiple events,

$$s(x, k) = 2x_k - \sum_i x_i^2 + 1,$$

exceeded a uniformly drawn random variable with the support $[0, 2]$.

The formula was presented to the subjects in a simple way (avoiding mathematical notation). Importantly, I told participants the main implication of the method, that is,

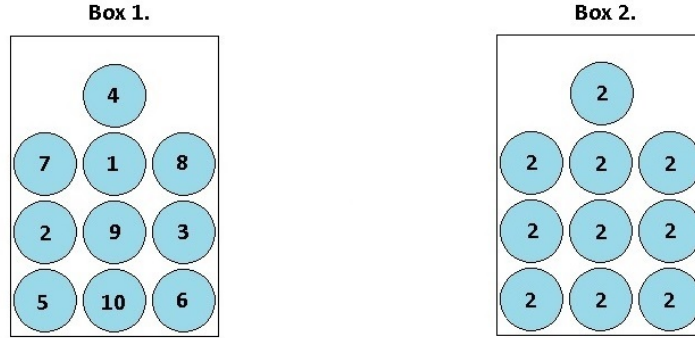


Figure 3: The composition of the boxes of a person whose rank was 2.

the probability of getting a large prize (12 Euro) is maximized when they allocate their points in a way that reflects their beliefs about their rank.

I followed the same procedure during the second belief elicitation, after the signal stage (after the strategy method in the control condition). However, during the first belief elicitation, subjects were not aware that they will be asked to state their beliefs one more time.

2.3 The Signal Stage

After eliciting the prior beliefs, participants were given instructions for the second task. We explained the nature of the task in a simple language, using pictures and two illustrative examples. The task was framed in a neutral way and described as follows.

There are two boxes: Box 1 and Box 2. Each box contains 10 balls with numbers written on them. Box 1 contains balls with numbers from 1 to 10, and every number appears exactly once. The composition of the second box depends on the subject's rank in the IQ test. Box 2 contains 10 balls that all have one number written on them, and this number is equal to the individual rank. The composition of the boxes of a person assigned Rank 2 is presented in Figure 3.

For every participant, the computer program randomly selected one of the two boxes. Next, a ball was drawn from the selected box and displayed on the participant's screen.

The participant did not know which box the ball was drawn from, but he knew that either box can be selected with equal probability. After seeing the ball, he had to state his beliefs about the box selected by the computer.

I used the same incentive-compatible elicitation method as for the prior and posterior belief elicitations. Participants had 100 points to allocate between Box 1 and Box 2 in proportions that reflect their beliefs about the source of the signal, and were rewarded for the truthful report with a higher probability of getting a large prize (12 Euro).

Importantly, subjects were instructed how to arrive at the Bayesian posterior given one's prior belief distribution. I explained it with an example in two steps. Firstly, I demonstrated how a person should allocate her points after different signal realizations if she knew precisely her rank. Then, I showed how a person should allocate her points if she was not sure about her rank, but was assigning a certain probability to it.

Step 1: How should a person ranked 2 allocate her points if she knew for sure that her rank is 2, and saw a ball with a number "2" on it? There are 10-times as many balls with "2" in Box 2 as there are in Box 1, hence it is 10-times as likely that the ball came from the second box. Therefore, the person should allocate 9 points to Box 1, and 10-times as many, 90 points, to Box 2 (the remaining point should be allocated to the box with higher probability).

Step 2: What if a person did not know her true rank, but she believed that there is 30% chance that her rank is 2? The same logic applies to this case. One can visualize 30% chance as 3 out of 10 balls in Box 2 having a number "2" on them.¹² In this imaginary case, there are 3-times as many balls with the number "2" on them in Box 2 as in Box 1, implying an allocation of 25 points to Box 1 and 3-times as many (75 points) to Box 2.

The interface enabled subjects to split their points in desired proportions without calculating the respective ratios. The screen-shot of the interface used in the second task is presented in Figure 4. Crucially, the text below the scale informed subjects about their

¹²One reason why I decided to introduce 10 balls was the ease of exposition in a case when a person is uncertain about his rank.

current allocation and the ratio between points allocated to the two boxes. By moving the cursor, participants could choose the number of points corresponding to allocating x -times as many points to one of the boxes (with $x \in \{1, 1.1, \dots, 99\}$). The graph below was illustrating the current allocation.

Before proceeding to the signal stage, participants were required to answer a set of control questions, designed to check their understanding of the task (including the steps necessary for arriving at the Bayesian posterior). The control questions also pointed out the aspects that participants may have missed at the first reading, but were necessary to fully comprehend the task.

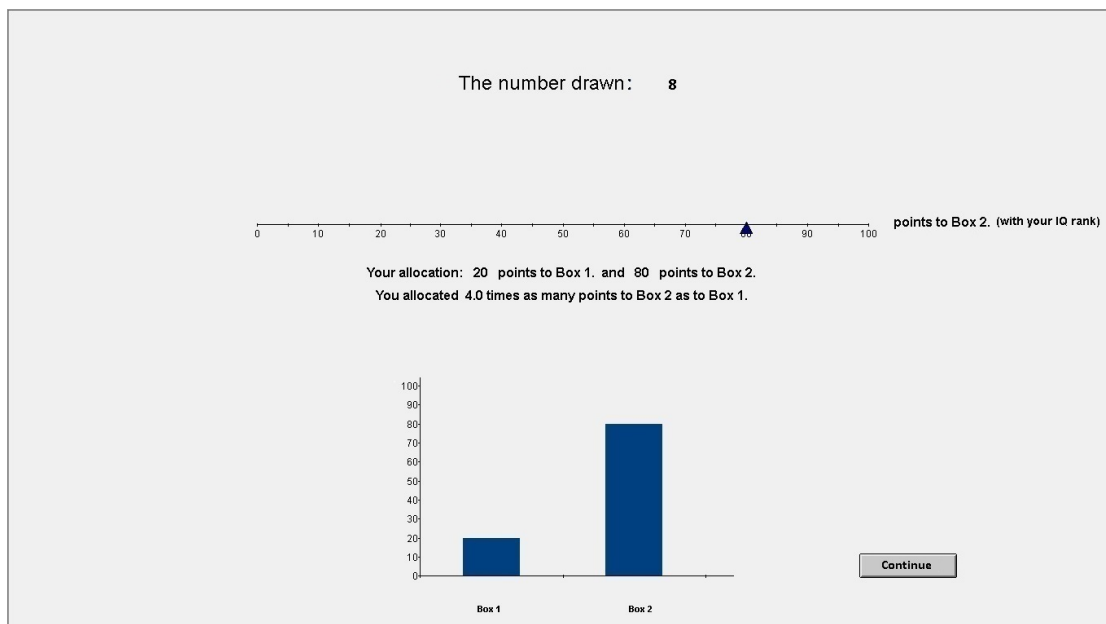


Figure 4: The screen-shot of the interface used in the second task (the signal stage).

2.4 Experimental Conditions

I introduced two experimental conditions: treatment and control. In the control condition, subjects did not see the number that was drawn but were asked to state their beliefs for every possible draw. The procedure, known as the Strategy Method, is commonly used in experiments investigating strategic interactions in games.

I informed participants in the control condition that the choices they are making are not entirely hypothetical. At the end of the session, one box was selected by the computer program and one ball was randomly drawn from the selected box. Subjects were paid as in the treatment condition, based on the decision that corresponded to the number drawn from the box. Note that the procedure is incentive-compatible as the probability of drawing any number is at least 5%.¹³

To alleviate concerns of the non-comparability of the two conditions, I adopted special procedures targeting the issues discussed in the literature. One concern raised in the experimental game theory literature is that players in the strategy method gain a better understanding of the game as a consequence of considering the problem from the point of view of different players. In my set-up, one can imagine that considering every possible signal realization may influence reported beliefs in the control condition.

For this reason, we asked the participants in the treatment condition to consider every possible signal realization *before* they saw the actual draw. Subjects were required to go through 10 slides, presented in random order, with the actual screen-shots of the interface displayed in the control condition. Participants were asked to contemplate a hypothetical decision in each slide before clicking on the button “Continue”, which appeared on the screen only after 15 seconds. While only subjects in the control condition were allowed to enter their choices, both groups were required to go through the task.

Another problem that may arise in the Strategy Method is framing the answers with the order of options. I addressed the issue by randomizing the order of the numbers displayed to a subject in the control condition, and the order of slides presented to participants in the treatment.

¹³However, if subjects were weighting the cost of cognitive effort against the expected payoff, they may exert less effort in the control condition. In this case, one would expect subjects to behave *less* rationally: their decisions would be characterized by a higher variance and they would end up further away from Bayesian update. This is the opposite of what I found.

2.5 Questionnaires

After each part of the experiment, I asked participants to fill in a 3-page questionnaire. The first set of questions, displayed on individual computer screens after the IQ test, included a short version of the Big-5 personality test (Gerlitz and Schupp, 2005) and the state-trait anxiety inventory STAI (Spielberger, 1983).

The Big-5 personality test was designed to measure personality along five dimensions: extroversion, conscientiousness, openness to experience, neuroticism, and agreeableness. The STAI measures the current state of anxiety and anxiety level as a personal characteristic. The second set of questions, answered by the participants after the main task, comprised the Emotion Regulation Questionnaire (Gross and John, 2003) and a subset of questions from the Achievement Emotions Questionnaire (Pekrun et al., 2011).

The Emotion Regulation Questionnaire was designed to assess the habitual use of two strategies commonly used to alter emotions. To alleviate the emotional impact of a situation, one may try to reinterpret it in a different way. This emotion regulation strategy, broadly referred to as *reappraisal*, relies on “applying mental models to the often ambiguous and incomplete information” (Uusberg et al., 2019). The second emotion regulation strategy, *suppression*, involves “inhibiting ongoing emotion-expressive behavior” (Gross and John, 1998, cited in Uusberg et al., 2019).

People differ in their use of reappraisal and suppression, and these differences have implications for their experiences of emotions, behavior in response to those emotions, and general well-being (Gross and John, 2003). The habitual use of the two strategies is measured by the degree to which subjects agree with particular statements, e.g. “I keep my emotions to myself” or “When I want to feel less negative emotion, I change the way I’m thinking about the situation”. I use the exact 10-item questionnaire developed by Gross and John (2003).

The Achievement Emotions Questionnaire was designed to measure *achievement emotions* (emotions that are directly linked to achievement activities or achievement outcomes) experienced by students in academic settings (Pekrun et al., 2011). I adopted

part of the questionnaire to measure the following test-related emotions: enjoyment, hope, pride, relief, anger, anxiety, shame, and hopelessness.

Participants in both conditions were asked to report what they felt *after* learning the nature of the task, but *before* they saw the number(s). They had to indicate, using a 7-point Likert scale, how strongly they agree (or disagree) with various statements, e.g. “I was proud of how well the test went”, or “I was angry about the task I had to do” (see Appendix for the entire list of questions and the instructions).

3 Results

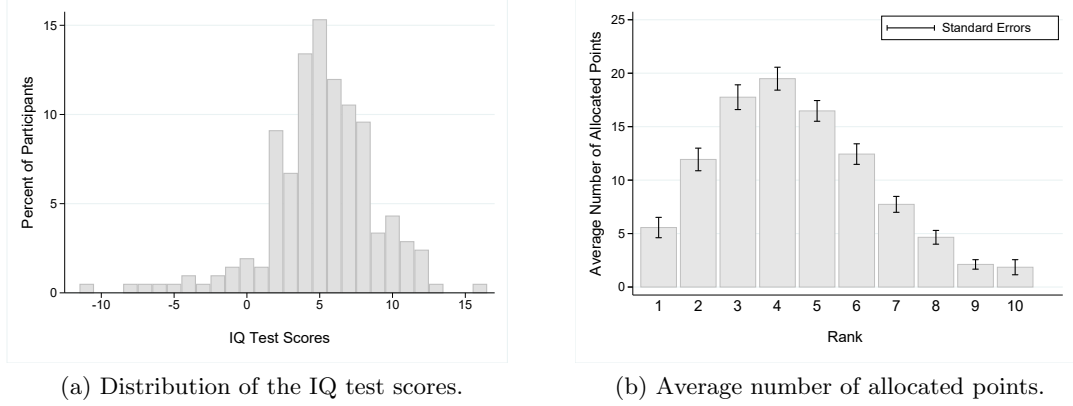
The experiment took place in August 2020 in the BonnEconLab at the University of Bonn.¹⁴ I conducted 52 sessions, with 1 to 6 participants in each session. I collected data from 167 participants in the treatment condition and 55 participants in the control condition. The experiment lasted around 80 minutes and the participants earned 21.25 Euro on average. In the following section, I report the analysis based on the data from 209 participants who correctly answered at least half of the control questions (I excluded 13 participants, that is 5.8% of the sample).

3.1 IQ Test Results and Individual Ranks

Figure 5 presents the distribution of the IQ test scores and ranks assigned to the participants based on the test results. The IQ test score distribution is fairly symmetrical (skewness -0.83), with a mean of 5.13 and a standard deviation of 3.73. The average rank is 5.65 with a standard deviation of 2.67. Importantly, there is no significant difference in the average IQ test score or rank assigned to the participants in the treatment and control group (see Appendix A).

¹⁴Due to the Covid-19 pandemic, I followed special procedures to ensure the safety of participants and others involved. The number of participants per session was restricted to 6 to ensure each participant a place in a separate room. Desks, chairs, and computer equipment were disinfected after every session and the rooms were aired before every session for at least half an hour. At the time of the experiment (August 2020), the Covid-19 pandemic was mostly under control in Germany; the lockdown restrictions were eased, allowing restaurants, schools, and public places to open with appropriate safety measures.

Figure 5: IQ Test Results and Individual Ranks.



3.2 Prior Beliefs about Rank

Before the main task, we elicited from every participant his entire belief distribution. I analyze the data in two ways. Firstly, I look at the aggregate belief distribution. Then, I examine individual distributions and report the averages of individual measures (these include mean belief about rank, median and range).

To look at the aggregate of individual belief distributions, I treat separately every decision to allocate x points, $x \in \{0, \dots, 100\}$, to rank k , $k \in \{1, \dots, 10\}$. For each of the 10 ranks, I calculate the average number of points allocated by the participants. The resulting aggregate distribution is presented on Panel b) in Figure 5 (each bar indicates the average \pm standard errors). It is visibly skewed to the right, with the mean belief of 4.47 and the median of 4. On average, the subjects appear to be *overconfident*, as they put a higher probability mass on lower (better) ranks.

In Table 1, I report the averages of individual measures of belief distribution. I look at the average mean belief, median belief, the first and third quartile, and range.

Table 1: Individual belief distributions.

	Mean Belief	Q1	Median	Q3	Range
Mean	4.47	3.71	4.45	5.16	4.89
(Std. Dev.)	(1.75)	(1.74)	(1.79)	(1.87)	(1.57)

Importantly, there is no significant difference between the treatment and the control group (see Appendix A). The averages, however, mask the fact that only 26 participants revealed symmetric belief distribution. Almost half of all subjects (100 participants) revealed a positively skewed belief distribution, and the remaining 83 participants revealed a negatively skewed belief distribution (the average difference between mean and median in both groups was 0.21). I define a person to be *overconfident* if his median belief is lower than his true rank. Similarly, I use a term *underconfident* to describe a person who assigns 50% or more probability mass to ranks higher than his true rank. A person is defined to be *unbiased* if his median belief matches his true rank.¹⁵ Using this definition, there are 127 overconfident, 58 underconfident, and 24 unbiased participants in my sample. Importantly, there is no significant difference in the average bias (defined as a difference between the true rank and the median belief) between the treatment and the control group (see Appendix A).

3.3 Decisions in the Main Task

The main experimental task, neutrally framed as “the second task”, differed depending on the condition. In the treatment condition, subjects observed one number and reported their beliefs about the box from which the number was drawn. In the control condition, participants saw, in random order, numbers from 1 to 10, and stated a report for each one of them. In this section, I describe the raw data on subjects’ decisions in the two conditions and present the results of the data analysis with and without using subjects’ decisions in the control condition.

3.3.1 Reports in the Treatment Condition (Raw Data)

Firstly, I describe the raw data on the decisions made by participants in the second task. This was our main task: allocating points to Box 1 (with numbers from 1 to 10) and Box 2 (indicating one’s rank) in a way that corresponds to one’s beliefs about the source of the signal. I interpret points allocated to Box 2 as the probability that a subject assigns to the event that the number displayed on the computer screen is his rank.

¹⁵In common language, Rank 1 denotes “the highest” rank, while Rank 10 is “the lowest”. To avoid confusion, I will not use the customary phrases, but the terms that match the values (for example, a subject whose rank is 5 and median belief is 4 puts higher probability on *lower* ranks).

Figure 6: Points allocated to Box 2 in the Treatment condition.

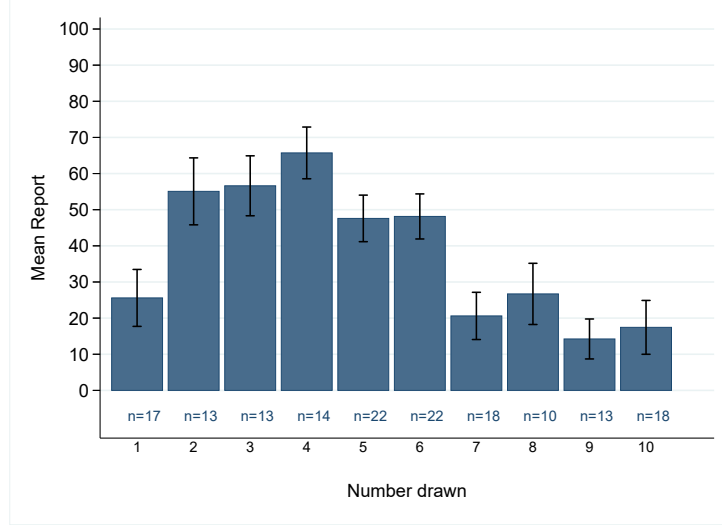
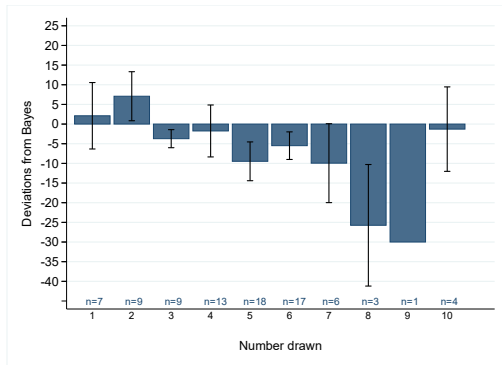
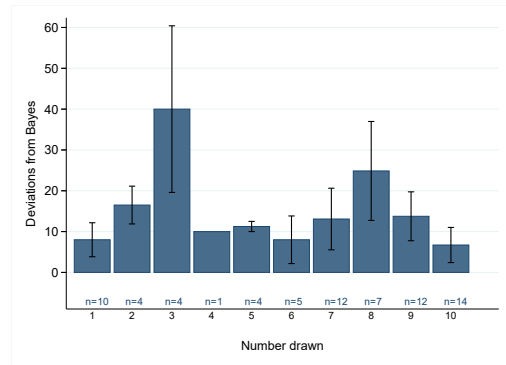


Figure 6 presents the average number of points allocated to Box 2 after a signal received in the treatment condition. The numbers above the x-axis indicate how many participants received a given signal and stated a report. For example, 14 participants in the treatment condition saw “4” displayed on their computer screens and allocated, on average, 65 points to Box 2 (revealing the average subjective probability of 65% that the number “4” is their rank). It is useful to contrast these decisions with the Bayesian benchmark. For each participant, I calculated a Bayesian posterior about the box given his priors and signal realization. The average deviations from the Bayesian update in the treatment condition are presented in Figure 7. I separately plotted cases in which

Figure 7: Mean deviation from Bayes for different signals.



(a) Treatment condition, signals within priors.



(b) Treatment condition, signals outside priors.

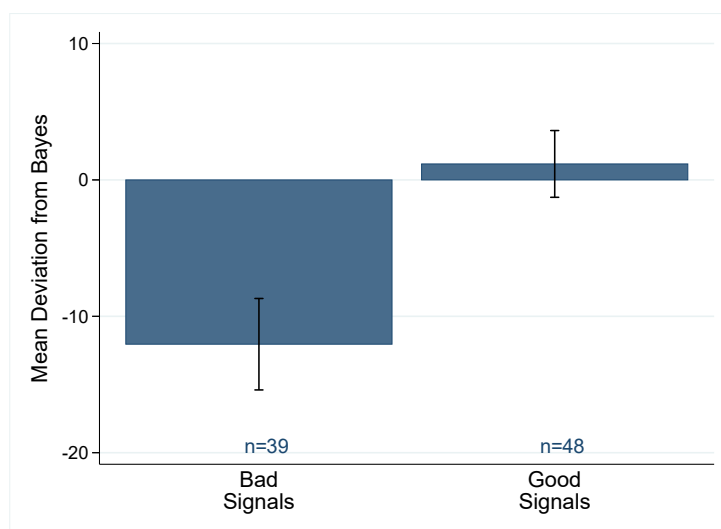
subjects assigned a non-zero prior probability to the number displayed on the screen (the graph on the left) and those in which subjects assigned the prior probability of zero (the graph on the right). I refer to the latter as “outside priors”. One can notice that the average decisions are below zero for higher numbers – after worse signals, subjects tend to allocate fewer points than prescribed by the Bayes’ rule. After better signals (indicated by lower numbers), subjects’ decisions are closer to the bayesian benchmark.

However, by looking only at the signals’ values one can miss an important point: signals might be perceived differently depending on subjects’ expectations. A person who believes that her rank is “5” might perceive a signal “4” as a “good” signal. At the same time, a person who firmly believes that her rank is “1” can be disappointed after seeing a “4” and view it as a “bad” signal. We take this into account in the next section.

3.3.2 Results Based on the Treatment Condition

Our experimental design enables us to define the signal’s valence depending on subjects’ expectations.¹⁶ In Figure 8, I present average deviations from the Bayesian update after signals that were worse than one’s median belief (the left bar) and those that were better or equal to one’s median belief (the bar on the right). Participants tend to allocate fewer points after signals that were worse than their median belief.

Figure 8: Deviations from Bayesian update for signals above/below one’s median belief.



¹⁶Previous work on asymmetric updating mostly used binary state and signal space, and referred to the signal indicating a higher state as a “good” signal (see the literature review in Appendix C).

The pattern visible on Figure 8 is confirmed by estimates presented in Table 2. The dependent variable is the number of points allocated to Box 2 (indicating one’s rank). The independent variable “Bayes” denotes the number of points prescribed by the Bayes’ rule. The variable “Good Signal” takes value 1 if the signal was lower or equal to the median belief and zero otherwise.¹⁷ In the second column, we control for individual median belief, and in the last column, we add a control for individual rank (both variables could potentially influence the probability of receiving a “good” signal). The sample is restricted to the participants who assigned non-zero prior belief to the signals they received.¹⁸ The coefficient at the “Good Signal” variable is around 13.0 and is significant at the 1% level, meaning that subjects report 13 percentage points higher beliefs that the signal is their rank after a “good” signal.

Table 2: The effect of the signal’s valence.

	(1)	(2)	(3)
Bayes	0.956*** (0.121)	0.954*** (0.122)	0.961*** (0.123)
Good Signal	13.629*** (4.241)	13.724*** (4.256)	12.976*** (4.418)
Median Belief		-0.815 (1.167)	-0.537 (1.245)
Rank			-0.604 (0.914)
Constant	-9.527 (7.651)	-5.757 (9.383)	-3.819 (9.862)
N	87	87	87

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 by participants in the treatment condition. “Bayes” denotes the number of points that should be allocated according to the Bayes’ rule. The sample is restricted to subjects who received a signal to which they assigned non-zero probability. “Good Signal” indicator variable takes value 1 if the signal was below or equal to the median of subject’s belief distribution, and 0 otherwise.

¹⁷The result is robust to using different definitions of a “good” signal relative to beliefs: considering only signals that are lower than the median belief as “good” signals, or replacing median with the mean of individual belief distribution.

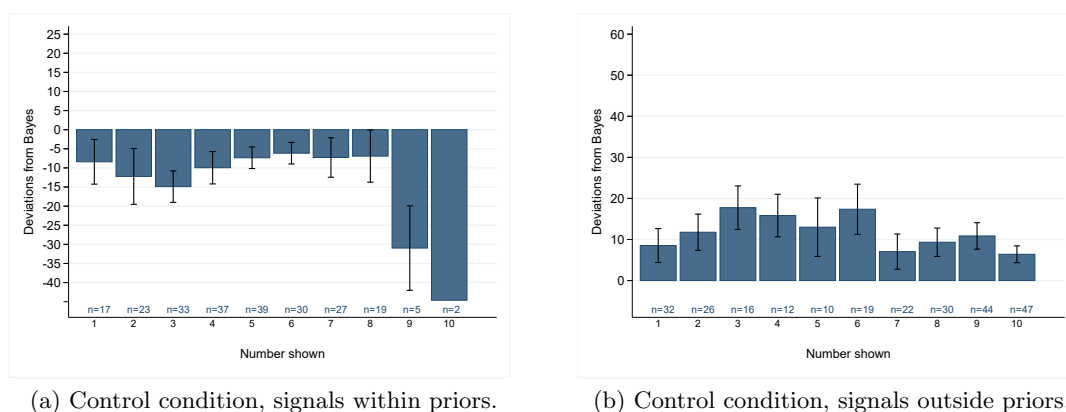
¹⁸The estimation based on the entire sample and controlling for signals to which participants assigned zero prior probability (“outside priors”) yield similar results, see Appendix B.

Importantly, this effect would not be captured if we defined “good” and “bad” signals in absolute terms. In the Appendix B.1, we replicate Figure 8 and Table 2 using the definition of “good” and “bad” signals commonly used in the literature: we define signals from 1 to 5 as “good” and signals from 6 to 10 as “bad”. The effect is much lower and not significant at any acceptable level. The result points toward the importance of taking into account subjects’ expectations to determine the signal’s valence. Being among 50% best performers is hardly a good news if you expect to be among the top 10%. We later argue that asymmetric updating is mostly driven by an emotional reaction to signals, and one’s prior beliefs likely serve as a reference point from which the signals are evaluated.¹⁹

3.3.3 Data Analysis Using Control Condition

In this section, I describe the data from the control condition. In Figure 9, I present participants’ decisions separately for signals to which they assigned non-zero prior probability (Panel a), and those to which they assigned the prior probability of zero (Panel b). The averages are consistently below zero in the left panel, meaning that subjects tend to allocate fewer points than prescribed by the Bayes rule regardless of the signal under consideration. Note that the decisions were incentivized, thus allowing us to argue

Figure 9: Mean deviation from Bayes for different signals.



¹⁹It is important to note that we gave participants much finer signals than most of the literature (usually signaling whether or not a subject is in the upper half of the distribution). We admit that subjects are likely to respond differently to coarser/finer signals and taking a simple average might not be a perfect comparison. Whether or not it is the case remains an open question for future research. Our hypothesis is that the average over finer signals is likely to be stronger than a response to a coarser signal due to a stronger emotional reaction: learning that one’s performance is in the bottom 10% is likely to be more painful than a signal of being in the lower half.

that participants made the best decisions using their prior beliefs about their rank and information from the signal. The only difference between the two conditions is that in the treatment condition participants received an actual signal. In Table 3, I present the results of a regression analysis based on the data from both conditions. I restrict the sample to the participants who assigned a non-zero prior probability to the signal that appeared on their screen (the estimation based on the entire sample controlling for signals “outside priors” yielded similar results, see Appendix B). The dependent variable is the number of points allocated to Box 2. Firstly, I regress it on the number of points prescribed by the Bayes’ rule (the independent variable “Bayes”) and a treatment dummy. As reported in the first column, both coefficients are positive and significant. In the second specification, I add an indicator variable “Good Signal”, which takes value 1

Table 3: The effect of the signal’s valence.

	(1)	(2)	(3)	(4)	(5)
Bayes	0.827*** (0.093)	0.765*** (0.094)	0.767*** (0.093)	0.767*** (0.093)	0.764*** (0.092)
Treatment	5.761* (2.982)	6.382** (2.884)	1.012 (4.111)	1.015 (4.125)	1.005 (4.170)
Good Signal		8.608*** (2.783)	5.944* (3.368)	5.936* (3.382)	5.943* (3.353)
Treatment \times Good			9.474* (5.415)	9.478* (5.429)	10.247* (5.511)
Median Belief				0.048 (1.124)	-0.231 (1.151)
Rank					0.661 (0.596)
Constant	0.331 (5.296)	-1.126 (5.466)	0.381 (5.660)	0.164 (7.443)	-2.306 (7.684)
N	319	319	319	319	319

Standard errors clustered at individual level. Their values in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 in the treatment condition. “Bayes” is the number of points that should be allocated according to the Bayes’ rule. The sample is restricted to the participants who received (or considered) a signal to which they assigned non-zero probability. “Treatment” is a variable indicating assignment to the treatment condition. “Good Signal” indicator variable takes value 1 if the signal was below or equal to the median of subject’s belief distribution, and 0 otherwise.

if the signal was better or equal to one’s median belief. A high and significant coefficient informs us that subjects tend to allocate more points to Box 2 in face of “good” signals. In the third specification, I add our main coefficient of interest – the interaction between the “Good Signal” and the “Treatment” variable. The coefficient at the interaction term is equal to 9.5 and significant at the 10% level. Importantly, the effect is similar if we add controls for individual rank and median belief.

One may worry about the fact that participants in the treatment condition with a probability of 50% decide about a signal that is their rank, while in the control condition, they decide about all 10 numbers. As a robustness check, we restrict the sample to the participants who saw a random number in the treatment condition. The results are gathered in Table 18 in Appendix B. While the coefficient at the interaction term is not significant ($p\text{-value} = 0.152$), due to the small sample size, the coefficient of 9.8 is not different from the one presented in Table 3. We conclude that the differences in the probability of observing one’s rank are not driving our results.

As an additional exercise, one that solves the problem of selection and uses the data in the most efficient way, I construct a matching estimator. The details of the procedure and the results are described in Appendix D. Here, let me briefly summarize it. For every participant in the treatment condition, I construct a counterfactual outcome using all observations from the control *regarding the same number* as the one seen by the subject in the treatment condition. However, not all of these observations receive the same weight. Those participants in the control condition, whose true rank and prior belief distribution were closer to the rank and beliefs of the participant in the treatment condition, receive a higher weight. I interpret the counterfactual as what the subject would report if he were in the control condition. The results based on matched data lend further support to the initial hypothesis. There is a significant difference in the reported probability of a signal being informative in the treatment condition compared to the counterfactual. The effect is entirely driven by differential responses to signals that are above and below one’s median prior belief. For favorable signals to which subjects assigned a non-zero prior probability, the difference amounts to 15.7 percentage points. In contrast, there is no difference in subjects’ reports after unfavorable signals.

3.4 Payoffs from the Main Task

In this section, I look at the payoffs from the main task in the treatment and the control conditions. In both conditions, subjects were remunerated with “lottery tickets”: a higher probability of receiving a large reward of 12 Euro. Decisions of participants in the treatment condition brought them, on average, 65.5% probability of receiving a large reward. At the same time, the average payoff taking all decisions in the control condition amounts to 78.2% probability of receiving a large reward (see Table 4). However, the actual payoffs subjects received in the Control condition were much lower and not significantly different from the payoffs of participants in the Treatment condition. The discrepancy between the two is due to the fact that participants made much worse decisions when deciding about their actual rank than when deciding about a random number. This holds true both for the Treatment and the Control condition.

There are notable differences when comparing decisions in the Treatment and the Control condition separately for signals equal to one’s rank and other signals, see Table 24 in Appendix F. When guessing about their actual rank, participants in the Treatment condition did better than subjects in the Control, although the difference of 8.4 percentage points is not statistically significant (p-value = 0.118). At the same time, subjects in the Control condition performed better when evaluating signals different from their true rank – the difference of 6.15 percentage points is significant at the 5% level (p-value = 0.024). We describe the differences in detail in Appendix F, here let us conclude that average payoffs in the two conditions mask considerable heterogeneity, which should be taken into account when making welfare comparisons.

Table 4: Differences in payoffs (probability of receiving a large reward) from the main task.

	Treatment	Control	p-value		
			H_0 : Diff < 0	Diff \neq 0	Diff > 0
Payoffs (all decisions)	65.57% (2.79)	78.16% (1.33)	0.000	0.000	1.000
Payoffs (actual draw)	65.57% (2.79)	64.96% (5.60)	0.541	0.919	0.459
N	160	49			

4 Belief Elicitation II

In this section, I take a closer look at beliefs about the rank elicited after the main task. I attempt to answer the following question: Do beliefs about the box translate to the posterior about the rank? Furthermore, I discuss the caveats of repeated belief elicitation and their consequences for the interpretation of the results.

4.1 Raw Data

Before delving into the analysis, I present the raw data on beliefs about the rank before and after the task. In Figure 10, I replicate Figure 5b), juxtaposing the data from the first and the second belief elicitation. The graphs were created using only observations from the Treatment condition. There is little difference in aggregate beliefs before and after the signals. This result may seem surprising as it suggests that, on aggregate, subjects have not learned much, even though they received informative signals. I will show in the following section that it is not the case that our treatment manipulation failed to move participants' beliefs. Rather, it is a consequence of conservatism (updating too little in response to informative signals) and asymmetry (updating differently after negative and positive signals), as well as the fact that many of the “bad” signals that subjects received were outside their prior belief distributions.

Figure 10: Average number of points allocated to 10 ranks.

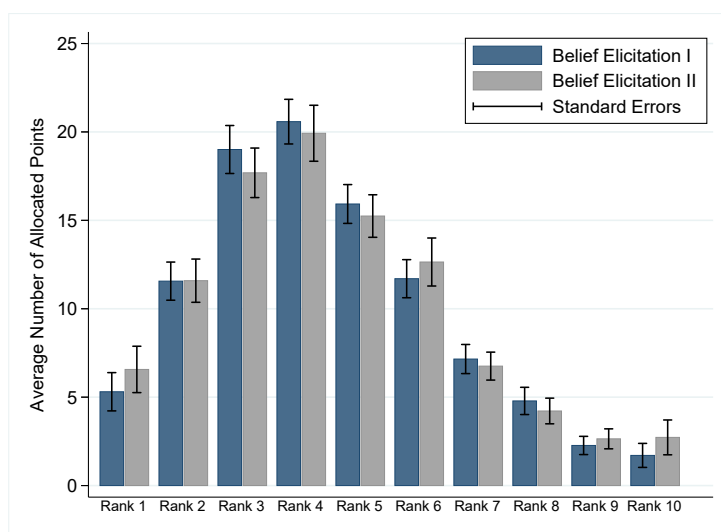
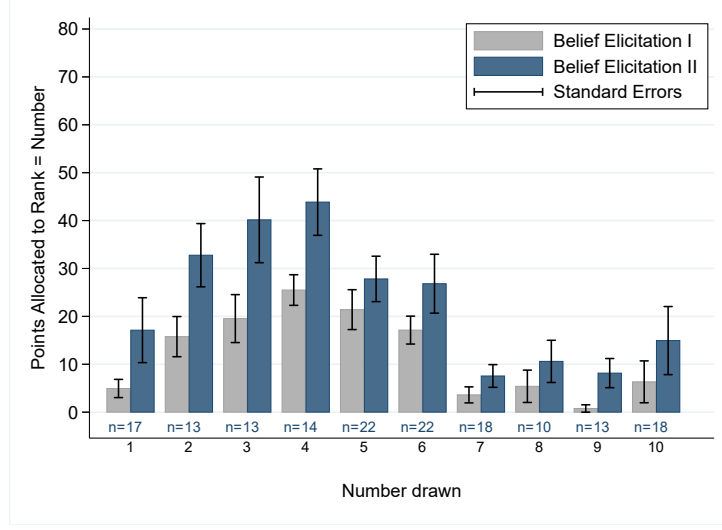


Figure 11: Points allocated to the relevant rank before and after the signal.



First of all, I show that the signals indeed moved subjects' beliefs about the respective rank. In Figure 11, I plot the average number of points allocated to the rank indicated by the signal (i.e. if a participant received a signal "2", only his allocations to Rank 2 are included). One can notice significant differences between the prior and posterior beliefs, and that those differences vary depending on the signal received. In Table 5, I present average allocations after "good" and "bad" signals separately for signals to which subjects assigned non-zero prior probability (I refer to them as "within prior") and those to which subjects assigned zero prior probability ("outside prior"). Two things

Table 5: Average number of points allocated to the relevant rank before and after the signal.

	"Good" Signals		"Bad" Signals	
	within prior	outside prior	within prior	outside prior
Belief Elicitation I	27.52 (2.43)	0 (0)	17.18 (1.54)	0 (0)
Belief Elicitation II	47.75 (4.14)	6.52 (2.73)	23.77 (2.89)	6.08 (1.62)
Difference	20.23	6.52	6.59	6.08
N	48	21	39	52

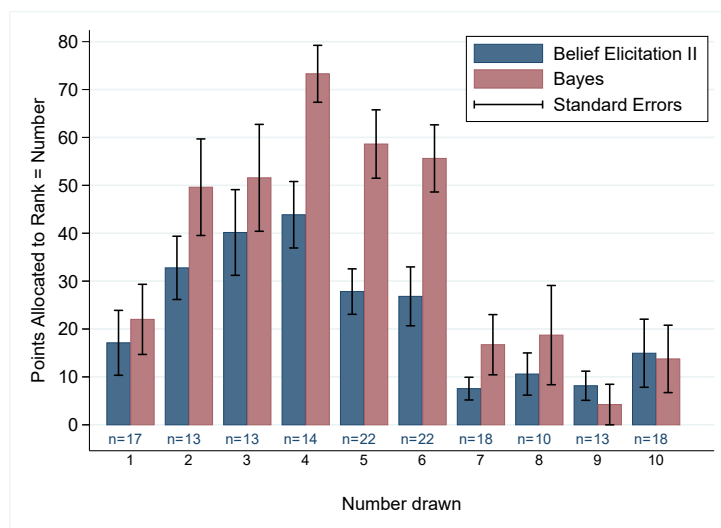
*Standard errors in parentheses.

are worth noting. Firstly, although participants received more “bad” signals ($n=91$) than “good” signals ($n=69$), they received more “good” signals to which they assigned non-zero probability ($n=48$) than “bad” signals of a similar kind ($n=39$). Secondly, there is a larger change in beliefs after “good” signals within priors than after “bad” signals within priors (20.23 versus 6.59 difference). In the following section, I analyze these differences in relation to the bayesian benchmark.

4.2 Data Analysis: Bayesian Benchmark

In Figure 12, I contrast beliefs elicited after the signal with the Bayesian benchmark, calculated based on the subject’s prior beliefs about the rank.²⁰ If a subject assigned zero prior probability to the signal that he received I assume the benchmark to be zero. There are two things to be noted. Firstly, subjects tend to allocate fewer points than prescribed by Bayes’ rule. This could be a sign of conservatism, that is, under-reaction to new information (ref.). Secondly, the differences vary depending on signal realization. We use regression analysis to examine to what extent they are driven by differential responses to “good” and “bad” news.

Figure 12: Points allocated to the rank corresponding to the signal.



²⁰Note that, since the signal is either entirely informative or uninformative, it should not affect any rank other than the one that corresponds to its realization. The prior beliefs on the relevant rank are all we need to calculate the Bayesian posterior.

The estimation results are presented in Table 6. The dependent variable is the number of points allocated to the rank corresponding to the received signal. The first two columns report estimates based on observations from participants who received signals to which they assigned non-zero probability (signals “within prior”). The regression in the last column includes only participants who received signals to which they assigned a prior probability of zero (“outside prior”). In the first specification, I regress the dependent variable on the number of points they should have allocated according to Bayes’ rule (the “Bayesian Posterior” variable). The coefficient at the “Bayesian Posterior” variable is 0.92 and statistically significant. Note, however, the negative coefficient at the constant variable, which informs us that participants allocated fewer points than they should have. In the second specification, we add the “Good Signal” variable, which takes value 1 if a signal was above or equal to one’s median belief. The coefficient at the “Good Signal” variable is high and significant – participants tend to allocate 15.5 points more to the corresponding rank if they received a good signal. Thus, they revealed 15.5 percentage points higher beliefs that the signal is their rank after a “good” compared to a “bad” signal. The result remains the same if we control for individual rank and/or median

Table 6: The effect of signal valence on beliefs about the respective rank.

	Signals “within prior”		“outside prior”
	(1)	(2)	(3)
Bayesian Posterior	0.921*** (0.117)	0.811*** (0.114)	
Good Signal		15.547*** (4.361)	0.447 (3.088)
Constant	-26.735*** (8.388)	-27.687*** (7.869)	6.077 (1.656)
N	87	87	73

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 by participants in the treatment condition. “Bayes” denotes the number of points that should be allocated according to the Bayes’ rule. “Good Signal” indicator variable takes value 1 if the signal was below or equal to the median of subject’s belief distribution, and 0 otherwise. The results are virtually unchanged if we control for individual rank and/or median belief.

belief (not shown in the table). There is no significant effect of signal valence for signals that were “outside” subject’s prior belief distribution – the coefficient at the “Good Signal” variable is not significant in the last column in Table 6.

Several points should be kept in mind when interpreting the data from the second belief elicitation. First of all, one problem common in experiments measuring beliefs multiple times is that consistency motives may play a role. It has been shown in the literature (Falk and Zimmermann, 2017) that people prefer to act consistently in order to signal their skills to others. Despite our best efforts to ensure anonymity and instruct subjects to treat each part of the experiment independently, the second belief elicitation data may be tainted by the desire to be seen as a consistent decision-maker.²¹ If the consistency motives are in play, and people desire to make consistent reports in the two elicitation procedures, then what we found is a lower bound on the effect.

Secondly, while we explained to the subjects in intuitive terms how to arrive at a Bayesian posterior about the box, we provided no such guidance on how to translate the prior belief distribution and the signal to the posterior belief about the rank (nor we explained how to arrive at the posterior belief distribution given one’s beliefs about the box). We believe this approach has both advantages and disadvantages. On the one hand, we did not frame participants in any way on what “should” be done in the experiment. On the other hand, we are losing control over what participants believe to be a rational course of action in an environment that is far from natural.²² Yet, the posterior beliefs about the box and the belief distribution elicited in Belief Elicitation II are surprisingly consistent, lending credit to the use of these methods and corroborating the main results. We describe the comparison between the two in the following section.

4.3 Data Analysis: Consistency

During the experiment, participants’ beliefs were elicited three times: before the task (Belief Elicitation I), as a part of the main task (beliefs about the box), and after the task (Belief Elicitation II). In Section 3, I described the data from Belief Elicitation I

²¹This concern is alleviated in our main analysis, as it is based on a comparison between the Treatment and the Control, and there is no reason to believe that consistency motives differ in the two conditions.

²²Although students are regularly given grades that are, to some extent, based on their relative performance, it is rather unusual to be asked to specify the entire belief distribution.

and subjects' beliefs about the box, while in the previous section, I contrasted Belief Elicitation I and II. There is one more comparison to be made, namely, beliefs about the box and Belief Elicitation II. This comparison enables us to answer the question: Do beliefs about the box translate to the posterior about the rank? The answer is important for the validity of our results – whether the asymmetry in beliefs about signal informativeness that we captured has any effect beyond the decisions in the main task.

To investigate this question, I construct a new variable “Consistent Posterior” that is a Bayesian posterior based on the subject’s beliefs about the box. Then, I examine its relation to the posterior beliefs about rank. In Table 7, I present the results of a regression analysis based on observations from the treatment condition (those participants received actual signals), separately for signals to which subjects assigned non-zero prior probability (left side of the table) and signals to which subjects assigned zero prior probability (on the right). The dependent variable is the number of points allocated in Belief Elicitation II to the rank corresponding to the signal received.

The coefficient at the Consistent Posterior variable in Specification 1 tells us that this number is strongly related to the number of points subjects should have allocated

Table 7: The effect of signal valence on beliefs about the respective rank.

	“within prior”			“outside prior”		
	(1)	(2)	(3)	(1)	(2)	(3)
Consistent Posterior	0.783*** (0.076)	0.712*** (0.082)	0.537*** (0.117)	0.134** (0.063)	0.134** (0.063)	0.185** (0.075)
Good Signal		8.926** (4.230)	-12.173 (11.064)		0.081 (3.020)	2.660 (3.627)
Good × Consistent			0.332** (0.161)			-0.175 (0.138)
Constant	-14.142** (5.354)	-14.436*** (5.250)	-5.045*** (6.883)	4.391*** (1.874)	4.368** (1.808)	3.705* (5.354)
N	87	87	87	73	73	73

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 by participants in the treatment condition. The independent variable “Consistent Posterior” refers to the Bayesian prediction based on the belief about the box. “Good Signal” indicator variable takes value 1 if the signal was better or equal than the median of individual prior belief distribution, and 0 otherwise. The results are virtually unchanged if we control for individual rank and/or median belief.

given their beliefs about the boxes. While the coefficient is significantly lower than 1, it is clear that the beliefs about the box affect subjects' posterior beliefs about the rank. The relation is much weaker for the signals "outside" subjects' prior belief distributions. Moreover, for signals "within prior", there is a strong effect of a "good" signal, significant at the 5% level. Even after controlling for their decisions about the boxes, participants tend to allocate more points to the respective rank after a "good" signal, but only if they assigned a non-zero probability to the signal they received. In Specification 3, I add an interaction of the two variables. For signals "within prior", the coefficient at the interaction term is equal to 0.33 and significant at the 5% level. The results show that, in addition to motivated reasoning about the source of the signal, there is an asymmetry in translating those beliefs to the posterior beliefs about the rank, and participants who received "good" signals were more consistent in their final reports.

4.4 Payoffs from Belief Elicitation I and II

In this section, I briefly describe the payoffs that participants received from Belief Elicitation I and II (more information could be found in Appendix F). I restrict the sample to the participants in the treatment condition. Recall that subjects were remunerated in "lottery tickets", that is, the probability of receiving a large reward of 12 euros. I refer to these probabilities as "payoffs". First of all, there is a significant difference between payoffs received for reports made before and after the signal. Participants received 3.67 percentage points higher probability (7.75% increase in relative terms) in the second belief elicitation. However, their payoffs would have been significantly higher, by 2.86 percentage points (5.6% increase in relative terms), if they updated their beliefs rationally. If we restrict the sample to the subjects who received signals "within prior" (for whom we can calculate the bayesian update), the gap between the payoffs from the rational update and the actual beliefs increases to 8.2 percentage points (14.7% in relative terms). Moreover, participants would have obtained higher payoffs in the second belief elicitation if they were consistent with their reports about the signal. The actual payoffs were 2.4 percentage points lower than the payoffs subjects would have obtained if they translated beliefs about the signal to the beliefs about the rank with no additional asymmetry (the difference is significant at the 10% level). The results show that, although

subjects updated their beliefs in a way that ensured higher payoffs, they could have done better if they updated rationally, or were consistent with their reports about the boxes.

5 Additional Evidence

In this section, I examine a complementary data set of subjects’ answers to questionnaires described in Section 2. Firstly, I look at the subjects’ personality traits, anxiety levels, as well as habitual use of emotion regulation strategies, and report their correlations with subjects’ decisions in the second task.

5.1 Emotion Regulation Questionnaire

In this section, I examine subjects’ answers to the emotion regulation questionnaire, BIG-5 and STAI. In Table 8, I report correlations between subjects’ decisions in the treatment condition (relative to the Bayesian benchmark) and the above-mentioned measures. The absolute deviations from Bayesian updating are correlated with the habitual use of reappraisal. The coefficient value of -0.18 indicates a weak, negative correlation significant at the 0.05 level.

Table 8: Deviations from rationality and agents’ characteristics in the treatment condition.

	DevB	Extr	Cons	Open	Neur	Agre	Trait	State	Reapp	Supr
DevB	1.00									
Extr	0.00	1.00								
Cons	0.05	-0.01	1.00							
Open	-0.09	0.22*	0.10	1.00						
Neur	0.12	-0.24*	-0.26*	0.16*	1.00					
Agre	-0.03	0.07	0.07	0.07	-0.13	1.00				
Trait	-0.07	0.29*	0.35*	-0.09	-0.71*	0.23*	1.00			
State	-0.15	0.28*	0.17*	-0.03	-0.58*	0.24*	0.70*	1.00		
Reapp	-0.18*	0.09	0.15	0.18*	-0.17*	0.22*	0.13	0.17*	1.00	
Supr	-0.04	-0.19*	0.05	-0.17*	-0.04	0.03	-0.13	-0.14	0.38*	1.00

* $p < 0.05$

Note: “DevB” stands for deviations from Bayesian update. I use the labels: “Extr”, “Cons”, “Open”, “Neur”, and “Agre” for BIG-5 personality traits: extraversion, conscientiousness, openness to experience, agreeableness and neuroticism, respectively. I denote Anxiety trait and state with “Trait” and “State” (the two measures are defined such that a higher score indicates less anxious individual). “Reapp” and “Supr” stands for emotion regulation strategies: reappraisal and suppression.

In Table 9, I present the estimates of regressions based on decisions made by participants in the treatment condition. I regress the independent variable, the absolute deviations from Bayesian update, on the independent variable “Reappraisal” that measures subject’s habitual use of reappraisal. The coefficient at the Reappraisal variable is negative and significant at the 0.05 level. Reporting one point higher response on the 7-point Likert scale in questions about one’s habitual use of reappraisal leads to a 3-point decrease in the distance from Bayesian update. The value doesn’t change much if I control for subject’s rank, median belief or whether the signal he received was below or above his median belief or not within the prior belief distribution. The results show that subjects’ decisions correlate with the way they handle positive and negative emotions in their daily life. The more used they are to regulate their emotions by thinking differently about the situation they found themselves in, the more they adhere to rational decision-making. To investigate this further, I take a closer look at emotion regulation strategies together with self-reported emotions experienced before the task.

Table 9: The effect of reappraisal on deviations from Bayes.

	(1)	(2)
Reappraisal	-2.96** (1.29)	-2.82** (1.29)
Constant	26.61*** (5.76)	27.33*** (7.50)
Controls	No	Yes
Observations	160	160

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is absolute deviations from the Bayesian update. Controls include the subject’s rank and median prior belief, a dummy variable equal 1 if the signal was below or equal to the median prior belief, and a dummy variable equal 1 if the signal was outside of the subject’s prior beliefs.

5.2 Test-related Emotions

In addition to the data presented so far, I collected survey data about test-related emotions experienced by participants before receiving the signal.²³

Out of eight test-related emotions, anxiety and hopelessness significantly correlate with absolute deviations from Bayesian updating in the treatment condition. However, when I regressed absolute deviations from Bayesian updating on all test-related emotions, only hopelessness was highly statistically significant ($p\text{-value} = 0.02$) and remained so, even after adding additional controls on subjects' rank, median belief, and signal's value or its relation to the subject's beliefs.

Hopelessness was measured by agreement with the statement "I felt that I would rather not do this part because I've lost all hope.". As reported in the first column in

²³In the instructions displayed on the screen, I highlighted that questions refer to the particular moment in time: *after* learning the nature of the task, but *before* seeing the number.

Table 10: The effect of self-reported emotions on deviations from rationality.

	(1)	(2)	(3)
Hopelessness	4.31** (1.83)	4.30** (1.82)	17.23*** (4.62)
Reappraisal		-2.82** (1.42)	2.21 (2.16)
Hopelessness \times Reappraisal			-3.10*** (1.02)
Constant	10.00 (8.28)	20.18** (10.11)	2.73 (11.40)
Controls 1	Yes	Yes	Yes
Controls 2	No	Yes	Yes
Observations	160	160	160

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is absolute deviations from the Bayesian update. The independent variable "Hopelessness" was measured by the extent to which a subject agreed with the statement "I felt that I would rather not do this part because I've lost all hope.". "Reappraisal" refers to self-reported habitual use of reappraisal. Controls 1 include all other emotions reported by subjects; Controls 2 include the measure of habitual use of suppression.

Table 10, stating a 1-point higher answer to the question translates to an increase of 4.3 points in absolute deviation from Bayesian updating (controlling for all remaining test-related emotions). The coefficient at the Hopelessness variable remains unchanged if I control for the emotion regulation strategies: suppression and reappraisal (Specification 2) in Table 10. Of the two strategies, only reappraisal is different from zero and significant. Moreover, it has the expected negative sign and value similar to that reported in Table 5. I hypothesize that the use of reappraisal counteracts the negative impact of Hopelessness. To test this hypothesis, I add to the regression the interaction of Hopelessness and Reappraisal. I report the estimation results in the last column of Table 10. The coefficient at the interaction term is negative and highly significant, whereas the coefficient at the Reappraisal variable loses its significance. At the same time, the coefficient at Hopelessness increases fourfold and gains significance, suggesting that its impact is much larger without the offsetting effect of reappraisal.

While only suggestive, the evidence presented in this section supports the view that the treatment effect is stemming from the visceral, emotion-based reaction to signals. That reaction lies at the heart of what economists call “the belief-based utility” and is the driving force behind asymmetric updating.

6 Conclusions

In this paper, I propose a new test of the hypothesis that people interpret favorable feedback as more informative. To this end, I designed a simple experiment with two conditions. In the treatment condition, participants observe a signal about their intelligence and decide whether the signal is informative or not. In the control condition, participants make the same choice without receiving a factual signal: they are asked to specify their actions conditioning on possible signal realizations. This design allows me not only to pin down the causal effect of signal valence on updating but also to uncover the underlying mechanism. The experimental data reveal that people tend to interpret favorable signals as more informative due to the changes in belief-based utility. Participants reported a 10 percentage point higher probability of a positive signal being entirely informative about their rank after receiving it, compared to what they would

conclude ex-ante, without observing its realization. The results cast a new light on the origins of overconfidence, pointing towards the role of affect in asymmetric updating.

Moreover, we observe additional asymmetry in how subjects translate their beliefs about signal informativeness into beliefs about ability – participants who received “good” signals were more consistent with their previous reports. Even though signals significantly shifted subjects’ beliefs, they did it selectively, with “good” signals having a larger impact on final beliefs. As a result, the aggregate overconfidence level remained the same at the end of the experiment. Our study reveals the mechanisms that perpetuate overconfidence in the face of feedback: a combination of conservatism, asymmetric updating, and overconfident priors (the latter results in subjects assigning zero probability to the majority of “bad” signals they received) is driving the result.

There is mounting evidence that people derive utility not only from physical outcomes but also from their beliefs about the current or future state. The belief-based utility is likely to be a driving force behind overconfidence, the demand for (and avoidance of) information, and belief polarization. Yet, the way it influences people’s actions and beliefs is not fully understood. My study takes the next step toward explaining its role by revealing how belief-based utility shapes the way we interpret new information.

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A Differences between the treatment and the control group

Table 11: Differences between participants in the Treatment and the Control condition.

	Treatment	Control	p-value		
			H_0 : Diff < 0	Diff \neq 0	Diff > 0
IQ score	5.12 (0.30)	5.16 (0.50)	0.47	0.94	0.53
Rank	5.59 (0.21)	5.82 (0.39)	0.31	0.61	0.69
Bias	1.18 (0.22)	1.23 (0.43)	0.46	0.91	0.54
Absolute Bias	2.38 (0.14)	2.60 (0.28)	0.24	0.47	0.76
N	160	49			

Note: “Bias” is defined as difference between rank and median belief. Standard errors in parenthesis.

Table 12: Differences between Treatment and Control in individual prior belief distributions.

			p-value		
	Treatment	Control	H_0 : Diff < 0	Diff \neq 0	Diff > 0
Prior Beliefs:					
Mean	4.43 (0.14)	4.56 (0.26)	0.33	0.65	0.67
1 st Quartile	3.69 (0.13)	3.79 (0.27)	0.35	0.70	0.65
Median	4.41 (0.13)	4.58 (0.27)	0.28	0.56	0.72
3 rd Quartile	5.11 (0.15)	5.34 (0.27)	0.23	0.45	0.77
N	160	49			

Table 13: Deviations from Bayes in the main task (Treatment vs Control).

<i>Dependent variable: absolute difference between subjects' reports and the Bayesian benchmark.</i>	
	(1)
Treatment	-0.46 (1.72)
Constant	14.23*** (0.93)
Observations	650

Standard errors clustered at the participant level.

Their values in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the absolute difference between subjects' belief about the box and the Bayesian benchmark (in cases when subjects' assigned zero prior probability to the signal displayed on-screen the rational benchmark is assumed to be 0). I interpret the dependent variable as a measure of rationality demonstrated during the task. "Treatment" is an indicator variable taking value 1 if the subject was in the Treatment condition and 0 otherwise (the Control condition).

Table 14: Decision time and time spent on the main task, in seconds.

	Treatment	Control	Difference
Total time	268.08 (6.27)	228.56 (17.64)	39.52
Total time (corrected)	251.15 (5.34)	228.56 (17.64)	22.60
N	155	49	
Decision time	52.40 (2.78)	22.86 (1.13)	29.54
Decision time (matched data)	52.40 (2.78)	24.21 (0.57)	28.13
N	155	490	

Note: In the Treatment condition, the number of observations is 155, because we did not measure the response time in Pilot.

B Additional Results

B.1 Defining signal valence in absolute terms

Figure 13: The average deviation from the Bayesian update for signals above/below 5.

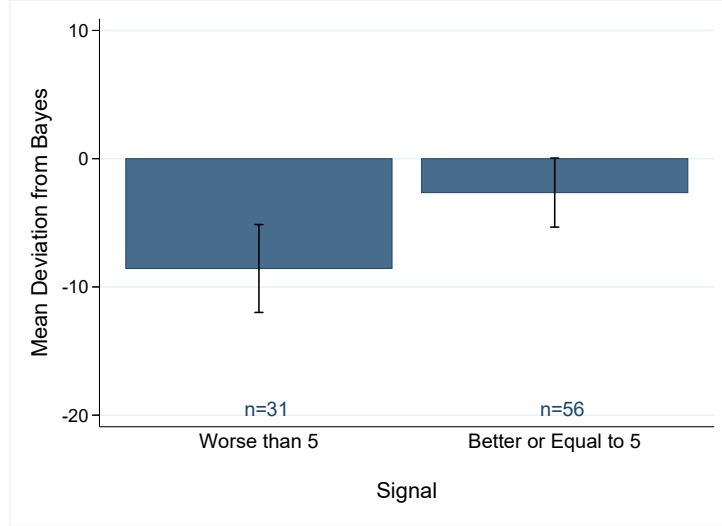


Table 15: The effect of the signal's valence (defined in absolute terms).

	(1)	(2)
Bayes	1.037*** (0.125)	1.048*** (0.125)
Good Signal	5.699 (4.511)	3.446 (4.965)
Rank		-1.038 (0.960)
Constant	-10.732 (8.189)	-4.456 (10.030)
N	87	87

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 in the treatment condition. "Bayes" is the number of points that should be allocated according to the Bayes' rule. The sample is restricted to the participants who received a signal to which they assigned non-zero probability. "Good Signal" indicator variable takes value 1 if the signal's value was equal to 1, 2, 3, 4 or 5 (good signals), and 0 otherwise.

B.2 Results based on the entire sample (including “outside priors”)

In this section, we replicate the results from Table 2 and Table 3 using the data from the entire sample. The results show that the coefficients at the “Good Signal” variable and the interaction term are very similar to the ones reported in the main text.

Table 16: The effect of the signal’s valence (only treatment condition).

	(1)	(2)	(3)	(4)	(5)
Bayes	0.713*** (0.051)	0.992*** (0.129)	0.956*** (0.130)	0.956*** (0.131)	0.967*** (0.131)
Good Signal	9.694*** (3.496)	8.958** (3.461)	13.629*** (4.558)	13.662*** (4.573)	12.320*** (4.616)
Outside Prior		20.239** (8.633)	22.315** (8.694)	22.041** (8.775)	23.072*** (8.745)
Outside Prior \times Good Signal			-10.893 (6.961)	-10.302 (7.289)	-9.745 (7.255)
Median Belief				-0.283 (1.004)	0.260 (1.050)
Rank					-1.079* (0.644)
Constant	9.231*** (2.515)	-9.241 (8.260)	-9.527 (8.224)	-8.217 (9.466)	-4.970 (9.609)
N	160	160	160	160	160

Standard errors in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 by participants in the treatment condition. “Bayes” denotes the number of points that should be allocated according to the Bayes’ rule (or zero if a subject assigned zero prior probability to the signal displayed on screen). “Good Signal” indicator variable takes value 1 if the signal was below or equal to the median of subject’s belief distribution, and 0 otherwise. “Outside Prior” indicator variable takes value 1 if a subject assigned zero prior probability to the rank corresponding to the signal he received.

Table 17: The effect of the signal's valence (both conditions).

	(1)	(2)	(3)	(4)	(5)
Bayes	0.695*** (0.039)	0.670*** (0.039)	0.670*** (0.039)	0.767*** (0.093)	0.765*** (0.093)
Treatment	4.547** (1.952)	4.841** (1.914)	1.616 (2.601)	1.012 (4.105)	1.015 (4.136)
Good Signal		5.0767** (2.140)	3.290 (2.587)	5.944* (3.362)	5.924* (3.361)
Treatment \times Good			7.357* (5.415)	9.474* (5.407)	9.875* (5.437)
Outside Prior				9.962* (5.890)	10.004* (5.951)
Controls 1	No	No	No	Yes	Yes
Controls 2	No	No	No	No	Yes
Constant	9.494 (1.291)	7.853 (1.464)	8.685 (1.640)	0.381 (5.651)	-1.477 (6.249)
N	650	650	650	650	650

Standard errors clustered at individual level. Their values in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 in the treatment condition. "Bayes" is the number of points that should be allocated according to the Bayes' rule. The sample is restricted to the participants who received (or considered) a signal to which they assigned non-zero probability. "Treatment" is a variable indicating assignment to the treatment condition. "Good Signal" indicator variable takes value 1 if the signal was below or equal to the median of subject's belief distribution, and 0 otherwise. "Outside Prior" is an indicator variable taking value 1 if a subject assigned a probability of zero to the signal. Controls 1 include interactions of the "Outside Prior" variable with "Treatment" and "Good Signal". Controls 2 include individual rank and median belief.

B.3 Results excluding participants who were guessing own rank

Table 18: The effect of the signal’s valence.

	(1)	(2)	(3)	(4)	(5)
Bayes	0.795*** (0.101)	0.735*** (0.104)	0.731*** (0.104)	0.732*** (0.104)	0.731*** (0.103)
Treatment	8.551** (3.669)	9.140** (3.508)	3.668 (5.379)	3.480 (5.392)	3.173 (5.458)
Good Signal		7.696** (3.021)	6.323* (3.338)	6.260* (3.363)	6.226* (3.341)
Treatment \times Good			9.799 (6.784)	10.005 (6.839)	10.019 (6.952)
Median Belief				0.351 (1.230)	0.131 (1.240)
Rank					0.629 (0.642)
Constant	2.311 (5.850)	1.230 (5.996)	2.299 (8.102)	0.700 (7.443)	-2.023 (8.526)
N	270	270	270	270	270

Standard errors clustered at individual level. Their values in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 in the treatment condition. “Bayes” is the number of points that should be allocated according to the Bayes’ rule. The sample is restricted to the participants who received (or considered) a signal to which they assigned non-zero probability. “Treatment” is a variable indicating assignment to the treatment condition. “Good Signal” indicator variable takes value 1 if the signal was below or equal to the median of subject’s belief distribution, and 0 otherwise.

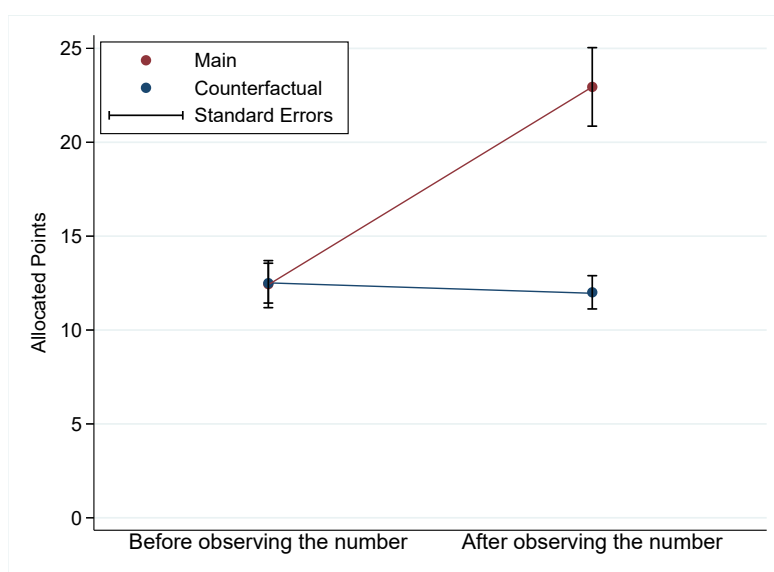
B.4 Manipulation Check

I argue that the treatment effect is caused by the utility from beliefs induced by the signal. First of all, I provide evidence that the signal received in the treatment condition affected subjects’ beliefs. In Figure 14, I present subjects’ beliefs before and after the main task, that is, beliefs revealed in the first and the second belief elicitation. The graph shows points allocated to the rank corresponding to the number displayed on subjects’ screens. I compare these values to the counterfactual: how many points they would allocate to the respective ranks if they did not receive a signal. There is no change in beliefs in the counterfactual scenario (denoted with a blue line). In the treatment

condition, the change in beliefs is significant (marked in red on the graph). Subjects allocated almost two times as many points in the second belief elicitation to the rank displayed on the screen.

Secondly, I exclude alternative hypotheses. One may worry that subjects in the control condition exerted less effort per decision (e.g. due to increasing marginal cost of effort or lower monetary incentives in the control condition). To alleviate this concern, we asked participants in the treatment condition, before they received an actual signal, to consider about every possible signal realization. We showed them, one by one, every possible number and asked them to think what they would do if this number was drawn later. This additional part makes the total time spent on the second task similar in both conditions (see Table 14 in Appendix A). One may argue that the total time spent on the task may not be a perfect measure of effort and there still may be differences in cognitive effort exerted when making a decision in the treatment and in the control condition. However, if this was the case, one would expect larger deviations from the rational benchmark in the control condition. As reported in Table 13 in Appendix A, there is no significant difference in absolute deviations from the Bayesian benchmark in the two conditions. I provide additional evidence to support my interpretation of the results as being driven by changes in belief-based utility in Section 5.

Figure 14: Beliefs before and after the signal.



C Literature: Design Comparison

The experiment developed for this paper differs from designs used in the literature in several ways. First of all, the new control condition addresses the problem of causal identification of the effect of signal valence, as described in the main body of the paper. Secondly, also the treatment condition diverges from the paradigm commonly used in experimental studies on belief formation. Guided by the hypothesis that it is the belief-based utility what drives the updating about ego-relevant characteristics, I aimed at designing an updating task that induces a strong emotional reaction to the signal. In order to clarify the differences between my design and experiments conducted in the past, I gathered and described dissimilar features of the design in Table 19.

While there are many papers studying overconfidence and asymmetric updating, in this review, I focus on papers that study updating about ego-relevant characteristics and do so by asking subjects to update their beliefs about their *relative* performance. For a review of the beliefs updating literature that includes updating about absolute performance as well as updating about non-ego-relevant parameters, I refer the reader to the recent works of Barron (2021) and Coutts (2019). An even broader review of the literature on errors in probabilistic reasoning could be found in Benjamin (2019).

The papers gathered in the first column in Table 19 are categorized based on various design features. In the second column, I describe the corresponding design feature used in my experiment. The last column presents the rationale behind choosing this particular feature for my work. One important design feature that requires an additional comment is the information structure. In almost all of the work reviewed in this section, the information structure follows the scheme presented in Figure 15.²⁴ There are two states of the world H and L indicating whether one's score was in the upper or the lower half of the test score distribution, and each subject receives a signal that is informative about the state with known precision, e.g. 75%, as shown in Figure 15. However, this signal structure becomes more complicated if extended to a larger signal and state space (see

²⁴See Table 19 for the references. Two papers that deviate from this signal structure are Eil and Rao (2011) and Zimmermann (2020) who introduce 10 states of the world and binary signals. A signal informs a subject whether or not he ranked higher than another participant who was randomly drawn from a group of 10 (see Figure 17; I denote the signals with H and L). The signal precision depends on the state and, for the first signal, can take one of the values: 55.6%, 66.7%, 77.8%, 88.9% or 100% (for the second signal it is 50%, 62.5%, 75%, 87.5% or 100%, as comparisons are made without replacement).

Figure 16) and I am not aware of any experimental work that implements it. The papers that used 10 states of the world in their design, Eil and Rao (2011) and Zimmermann (2020), use binary signals (see Figure 17).

Figure 15: Design used in the literature (2 states).

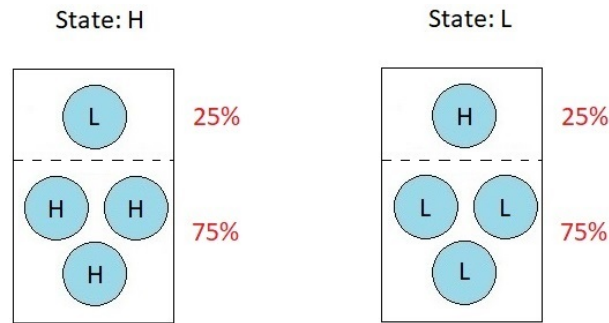


Figure 16: Design used in the literature extended to 10 states.

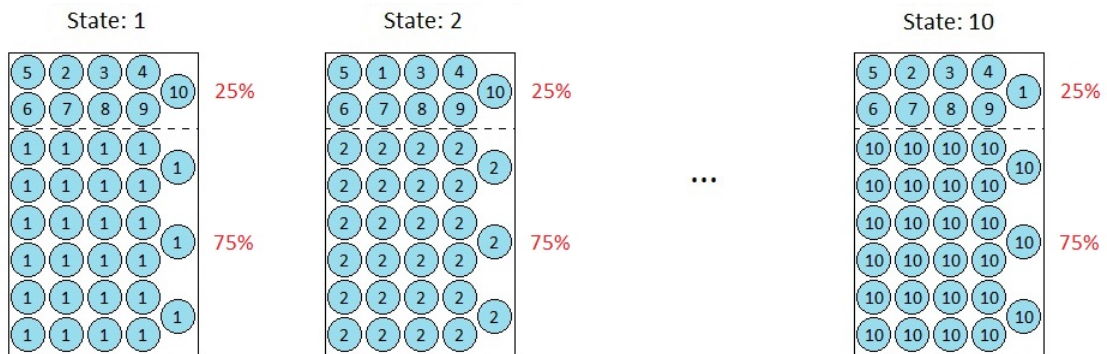
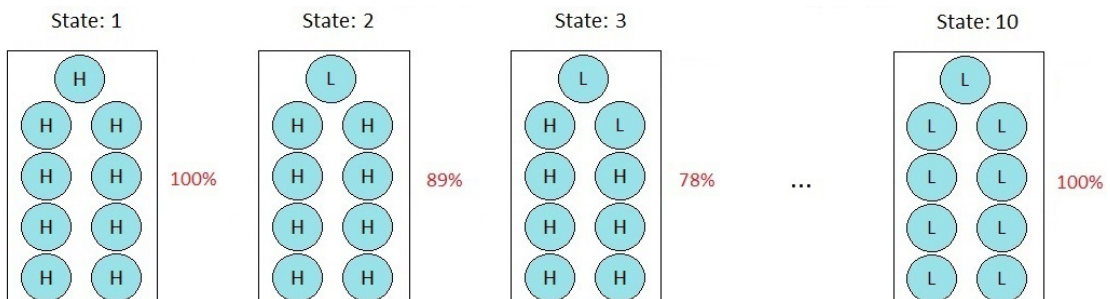


Figure 17: Design used in Eil and Rao (2011) and Zimmermann (2020).



The design used in the literature extended to 10 states (Figure 16) can be simplified by distinguishing two urns: one with balls indicating the state (“IQ” urn), and the other with every possible number (“Random” urn).²⁵ This is presented on Figure 18 and Figure 19 that illustrate the cases of 2 and 10 states of the world, respectively. Note that the information structure introduced in Figure 18 *is equivalent* to the one used in the literature that we depicted on Figure 15, if the IQ urn and the Random urn are being selected with equal probability. If the state is H , a ball indicating H is drawn with probability $0.5 \cdot 0.5 + 0.5 \cdot 1 = 0.75$, exactly the same as in Figure 15. Similarly, Figure 19 is equivalent to the information structure in Figure 16 with the signal precision of 55%.

Figure 18: Design developed in this paper (2 states).

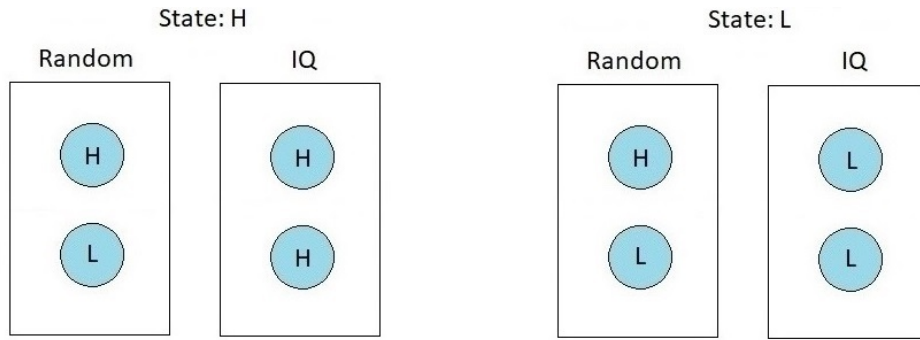
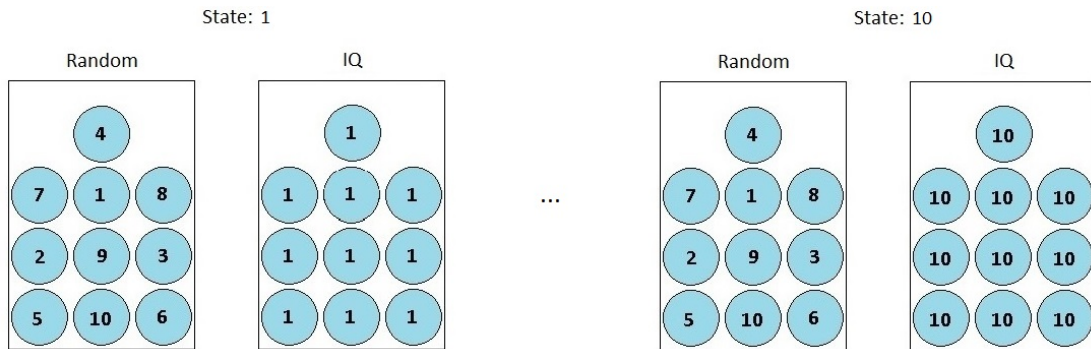


Figure 19: Design developed in this paper (10 states).



²⁵One could also distinguish the two urns along the dashed line in Figure 16, with the Random urn containing all numbers except the one that indicates the state. This design, however, lacks the intuitive interpretation of “a random urn” from which *any number* can be drawn with *the same* probability, hence it might be more difficult to explain to the participants.

Table 19: Literature review: design comparison.

Other Work	This Paper	Purpose
1. Number of signals:		
<ul style="list-style-type: none">– more than 1 signal <p>Buser et al., 2018; Coutts, 2019; Drobner and Goerg, 2021; Eil and Rao, 2011; Möbius et al., 2014; Zimmermann, 2020.</p>	<ul style="list-style-type: none">– 1 signal	<ul style="list-style-type: none">– separating reaction to signals from information aggregation.
<ul style="list-style-type: none">– 1 signal <p>Drobner, 2021; Ertac, 2011; Schwardmann and Van der Weele, 2019.</p>		
2. State space, signal space, signal precision:		
<ul style="list-style-type: none">– 2 states (above or below 50%; above or below 85% in Coutts, 2019),– 2 signal values,– signal precision: 67% <p>Coutts, 2019; Drobner, 2021; Drobner and Goerg, 2021.</p>	<ul style="list-style-type: none">– 10 states (deciles of the distribution)– 10 signal values– a signal is either perfectly informative or entirely uninformative (with equal probability).	<ul style="list-style-type: none">– richer state space and signal space to induce a stronger emotional reaction to a signal (based on the observation that it is more painful for subjects to be in the bottom 10% than in the bottom 50%).– by introducing signals that are perfectly informative or entirely uninformative (with equal probability), we reduce the compression effect described by Ambuehl and Li (2018).
<ul style="list-style-type: none">– 2 states (above or below 50%)– 2 signal values– signal precision: 70% <p>Buser et al., 2018.</p>		
<ul style="list-style-type: none">– 2 states (above or below 50%)– 2 signal values– signal precision: 75% <p>Möbius et al., 2014; Schwardmann and Van der Weele, 2019.</p>		
<ul style="list-style-type: none">– 3 states (lower 20%, middle 60%, or upper 20%)– 2 signal values– perfectly informative but coarse signals <p>Ertac, 2011.</p>		
<ul style="list-style-type: none">– 10 states (deciles of the distribution)– 2 signal values– signal precision depends on the state: 56%, 67%, 78%, 89% or 100%. <p>Eil and Rao, 2011; Zimmermann, 2020.</p>		

Other Work	This Paper	Purpose
3. Information structure and implementation:		
<ul style="list-style-type: none">– information structure as in Figure 15– a signal is true or false with precision known to the subjects <p>Buser et al., 2018; Coutts, 2019; Drobner and Goerg, 2021; Möbius et al., 2014; Schwardmann and Van der Weele, 2019. Drobner, 2021, uses the same information structure (Figure 15), but the signal is a comparison with another subject.</p>	<ul style="list-style-type: none">– information structure as in Figure 18. <p>It is equivalent to the information structure from Figure 16 with the signal precision of 55%.</p>	<ul style="list-style-type: none">– it would not be possible to introduce richer state and signal space using any other information structure from the literature.
<ul style="list-style-type: none">– information structure as in Figure 17– a signal is a pairwise comparison with another subject <p>Eil and Rao, 2011; Zimmermann, 2020.</p>		
<ul style="list-style-type: none">– a signal is always true, but only reveals whether the subject is in the top or the bottom half of the distribution, and not precisely the state <p>Ertac, 2011.</p>		
4. Comparison group:		
<ul style="list-style-type: none">– a group of 4 <p>Drobner, 2021; Schwardmann and Van der Weele, 2019.</p>	<ul style="list-style-type: none">– 300 other participants	<ul style="list-style-type: none">– a larger comparison group makes it more difficult to use reappraisal to lessen the impact of the negative signal (e.g. in the case of a group of four, one can easily attribute a negative signal to being assigned to a particularly strong pair of subjects). When there is another way of “explaining” a bad signal, there may be no need for (costly) belief distortion.
<ul style="list-style-type: none">– a group of 8 <p>Buser et al., 2018.</p>		
<ul style="list-style-type: none">– a group of 10 <p>Eil and Rao, 2011; Ertac, 2011; Zimmermann, 2020.</p>		
<ul style="list-style-type: none">– a group larger than 10 <p>Coutts, 2019; Drobner and Goerg, 2021; Möbius et al., 2014.</p>		
5. Timing of revealing information:		
<ul style="list-style-type: none">– In most of the papers mentioned above it is unclear whether and when subjects expected the resolution of uncertainty (see Drobner, 2021, for a comprehensive literature review). This problem was noticed and tested in a contemporaneous work of Drobner (2021).	<ul style="list-style-type: none">– available online, one week after the session	<ul style="list-style-type: none">– to describe the behavior with a one-period model without dynamic concerns– to bring the design closer to the real-world situations: grades are rarely immediate, need to be checked etc.

D Matching

Note that the research question does not refer to the treatment effect itself, but rather the heterogeneity in the treatment effects. Although the assignment into the treatment and control condition is random, the assignment of signals to agents is not. Imagine a subject who believes his rank is 1. In the control condition, he would consider all ten numbers, and 9 out of 10 decisions would pertain to an unfavorable signal. On the other hand, if he was in the treatment condition and his rank was indeed 1, he would receive a bad signal with much lower probability: $\frac{1}{2} \times \frac{9}{10}$. This leads to the covariance between the treatment status and signals considered by the participants. If there are reasons to believe that people with different beliefs or ranks respond differently to good signals, a simple comparison of means would not recover the treatment effect. I present this argument formally in Appendix D.4. Moreover, the mapping from prior belief distribution and rank to belief about the box is likely to be non-linear. As a consequence, the OLS estimates may not be efficient.

For these reasons, I use a different approach to analyze the data. I follow Heckman et al. (1998) and construct a matching estimator:

$$\hat{Y}_i^N = \sum_{j=1}^J w_j^i Y_j^C, \quad (1)$$

where \hat{Y}_i^N denotes beliefs of subject i from the treatment condition if he had not received the signal (the counterfactual outcome), Y_j^C denotes beliefs of subject j in the control condition (it includes a correction for potential bias as in Abadie and Imbens, 2011), $j = \{1, \dots, J\}$, and w_j^i is the weight assigned to j in the counterfactual outcome of subject i . The weights are normalized such that $0 \leq w_j \leq 1$ and $\sum_{j=1}^J w_j = 1$. I estimate the weights using a kernel regression for each participant in the treatment condition. I describe the estimation procedure in detail in Appendix D.5.

Intuitively, I construct the counterfactual to the participant i in the following way: I take the decisions of *all* participants in the control condition regarding the number that the participant i saw. However, not all observations in the control condition receive the same weight. Those participants whose true ranks and prior beliefs were closer to

that of the participant i , receive a higher weight.²⁶ I interpret the counterfactual as what would subject i decide if he was in the control condition. Having constructed this counterfactual scenario, I look at the effect of a “good” signal on updating using regression analysis.

D.1 Regressions Analysis

The results of regression analysis using counterfactual outcomes are reported in Table 20. The dependent variable is the difference in points allocated to Box 2 (indicative of one’s rank) in the treatment and the counterfactual scenario. I interpret it as the difference in reported probabilities that a signal is entirely informative about one’s rank after receiving it, compared to what they would conclude if they considered the same signal in the control condition.

In the first specification, I regress this difference on a constant. The coefficient is significant and is equal to 4.95, a value similar to the one obtained in the regression based on all observations from the control condition. Subjects reported around 5 percentage points higher probability that the signal is their rank after receiving it. However, the effect is entirely driven by the response to “good” signals. In the second specification, I add an indicator variable “Good Signal” which takes the value of 1 if a signal received was lower or equal to the subject’s median belief. The coefficient at the Good Signal is positive and significant. After receiving a “good” signal, participants tend to put 10.55 higher probability on the signal being their rank in the treatment condition compared to what they would decide ex ante. There is no significant difference after “bad” signals.

In the third specification, I control for signals that were outside of subjects’ prior belief distribution. I add a dummy variable “Outside Priors” taking the value of 1 if a subject assigned a prior probability of zero to the signal displayed on his screen. However, a signal outside of priors can also be good or bad and can have a different effect depending on its valence. In the last specification, I add an interaction term of the Good Signal and the Outside Prior variable. The estimated effect is negative and

²⁶In the baseline specification, I match subjects using their true rank and prior beliefs. In Appendix D.7, I report the results based on two alternative specifications: in Specification 2, I match participants based on their true rank and prior beliefs about the number under consideration, and in Specification 3, I use only the distribution of prior beliefs (in theory, it subsumes information that a subject has about his performance). The results are very similar to the baseline specification.

Table 20: The effect of the signal’s valence.

	(1)	(2)	(3)	(4)
Good Signal		10.55*** (3.87)	7.72* (3.94)	15.68*** (5.05)
Outside Priors			-10.59*** (3.92)	-2.91 (4.96)
Outside Priors \times Good				-19.40** (7.89)
Constant	4.95** (1.96)	0.40 (2.54)	6.45* (3.35)	2.06 (3.75)
Observations	160	160	160	160

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the difference between numbers of points allocated to Box 2 in the treatment and in the counterfactual (kernel-based matching). “Good Signal” indicator variable takes value 1 if the signal was below or equal to the median of subject’s belief distribution, and 0 otherwise. “Outside priors” indicator variable takes value 1 if the subject attached a zero prior probability to the signal being his rank, and 0 otherwise.

counteracts the positive effect of the Good Signal variable. The coefficient at the Good Signal variable is higher compared to previous estimates. If subjects assigned a non-zero prior probability to the “good” signal displayed on their computer screen, they reported a 15 pp higher probability that the signal was their rank in the treatment condition compared to what they would decide in the control condition.²⁷ If the signal was outside the subject’s prior belief distribution, the effect is entirely reduced.²⁸ There is no significant difference in the case of “bad” signals.

The standard errors reported in Table 20 do not account for the matching procedure. There is additional uncertainty coming from that we do not know how well our

²⁷The result is higher than the one from the regression analysis. In Appendix E, I conduct a regression analysis using additional 60 observations from another experiment. In this study, we used the same instructions to elicit prior beliefs and hypothetical choices, however, before the later subjects filled-in multiple price lists to assess their WTP for signals. Since the two experiments were not ideantical, I do not add the observations to the sample. Nevertheless, I believe it is still worthwhile to have a look at the combined dataset. The estimated coefficients are very close to the matching.

²⁸The results suggest that the good-news effect is not universal across signals. Getting a signal “too good to be true” makes the agent skeptical and leads him to assign a lower probability than he would in the control condition. It does not contradict the theory, as for the subject to experience the belief-based utility it is necessary that the signal affects his beliefs, and it may not be the case if the signal is outside of the subject’s priors.

control group reproduce the counterfactual outcome. Abadie and Imbens (2006) derive analytical formulas for a consistent estimator for the large-sample variance of the nearest-neighbor matching estimator. However, large-sample techniques may not be well suited when the number of units in the comparison group is small (Abadie et al., 2010).²⁹ For this reason, I employ an inferential technique proposed in Abadie et al. (2010) that I describe in the following section.

D.2 Placebo Studies

For a robust inference in a finite sample, Abadie et al. (2010) propose an inferential technique based on “placebo studies”. The idea behind it is to compare the actual treatment effect to the distribution of so-called “placebo” treatment effects. The latter is calculated by assigning the treatment status to a random sample of all observations, conducting the same analysis and storing the estimated coefficients. I provide the details of the procedure in Appendix D.6. I focus on Specification 4) from the previous section.

Figures 20 and 21 summarize the results of the placebo studies. In Figure 20, I present a histogram of coefficients at the Good Signal variable. Figure 21 shows the coefficients at the interaction of the Good Signal and the Outside Prior variable. The vertical lines denote the actual treatment effects. One can notice that their magnitudes are extreme relative to the distributions of coefficients in the placebo studies, indicating the statistical significance of the actual treatment effects. The empirical distribution of the placebo effects allows me to calculate the p-value of a two-sided test to assess the statistical significance of the actual treatment effect. Formally, I test a hypothesis that there is no difference between the actual treatment effect and the placebo treatment effect. The corresponding p-values are 0.003 in the case of the Good Signal variable and 0.039 for the interaction term. I conclude that both effects are statistically significant.

²⁹Although the sample size of 209 subjects would not be considered small for an experimental economist, it is a small sample given our set-up. If we divide participants based on their prior belief distribution, observed signal, and its relation to the subject’s priors, we end up with much smaller comparison groups.

Figure 20: Distribution of coefficients at the Good Signal variable (Specification 4).

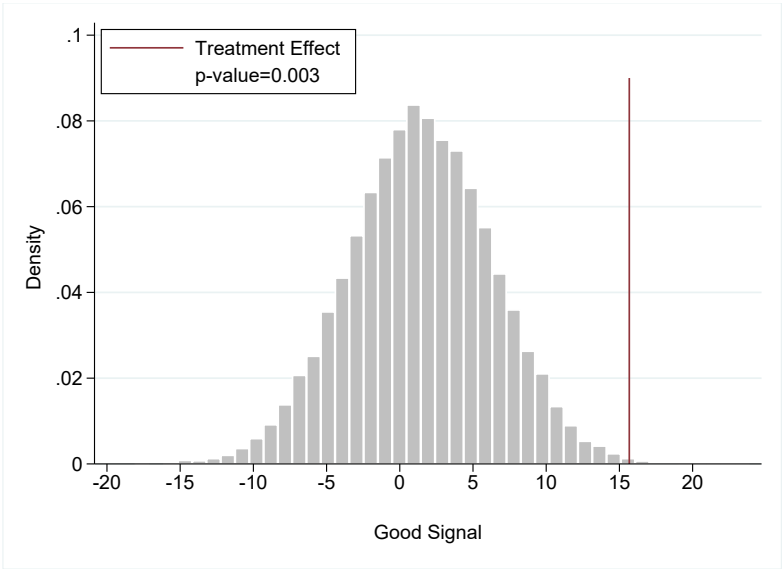
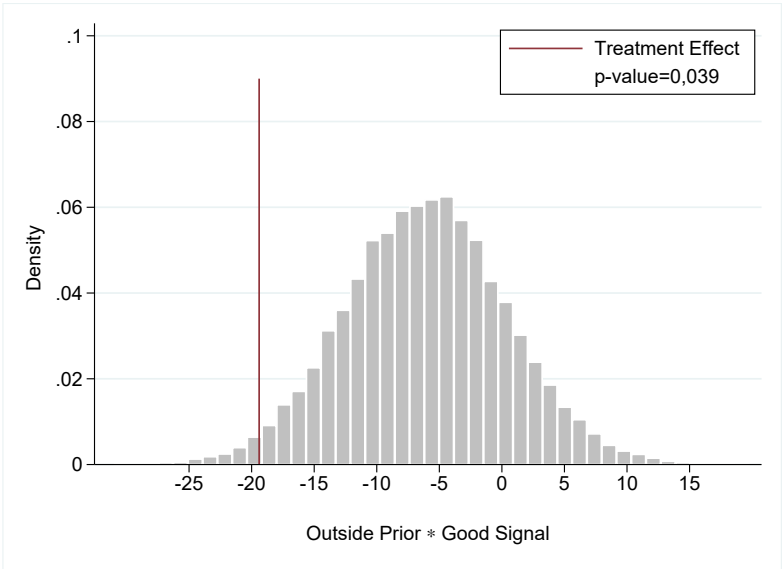


Figure 21: Distribution of coefficients at the interaction term (Specification 4).



D.3 Estimation Procedure

In this section, I provide more details on the estimation procedure. I begin by motivating the use of a matching estimator and contrasting it with a linear regression. Next, I describe the procedure that I use to determine the parameters of the matching estimator. Finally, I discuss the inference.

I adopt the standard model of treatment effects (Abadie and Imbens, 2006; Heckman et al., 1998; Rosenbaum and Rubin, 1983). Let $Y_i(W_i)$ denote the outcome of interest: the number of points that a participant i allocated to Box 2. W_i is a binary variable indicating whether the subject was assigned to the treatment ($W_i = 1$) or the control condition ($W_i = 0$). The average treatment effect is defined as

$$\tau = \mathbb{E}[Y_i(1) - Y_i(0)]. \quad (2)$$

Let subjects' decisions be described by an additive model

$$\begin{aligned} Y_i(1) &= \mu_1(\mathbf{X}_i) + \varepsilon_1 \\ Y_i(0) &= \mu_0(\mathbf{X}_i) + \varepsilon_0, \quad \varepsilon_1, \varepsilon_0 \sim i.i.d \end{aligned} \quad (3)$$

where μ_1 and μ_0 are unknown functions of a k -dimensional vector of individual characteristics \mathbf{X} .

Because of a random assignment to the two conditions, the sample average of outcomes recorded in the control condition is an unbiased estimator of the counterfactual outcome $Y_i(0)$. Therefore, a consistent estimation of the treatment effect entails a simple comparison of mean outcomes in both groups of participants

$$\hat{\tau} = \frac{1}{|N_T|} \sum_{i \in N_T} Y_i - \frac{1}{|N_C|} \sum_{i \in N_C} Y_i, \quad (4)$$

where N_T and N_C denote the set of participants in the treatment and the control condition respectively, and $|A|$ is the cardinality of a set A .

However, I am interested in heterogeneous treatment effects, defined as

$$\tau(\mathbf{x}) = \mathbb{E}[Y_i(1) - Y_i(0) | \mathbf{X}_i = \mathbf{x}]. \quad (5)$$

In case of a random assignment, one can simply compare means

$$\tau(\mathbf{x}) = \mathbb{E}[Y_i | W_i = 1, \mathbf{X}_i = \mathbf{x}] - \mathbb{E}[Y_i | W_i = 0, \mathbf{X}_i = \mathbf{x}]. \quad (6)$$

Note that the conditional expectation satisfy $\mathbb{E}[Y_i | W_i = w, \mathbf{X}_i = \mathbf{x}] = \mu_w(\mathbf{x})$.

D.4 Potential Problem: Selection

Estimation of $\mu_w(\mathbf{x}) = \mathbb{E}[Y_i | W_i = w, \mathbf{X}_i = \mathbf{x}]$ turns out to be quite challenging. To illustrate why the OLS estimate may not be consistent, let's consider the task of estimating the treatment effect among participants who received a good signal. Consequently, X_i consists of a dummy variable equal to 1 for people who received a signal above or equal to their median belief, and 0 otherwise. The treatment effects are defined as

$$\tau(1) = \mathbb{E}[Y_i | W_i = 1, X_i = 1] - \mathbb{E}[Y_i | W_i = 0, X_i = 1]$$

and

$$\tau(0) = \mathbb{E}[Y_i | W_i = 1, X_i = 0] - \mathbb{E}[Y_i | W_i = 0, X_i = 0].$$

We are interested in the difference $\tau(1) - \tau(0)$.

Although the assignment to the treatment and control conditions is random, the experimental design may lead to the covariance between the treatment status and signals received by participants. This correlation may arise because participants in the treatment condition are presented one number, which is their rank with probability $\frac{1}{2}$. Therefore, underconfident agents will see a good signal more often than overconfident agents. In contrast, in the control conditions subjects see all 10 signals.

Formally, let U_i denote a binary variable indicating whether an agent is underconfident ($U_i = 1$) or not ($U_i = 0$). The measured treatment effect of a good signal can be

decomposed into

$$\begin{aligned} \underbrace{\hat{\mathbb{E}}[Y_i|W_i = 1, X_i = 1, U_i] - \hat{\mathbb{E}}[Y_i|W_i = 0, X_i = 1, U_i]}_{\text{observed difference between treatment and control}} &= \underbrace{\mathbb{E}[Y_i(1) - Y_i(0)|W_i = 1, X_i = 1, U_i]}_{\text{treatment effect}} \\ &+ \underbrace{\mathbb{E}[Y_i(0)|W_i = 1, X_i = 1, U_i] - \mathbb{E}[Y_i(0)|W_i = 0, X_i = 1, U_i]}_{\text{selection}} \end{aligned}$$

The selection arises if participants in the treatment and control conditions behave differently (on average) even absent any intervention. Let's decompose the selection term further. Let $Pr(X_i = 1|W_i = 1, U_i = 1)$ denote the probability that a subject i observes a good signal, while receiving the treatment and being underconfident. We can expand the selection term as follows

$$\begin{aligned} \mathbb{E}[Y_i(0)|W_i = 1, X_i = 1, U_i] &= \mathbb{E}[Y_i(0)|W_i = 1, X_i = 1, U_i = 1] Pr(X_i = 1|W_i = 1, U_i = 1) \\ &+ \mathbb{E}[Y_i(0)|W_i = 1, X_i = 1, U_i = 0] (1 - Pr(X_i = 1|W_i = 1, U_i = 1)) \end{aligned}$$

and

$$\begin{aligned} \mathbb{E}[Y_i(0)|W_i = 0, X_i = 1, U_i] &= \mathbb{E}[Y_i(0)|W_i = 0, X_i = 1, U_i = 1] Pr(X_i = 1|W_i = 0, U_i = 1) \\ &+ \mathbb{E}[Y_i(0)|W_i = 0, X_i = 1, U_i = 0] (1 - Pr(X_i = 1|W_i = 0, U_i = 1)) \end{aligned}$$

Due to the random assignment to the treatment and control, it follows that

$$\gamma_1 \equiv \mathbb{E}[Y_i(0)|W_i = 1, X_i = 1, U_i = 1] = \mathbb{E}[Y_i(0)|W_i = 0, X_i = 1, U_i = 1]$$

and

$$\gamma_0 \equiv \mathbb{E}[Y_i(0)|W_i = 1, X_i = 1, U_i = 0] = \mathbb{E}[Y_i(0)|W_i = 0, X_i = 1, U_i = 0].$$

In other words, conditional on X, U , the assignment is as good as random. This simplifies the selection term

$$\begin{aligned} & \mathbb{E}[Y_i(0)|W_i = 1, X_i = 1, U_i] - \mathbb{E}[Y_i(0)|W_i = 0, X_i = 1, U_i] \\ &= \gamma_1 Pr(X_i = 1|W_i = 1, U_i = 1) + \gamma_0 (1 - Pr(X_i = 1|W_i = 1, U_i = 1)) \\ & \quad - \gamma_1 Pr(X_i = 1|W_i = 0, U_i = 1) - \gamma_0 (1 - Pr(X_i = 1|W_i = 0, U_i = 1)) \end{aligned}$$

This means that the selection term is zero if and only if participants in the treatment condition are as likely to receive a good signal as participants in the control condition,

$$Pr(U_i = 1|W_i = 1, X_i = 1) = Pr(U_i = 1|W_i = 0, X_i = 1).$$

However, participants in the control condition see all signals, while participants in the treatment see their own rank with probability $\frac{1}{2}$. Although we make sure that the participants are randomly allocated to the two groups, we cannot ensure that the signals relative to prior beliefs are randomly allocated to participants.

D.5 Solution: Matching Estimator

To deal with the potential selection issues (as well as to deal with potentially complex non-linearity of $\mu_w(\mathbf{x})$), I follow Heckman et al. (1998) and construct a matching estimator. For every participant in the treatment condition, I construct a counterfactual outcome based on decisions of participants in the control conditions with similar characteristics. In the example described in the previous section, we would match the underconfident agent who saw a good signal in the treatment condition with participants in the control condition, who were also underconfident and considered the same signal.

Following Heckman et al. (1998), I use a kernel regression to estimate the counterfactual outcomes $Y_i(0)$ for every participant in the treatment condition. The key identification assumption is that, conditional on all observables included in the matching procedure, the assignment of signals to participants is as good as random.

Formally, the treatment effect can be written as

$$\hat{\tau}(\mathbf{x}) = |N_T|^{-1} \sum_{i \in N_T} \left(Y_i - \sum_{j \in N_C} w_j^i (Y_j + \hat{\mu}_1(\mathbf{X}_i) - \hat{\mu}_1(\mathbf{X}_j)) \right), \quad (7)$$

where for each participant i in the treatment condition, w_j^i is a weight that I assign to a subject j from the control condition. The more similar participants i and j are (in terms of characteristics in \mathbf{X}), the higher the weight w_j^i . The weights are normalized such that $\sum_j w_j^i = 1$ and $w_j^i > 0, \forall_{i,j}$. The correction term $\hat{\mu}_1(\mathbf{X}_i) - \hat{\mu}_1(\mathbf{X}_j)$ removes the potential asymptotic bias of the matching estimator, as suggested by Abadie and Imbens (2011), where $\hat{\mu}_w(\mathbf{X})$ is a consistent regression estimator of $\mu_w(\mathbf{X})$. The intuition behind the bias correction is as follows. For a good match, the distance between \mathbf{X}_i and \mathbf{X}_j is small and the correction term $\hat{\mu}_1(\mathbf{X}_i) - \hat{\mu}_1(\mathbf{X}_j)$ vanishes. At the same time, the bias correction provides insurance in case of an imprecise match. In this case, the decision that a subject i would have made in the control condition becomes

$$\hat{Y}_i(0) = \hat{\mathbb{E}}[Y_i | W_i = 1, \mathbf{X}_i] + \sum_{j \in N_C} w_j^i \left(Y_j - \hat{\mathbb{E}}[Y_i | W_i = 1, \mathbf{X}_j] \right).$$

That is, the counterfactual outcome is equal to the regression prediction augmented with the matching term. The latter makes the whole estimator robust to a potential misspecification of the regression function stemming from the non-linearity of $\mu_w(x)$.

The weights w_j^i were constructed using the Epanechnikov kernel $K_{\mathbf{h}}(x)$ with a vector of parameters $\mathbf{h} > 0$

$$K_{\mathbf{h}}(\|\mathbf{X}_i - \mathbf{X}_j\|) = \begin{cases} \frac{3}{4} \left(1 - (\|\mathbf{h}(\mathbf{X}_i - \mathbf{X}_j)\|)^2 \right) & \text{if } \|\mathbf{h}(\mathbf{X}_i - \mathbf{X}_j)\| \leq 1 \\ 0 & \text{otherwise,} \end{cases} \quad (8)$$

where the length of \mathbf{h} is equal to the number of characteristics included in \mathbf{X} .

Thereby, the weights are given by

$$w_j^i(\mathbf{h}) = \begin{cases} \frac{K_{\mathbf{h}}(\|\mathbf{X}_i - \mathbf{X}_j\|)}{\sum_{j \in \mathcal{J}_i} K_{\mathbf{h}}(\|\mathbf{X}_i - \mathbf{X}_j\|)} & \text{if } j \in \mathcal{J}_i \\ 0 & \text{otherwise,} \end{cases} \quad (9)$$

where $\mathcal{J}_i \subset N_C$ is the set of individuals in the control condition who considered the same number as subject i . Epanechnikov kernel has similar interpretation to the nearest-neighbor matching often employed in the applied literature. It assigns positive weights only to a compact subset of neighbors whose characteristics are the closest to those of the target point. However, in contrast to nearest-neighbor matching which gives all the points in the neighborhood equal weight, Epanechnikov kernel assigns weights that decline smoothly with distance from the target point. This ensures that the resulting approximation of the conditional expectations $\mu_w(\mathbf{x})$ is smooth.

Parameter vector \mathbf{h} controls the support of the kernel – how many closest neighbors to include and how quickly the weights decay with the distance. I follow a standard practice in the literature and estimate \mathbf{h} using leave-one-out cross validation on the sample of participants in the control condition (see e.g. Hastie et al., 2009, Chapter 6). For a given \mathbf{h} , I estimate, using kernel regression, the probability each individual $k \in N_C$ assigned to Box 2

$$\hat{Y}_k(\mathbf{h}) = \sum_{j \in N_C \setminus \{k\}} w_j^k(\mathbf{h}) (Y_j + \hat{\mu}_1(\mathbf{X}_i) - \hat{\mu}_1(\mathbf{X}_j)).$$

I choose \mathbf{h} to minimize the mean squared prediction error

$$|N_C|^{-1} \sum_{k \in N_C} \left(Y_k - \hat{Y}_k(\mathbf{h}) \right)^2.$$

As for the choice of observables \mathbf{X} , I match agents in the treatment to those in the control who make decision regarding *the same* signal. Given this initial selection, I consider three specifications. In the baseline specification, I match participants based on their rank and prior belief distribution.

In the second specification, I match participants based on their rank and prior beliefs with respect to the number displayed on the computer screen. For example, if a participant observed the number “3” displayed on his screen, he would be matched based on his rank and how many points he allocated to rank 3 in the prior beliefs elicitation. In the third specification, I use only the prior belief distribution.

I estimate a consistent regression estimate $\hat{\mu}_w(x)$ using the estimator proposed in Hahn (1998)

$$\hat{\mu}_1(\mathbf{x}) = \frac{\hat{E}[YW|X = \mathbf{x}]}{\hat{E}[W|X = \mathbf{x}]} \quad (10)$$

To obtain consistent estimators for the conditional expectations on the right-hand side of (10), I use an OLS for the nominator and a logit regression for the denominator.

D.6 Inference

For a robust inference in a small sample, I employ the inferential techniques proposed in Abadie et al. (2010). The idea behind these techniques is to test whether the estimated treatment effect is large relative to the distribution of so-called “placebo effects”. The placebo effect is estimated by assigning the treatment status to a random sample of all participants and conducting the same regression analysis.

To this end, I draw a random sample of 160 observations (i.e. equal to the number of subjects in the treatment condition) from all observations in the experiment and assign them the treatment status. I follow the matching procedure to create a counterfactual for each of those 160 observations. Next, I use the observations and their counterfactuals to estimate a placebo treatment effect. I run the same regression as those described in Table 20 and store the resulting coefficients.

I repeat this procedure 20 000 times to obtain a distribution of the placebo effects. Those coefficients are presented in the histograms in Figures 20 and 21. The empirical distribution of the placebo effects allows me to calculate the p-values of a two-sided test to assess the statistical significance of the actual treatment effect.

D.7 Different Specifications

In this section, I present the results of my analysis described in the previous section with the counterfactual based on different matching criteria. In Specification 2, I match participants based on their true rank and prior beliefs about the number under consideration (as opposed to the entire belief distribution in Specification 1). In Specification 3, I use only the prior belief distribution.

D.7.1 Matching Specification 2

Figure 22: Replication of Figure 9 with Panel b) based on Specification 2.

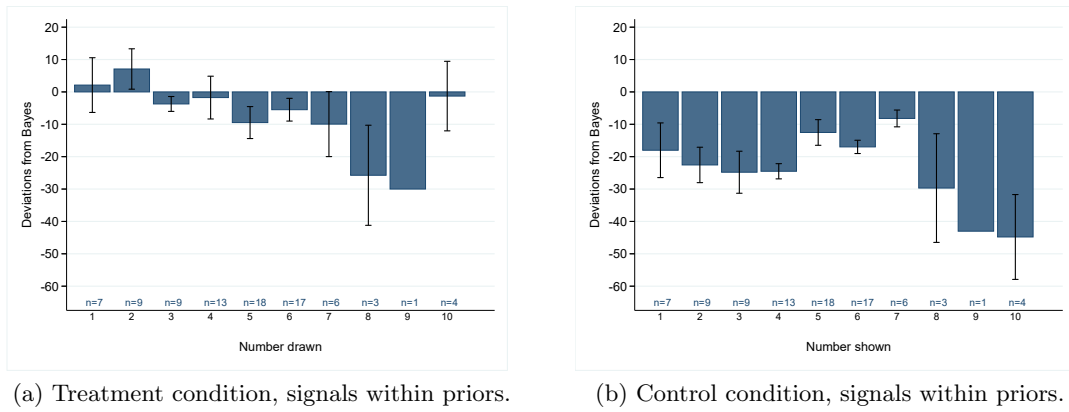


Table 21: The effect of the signal's valence.

	(1)	(2)	(3)	(4)
Good Signal		11.44*** (4.13)	7.38* (4.12)	14.69*** (5.31)
Outside Priors			-15.18*** (4.10)	-8.13 (5.22)
Outside Priors \times Good				-17.82** (8.29)
Constant	7.49*** (2.09)	2.56 (2.71)	11.24*** (3.51)	7.21* (3.94)
Observations	160	160	160	160

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the difference between numbers of points allocated to Box 2 in the treatment and in the counterfactual.

Figure 23: Distribution of coefficients at the Good Signal variable (Specification 4).

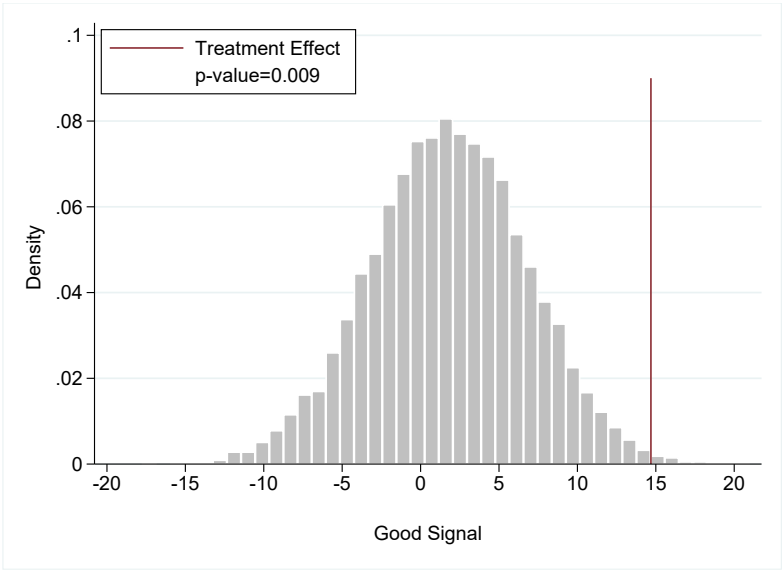
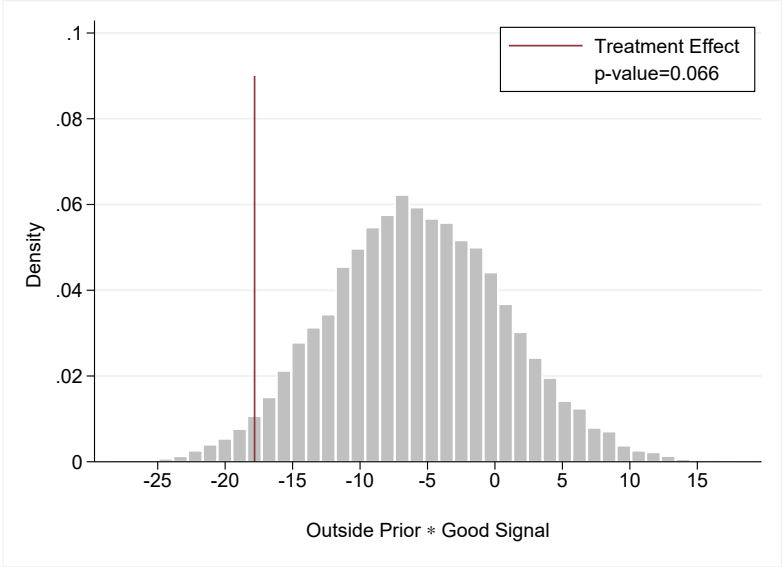


Figure 24: Distribution of coefficients at the interaction term (Specification 4).



D.7.2 Matching Specification 3

Figure 25: Replication of Figure 9 with Panel b) based on Specification 3.

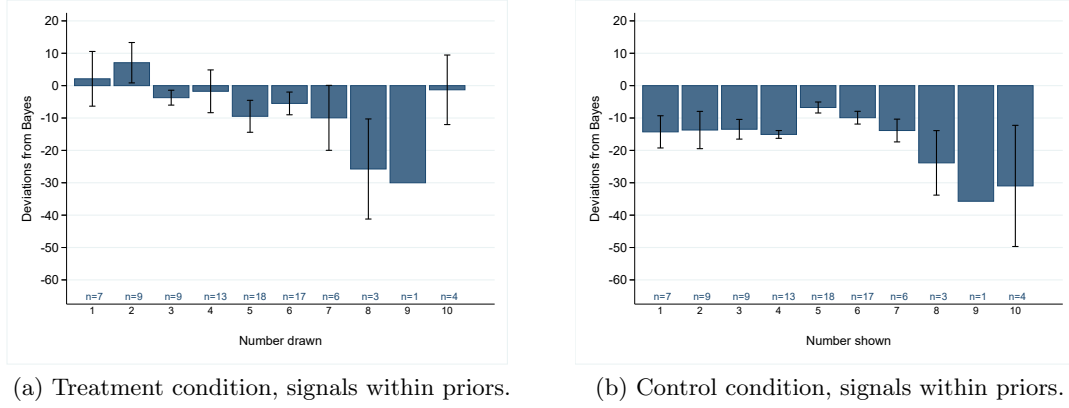


Table 22: The effect of the signal's valence.

	(1)	(2)	(3)	(4)
Good Signal		10.57*** (3.77)	8.88** (3.89)	15.88*** (5.01)
Outside Priors			-6.36 (3.87)	0.41 (4.92)
Outside Priors \times Good				-17.09** (7.82)
Constant	4.43** (1.91)	-0.13 (2.48)	3.51 (3.31)	-0.36 (3.72)
Observations	160	160	160	160

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the difference between numbers of points allocated to Box 2 in the treatment and in the counterfactual (kernel-based matching). "Good Signal" indicator variable takes value 1 if the signal was below or equal to the median of subject's belief distribution, and 0 otherwise. "Outside priors" indicator variable takes value 1 if the subject attached a zero prior probability to the signal being his rank, and 0 otherwise.

Figure 26: Distribution of coefficients at the Good Signal variable (Specification 4).

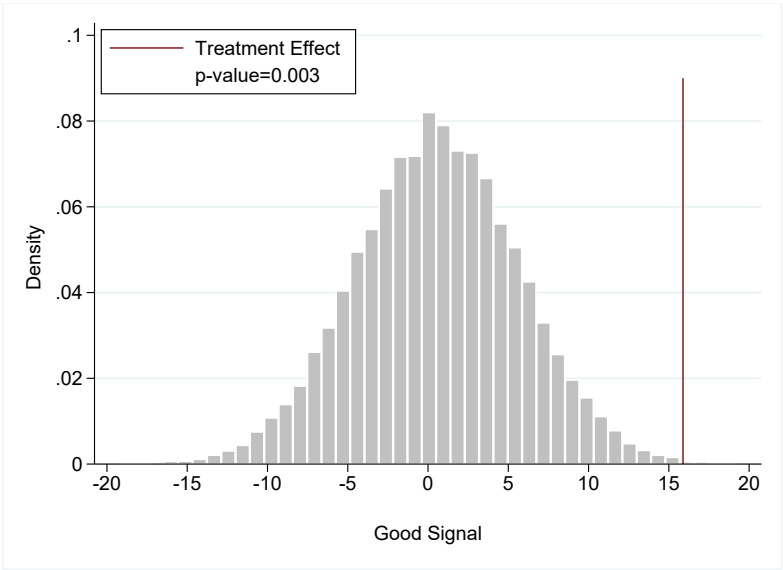
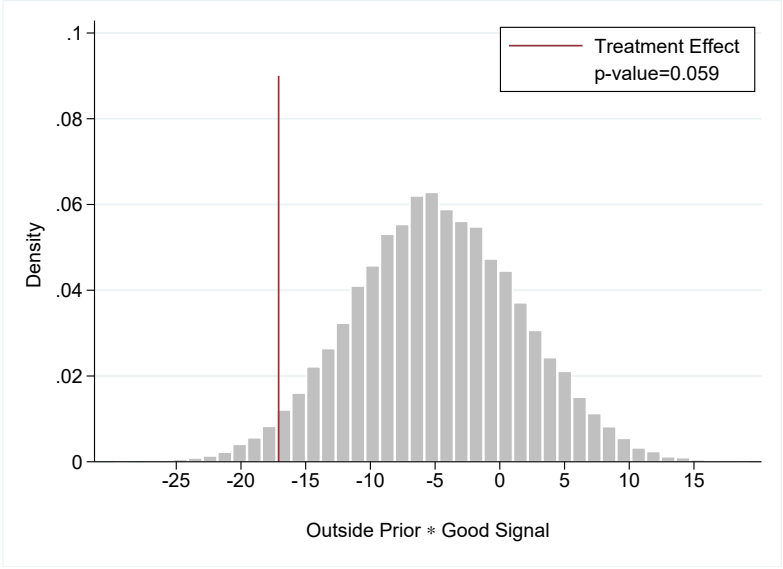
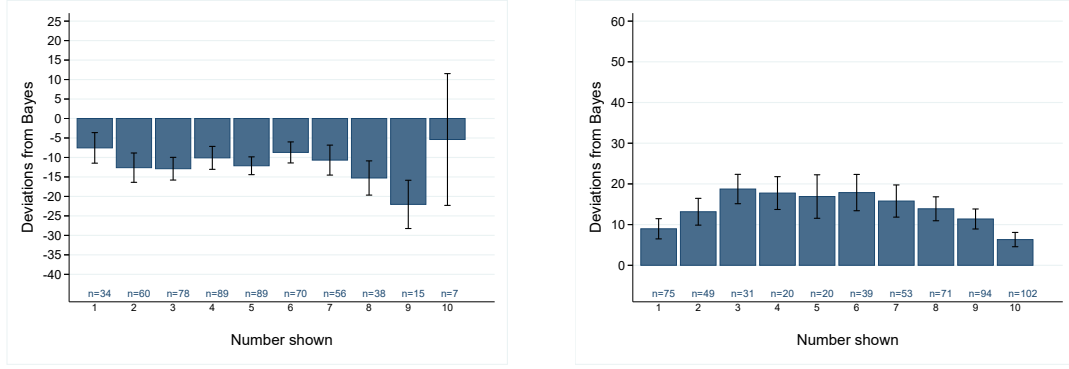


Figure 27: Distribution of coefficients at the interaction term (Specification 4).



E Results with Additional Data

Figure 28: Mean deviation from Bayes for different signals.



(a) Control condition, signals within priors.

(b) Control condition, signals outside priors.

Table 23: The effect of the signal's valence.

	(1)	(2)
Bayes	0.775*** (0.055)	0.777*** (0.055)
Treatment	-0.719 (4.293)	-0.777 (4.294)
Good Signal	0.483 (2.182)	0.560 (2.184)
Treatment \times Good	14.856*** (5.766)	15.115*** (5.775)
Rank		0.324 (0.383)
Constant	1.634 (3.515)	-0.334 (4.216)
N	623	623

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is the number of points allocated to Box 2 in the treatment condition. “Bayes” is the number of points that should be allocated according to the Bayes’ rule. The sample is restricted to the participants who received (or considered) a signal to which they assigned non-zero probability. “Treatment” is a variable indicating assignment to the treatment condition. “Good Signal” indicator variable takes value 1 if the signal was below or equal to the median of subject’s belief distribution, and 0 otherwise.

F Data Analysis: Payoffs

F.1 Payoffs from the Main Task

Table 24: Differences in payoffs (probability of receiving a large reward) in the main task.

	Treatment	Control	p-value		
			H_0 : Diff < 0	Diff \neq 0	Diff > 0
Payoffs (Signal = Rank)	53.51% (4.41)	45.11% (5.57)	0.882	0.237	0.118
N	73	49			
Payoffs (Signal \neq Rank)	75.69% (3.19)	81.84% (1.23)	0.024	0.048	0.976
N	87	441			

F.2 Payoffs from Belief Elicitation I and II

In this section, I describe subjects' payoffs from the first and the second belief elicitation as well as the payoffs subjects would have gotten if they had rationally updated their beliefs about rank. Firstly, let me compare the payoffs in the two conditions. The results are gathered in Table 25. While signals moved subjects' beliefs in the Treatment condition ensuring a larger payoff, there is no significant difference in payoffs in the Control condition (which should not come as a surprise, since participants in the Control condition did not receive any new information).

Table 25: Payoffs from Belief Elicitation I and II in the two conditions.

	Belief Elicitation I	Belief Elicitation II	p-value		
			H_0 : Diff < 0	Diff \neq 0	Diff > 0
Treatment	47.35% (1.32)	51.02% (1.72)	0.046	0.092	0.954
Control	45.97% (2.39)	48.41% (2.41)	0.236	0.471	0.764

Since only participants in Treatment received information that shifted their beliefs, let me focus only on these subjects. The difference in payoffs between the first and the second belief elicitation shows that even though the aggregate belief distribution seems to change little after the task (see Figure 10), the individual distributions changed in a way that guaranteed higher payoffs. Still, the payoffs would have been 2.86 percentage points higher (5.61% increase in relative terms), if participants had updated their beliefs rationally based on their prior belief distribution and the signal they received. The averages and the corresponding tests are gathered in Table 26. However, the first difference was calculated including participants who assigned a prior probability of zero to the signal displayed on their screens. For those subjects, the rational posterior is assumed to be zero, and it reduces the average difference. Indeed, for participants who assigned a non-zero prior probability to the signal displayed on-screen, the difference between the second belief elicitation and the rational benchmark is equal to 8.20 percentage points (14.69% increase in relative terms). It means that, for those participants, the payoff would have been almost 15% higher, if they had updated according to the Bayes rule.

Table 26: Payoffs from Belief Elicitation II and the rational update.

Payoff Elicitation II	Payoff if rational	Diff	H_0: Diff < 0	p-value	
				Diff \neq 0	Diff > 0
51.02% (1.72)	53.87% (2.37)	2.86%	0.97	0.06	0.03

Sample restricted to the subjects who assigned non-zero prior to the signal:

Payoff Elicitation II	Payoff if rational	Diff	H_0: Diff < 0	p-value	
				Diff \neq 0	Diff > 0
55.83% (2.37)	64.03% (2.31)	8.20%	1.00	0.00	0.00

Note: “Payoff if rational” refers to the payoff from Belief Elicitation II if subjects updated according to the Bayes’ rule based on their prior belief distributions and received signals. In the case of participants who assigned a prior probability of 0 to the signal, we assumed the rational posterior to be 0. “Restricted sample” includes only participants who received a signal to which they assigned a non-zero prior probability (87 subjects in the Treatment condition).

Those subjects would also be better-off if they formed a rational posterior using their beliefs about the box. In Table 27, I compare the payoffs from the second belief elicitation with the payoffs that participants would have gotten if they updated their beliefs about the rank in a way consistent with their decisions about the signal. The difference between payoffs in Belief Elicitation II and the Consistent Posterior is equal to 2.42 percentage points (4.74% increase in relative terms) and is significant at the 10% level. If we restrict the sample to the participants who assigned non-zero prior belief to the signal they received, the difference increases to 5.50 percentage points (9.85% increase in relative terms) and is significant at the 1% level.

Table 27: Payoffs from Belief Elicitation II and the counterfactual beliefs – if subjects were consistent with their inferences about the signal.

Payoff Elicitation II	Payoff if consistent	Diff	H_0: Diff < 0	p-value Diff \neq 0	Diff > 0
51.02% (1.72)	53.44% (2.31)	2.42%	0.948	0.103	0.051

Sample restricted to the subjects who assigned non-zero prior to the signal:

Payoff Elicitation II	Payoff if consistent	Diff	H_0: Diff < 0	p-value Diff \neq 0	Diff > 0
55.83% (2.37)	61.33% (3.70)	5.50%	0.993	0.013	0.007

Note: “Payoff if consistent” refers to the payoff from Belief Elicitation II if subjects formed beliefs consistent with their inference about the signal. “Restricted sample” includes only participants who received a signal to which they assigned a non-zero prior probability (87 subjects in the Treatment condition).

G Information Acquisition

In this section, I describe the data from the very last part of the study. As I already mentioned in the main text, we informed participants that they will not learn the test result on the day of the experiment. They could obtain this information only one week later by clicking on a website that was created for the experiment. Every participant was given a sealed envelope with a personal link inside.³⁰ Under this link, one week after their session, they could find their rank in the IQ test, as well as the details of their payment. This personal information was not accessible to other participants, as only the person who knew the link (part of which was the participant’s number and a four-digit code) could access it. The website was programmed in oTree and enabled us to collect information about participants who decided to check it.

Overall, 51% of all participants checked their links *even though* this part of the study was not incentivized (subjects did not get any money for it). There is no significant difference in information acquisition between the treatment and the control group (p-value = 0.962). While we cannot say for sure what motivated subjects to click or not (the reasons may range from simply losing the envelope to various motives described in the information avoidance literature, see Golman et al., 2017, for a literature review), we can check for individual traits that correlate with subjects’ choices.

The results of simple regression analysis are gathered in Table 28. The independent variable is an indicator variable taking value 1 if a subject decided to check the website. We observe that the lower the relative performance of a subject (the higher the rank) the lower the likelihood of checking the link. A person whose rank was Rank 1 will acquire information with 74% probability, while a subject ranked 10 – only with 29% chance. One possible explanation is that less cognitively able participants may be more likely to forget or lose the envelope, however, the next column in the regression shows that beliefs about the rank play a role. Participants with higher beliefs (lower perceived performance) tend to check the link less. An increase in the median belief by one rank translates to a 5 percentage point decrease in the probability of acquiring information.

³⁰Each envelope was placed in front of the subject, and its purpose was explained in the instructions. At the end of the experiment, research assistants reminded subjects not to forget the envelopes. The text inside informed subjects about the date and the type of information they can find under the link.

Table 28: Rank, beliefs and information acquisition.

	(1)	(2)	(3)	(4)	(5)
Median Belief			-0.05** (0.02)		
Bias				0.05** (0.02)	
Overconfident					0.15 (0.09)
Rank		-0.05*** (0.01)	-0.04*** (0.01)	-0.09*** (0.02)	-0.07*** (0.017)
Constant	0.51*** (0.03)	0.79*** (0.08)	0.95*** (0.10)	0.95*** (0.10)	0.80*** (0.08)
Observations	209	209	209	209	209

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is a dummy variable indicating whether or not a participant checked his IQ test result. The independent variable “Median Belief” refers to the rank that lies in the middle of the subject’s prior belief distribution. “Bias” denotes the difference between the subject’s actual rank and his median belief. It takes positive values for agents who overestimate their performance and negative values for those who underestimate it. The indicator variable “Overconfident” takes value 1 if the subject’s bias is larger than zero.

To further investigate the link between the subject’s rank, beliefs, and information acquisition, we look at the effect of the subject’s bias, which we define as a difference between one’s true rank and median belief (positive values indicate an overestimation of one’s relative performance). The coefficient at the Bias variable is positive and significant, revealing that the larger the bias the more likely a subject is to acquire information if his bias has a positive sign (he tends to overestimate his performance) and the less likely if it has a negative sign (he underestimates his performance). In other words, overconfident subjects tend to seek information, while underconfident participants shy away from it.³¹ We obtain a qualitatively similar result if we regress our dependent variable on an indicator variable “Overconfident” taking value 1 if the subject’s bias, as defined above, is larger than zero. Being overconfident is associated with a 15 percentage point higher probability of checking the link, controlling for individual rank. However, the coefficient misses the conventional threshold for statistical significance (p-value = 0.117).

³¹Here: ref. to the literature

Table 29: Received signal, beliefs and information acquisition.

	(1)	(2)	(3)	(4)	(5)	(6)
Signal Value	-0.026* (0.014)	-0.015 (0.014)	-0.014 (0.014)			
Good Signal				0.002 (0.080)	-0.030 (0.078)	0.032 (0.081)
Median Belief			-0.054** (0.022)			-0.058** (0.024)
Rank		-0.048*** (0.015)	-0.038** (0.015)		-0.052*** (0.014)	-0.040*** (0.015)
Constant	0.647 (0.087)	0.860*** (0.106)	1.035*** (0.128)	0.505*** (0.053)	0.813*** (0.099)	0.974*** (0.117)
Observations	160	160	160	160	160	160

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is a dummy variable indicating whether or not a participant checked his IQ test result. The independent variable “Signal Value” denotes the value of the signal received (the number displayed onscreen). Higher values indicate worse signals. The independent variable “Median Belief” refers to the rank that lies in the middle of the subject’s prior belief distribution. “Good Signal” is a dummy variable taking value 1 if a signal was better or equal to the subject’s median belief.

The relationship between the signal received and information acquisition is less clear, as presented in Table 29. While the value of the signal seems to be related to our variable of interest as expected (the higher the rank displayed on-screen the lower the probability of checking the link), the effect is only significant at the 10% level (p -value = 0.071) and it loses significance once we control for subject’s rank. There is also no significant effect of the signal valence, nor a positive or a negative surprise (defined as a difference between the signal and the median belief, not shown in the table). However, in all these cases, we can only analyze the behavior of participants in the treatment condition – those who received signals – reducing our sample size to 160. Any more complex relation between the subject’s rank, beliefs, received signal, and information acquisition is unlikely to be found in the collected dataset.

Last but not least, we look at correlations between information acquisition and personality traits, emotions experience during the task, and habitual use of emotion-regulation strategies. We report no significant correlation between any of the Big-5

Table 30: Personality traits and information acquisition.

	(1)	(2)	(3)
Extraversion	0.003 (0.01)	0.008 (0.01)	0.009 (0.01)
Conscientiousness	-0.015 (0.01)	-0.015 (0.01)	-0.012 (0.01)
Openness	-0.007 (0.01)	-0.010 (0.01)	-0.011 (0.01)
Neuroticism	-0.010 (0.01)	-0.008 (0.01)	-0.013 (0.01)
Agreeableness	0.019 (0.01)	0.012 (0.01)	0.014 (0.01)
Anxiety Trait			-0.005 (0.01)
Anxiety State			0.002 (0.01)
Rank		-0.049*** (0.01)	-0.048*** (0.01)
Constant	0.589* (0.32)	0.929*** (0.32)	1.089** (0.46)
Observations	209	209	209

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is a dummy variable indicating whether or not a participant checked his IQ test result.

personality traits nor STAI and information acquisition. In a regression including all personality and anxiety measures as independent variables, only Agreeableness comes close to being statistically significant (p -value = 0.107), with more agreeable individuals being more likely to check the link. However, its effect disappears if we control for the individual rank. In the second specification, we regress our variable of interest on the measures of achievement emotions and emotion regulation strategies, controlling for the subject's rank. Out of the eight achievement emotions, two are significantly correlated with information acquisition: anger and anxiety. Reporting a 1 point higher feeling of anger on a 7-point Likert scale is associated with a 4.9 percentage point lower probability of checking the link (p -value = 0.063). At the same time, reporting a 1 point higher

feeling of anxiety is related to a 9.6 percentage point higher probability of acquiring information (p-value = 0.040). Neither reappraisal nor suppression is correlated with information acquisition.

Table 31: Achievement emotions and information acquisition.

	(1)	(2)	(3)
Enjoyment	0.004 (0.02)	-0.005 (0.02)	-0.006 (0.02)
Hope	0.006 (0.03)	-0.001 (0.03)	-0.003 (0.03)
Pride	0.044 (0.03)	0.050 (0.03)	0.050 (0.03)
Relief	0.046 (0.03)	0.044 (0.03)	0.043 (0.03)
Anger	-0.043 (0.03)	-0.049* (0.03)	-0.047* (0.03)
Anxiety	0.089* (0.05)	0.096** (0.05)	0.095** (0.05)
Shame	-0.024 (0.03)	-0.004 (0.03)	-0.004 (0.03)
Hopelessness	0.019 (0.04)	0.006 (0.04)	0.007 (0.04)
Reappraisal			0.022 (0.03)
Supression			-0.020 (0.04)
Rank		-0.048*** (0.01)	-0.048*** (0.01)
Constant	0.154 (0.19)	0.457** (0.20)	0.448* (0.25)
Observations	209	209	209

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: The dependent variable is a dummy variable indicating whether or not a participant checked his IQ test result. The independent variables denote survey measures of the achievement emotions. “Reapp” and “Supres” denote the two emotion regulation strategies: reappraisal and supression.