Learning about One's Self *

Yves Le Yaouanq † Peter Schwardmann ‡

February 2020

Abstract

To better understand why naiveté about present bias is so prevalent and persistent, we investigate people's (in)ability to learn from their past behavior. Participants in our experiment repeatedly decide how much to work on an unpleasant task and are asked to predict their future effort. We find that participants are naively present biased at first, but update their beliefs once they gain experience with the task. Moreover, our methodology allows us to establish that the amount of updating we observe would eliminate naiveté in the long run. A treatment in which we vary the nature of the task after an initial experience shows that learning is unencumbered by a change in environment. Taken together, our results suggest that persistent naiveté results neither from a fundamental inferential bias nor from an inability to transport newly acquired self-knowledge to new settings. However, participants exhibit another bias: they underestimate their future learning, which may lead to underinvestment in experimentation and a failure to activate self-regulation mechanisms.

*This experiment was preregistered in the AEA RCT Registry (*AEARCTR-0003060*) and received IRB approval at the University of Munich (ID 2018-03). We thank Florian Englmaier, Ben Enke, Lena Greska, Taisuke Imai, Alex Imas, Simas Kucinskas, David Laibson, George Loewenstein, Michael Muehlegger, Takeshi Murooka, Matthew Rabin, Gautam Rao, Klaus Schmidt, Andreas Steinmayr, Séverine Toussaert, and seminar audiences at Harvard U, CMU, HU Berlin, LMU Munich, Erasmus University Rotterdam, the CESifo Conference on Behavioral Economics, the European Behavioral Economics Meeting in Bonn, the ECBE conference at UCSD, and the briq belief workshop for helpful comments. We gratefully acknowledge financial support from the *Deutsche Forschungsgemeinschaft* through CRC TRR 190.

†Department of Economics, University of Munich (LMU), Kaulbachstr. 45, D-80539 Munich, Germany; email: yves.leyaouanq@econ.lmu.de

‡Department of Economics, University of Munich (LMU), Ludwigstr. 28, D-80539 Munich, Germany; email: peter.schwardmann@econ.lmu.de

1 Introduction

When deciding how much to work next week, we tend to set ambitious goals for ourselves. But come next week, we often work less than we originally intended. Such time inconsistencies are especially harmful to someone who is naive about their existence. Whereas a sophisticated individual will commit to her desired course of action and avoid being exploited in markets, a naive individual bears the brunt of her present bias.[1] Naiveté about present bias has been documented in exercising (DellaVigna and Malmendier, 2006), tobacco consumption (Giné et al., 2010), saving behavior (John, 2018) and real-effort experiments (Augenblick and Rabin, 2019). Yet it is puzzling that naiveté should persist in repeated behaviors, which afford ample opportunity to learn. To resolve this puzzle we require a better understanding of how individuals draw or fail to draw lessons from their experience.

Our preregistered experiment investigates how participants update their beliefs about future effort based on a past effort choice. We develop a simple methodology that permits a non-parametric analysis of the updating process and the definition of useful benchmarks against which participants' learning biases can be detected. We are able to ascertain not just whether participants learn, but how well they learn, how much they expect to learn, and whether they are able to transport what they learn from one setting to another setting.

Over three weeks, 187 subjects participated in five experimental sessions. The first session took place in the experimental laboratory and allowed participants to familiarize themselves with the experimental tasks and the mechanism used to elicit beliefs. The following four sessions took place online and are labeled date 1, 2, 3 and 4. At dates 2 and 4 participants had the opportunity to complete a maximum of 40 computer screens of an unpleasant task, which either involved positioning sliders on pre-specified targets or counting zeroes in tables of ones and zeroes. At dates 1 and 3, on average five days before dates 2 and 4 respectively, we elicited participants' ex-ante preferences over and predictions of future effort. Ex-ante preferences were binding with a probability of 5 percent, in which case participants had to complete the stated number of screens at the next date or forgo any payment for the task. Subjects were paid a few days after the experiment, conditionally on participating in all sessions.

Our methodology leverages two pieces of data. The first primitive describes the on-the-spot effort choices at dates 2 and 4. We transform effort at a given date t into a binary variable a_t that equals 1 (*high effort*) if the subject completes at least 20 of

[1] See section 2.1 of DellaVigna (2009) and Laibson (2015) for the link between naiveté and a lack of demand for commitment, and Kőszegi (2014) for the exploitation of naive agents in markets.

40 screens and 0 (*low effort*) otherwise. The true data-generating process is given by the probability distribution $q(a_2, a_4)$ over the four possible intertemporal events $(a_2, a_4) \in \{0,1\}^2$ and can be constructed from the empirical frequencies of high and low effort. This primitive characterizes not only the marginal distributions of a_2 and a_4, which describe participants' probability of high effort at a given date, but also the informativeness of effort at date 2 for effort at date 4. We measure the *actual informativeness* of date 2 effort by the likelihood ratios $q(a_2 \mid a_4 = 1)/q(a_2 \mid a_4 = 0)$, defined for both $a_2 = 0$ and $a_2 = 1$. These likelihood ratios provide the answer to the question of how much more likely a participant is to observe a_2, if she is the type to exert high effort at date 4.

Analogously, the second primitive describes participants' probabilistic beliefs about their future effort. At date 1, we elicit prior belief distributions p_1, defined over the four possible future events $(a_2, a_4) \in \{0,1\}^2$. At date 3, after observing participants' realized effort a_2, we elicit their posterior beliefs p_3 about effort at date 4. The prior belief distribution captures both a participant's perceived probability of exerting high effort at a given date and the *anticipated informativeness* of date 2 effort, given by the subjective likelihood ratios $p_1(a_2 \mid a_4 = 1)/p_1(a_2 \mid a_4 = 0)$. Our experiment is purposefully simple and features only two work dates in order to allow for the meaningful elicitation of the complete prior belief distribution.

We find that subjects are present biased at both work dates. The fraction of subjects that commits to high effort at date 1 (date 3) is 16.4 (14.8) percentage points higher than the fraction of subjects that ends up exerting high effort on the spot. Moreover, effort choices at date 2 are highly informative about effort choices at date 4. Of those subjects that exert high effort at date 2, 73.3 percent exert high effort at date 4, whereas of those that exert low effort at date 2, only 9 percent exert high effort at date 4. The fact that subjects exhibit present bias and that their past behavior is highly informative about future behavior implies that there is both something to learn about and something to learn from.

As a first step in the analysis of learning we can compare beliefs about date 4 effort at date 1 (*prior*) and at date 3 (*posterior*). The average prior belief that effort will be high at date 4 is 61.4 percent, the average posterior belief is 53.7 percent, and the actual likelihood of high effort is 43.6 percent. Therefore, participants are initially naive and become less naive after having experienced the task.[2] However,

[2]Theoretically, apparent naiveté about self-control could be the result of sophisticated subjects stating high beliefs in an attempt to use the belief elicitation as a soft commitment to exert high effort. We test for this confound by, after beliefs have been elicited, randomly varying whether the belief elicitation is payoff-relevant. Payoff-relevance does not lead to higher effort, suggesting that belief statements are not used as commitment devices.

our goal is to ascertain not only whether subjects learn at all, but whether the average updating that we observe is quantitatively appropriate.

To this end, we use Bayes' rule to construct a benchmark, the *informed posterior beliefs*, that combines participants' average date 1 prior $p_1(a_4)$ with the actual informativeness of behavior $q(a_2 \mid a_4 = 1)/q(a_2 \mid a_4 = 0)$. We find that participants' elicited posteriors are *not* significantly different from these informed posteriors after both high and low effort at date 2, and we prove that this result implies that the population would become sophisticated in the long run, should participants perform the task infinitely often and maintain the same updating behavior. Therefore, we find no evidence for an inferential bias that would explain a persistent overestimation of future effort.

To investigate whether subjects expect to learn from their behavior, we construct another benchmark, the *anticipated posterior beliefs*, based on each subjects' unconditional prior $p_1(a_4)$ and anticipated informativeness of behavior $p_1(a_2 \mid a_4 = 1)/p_1(a_2 \mid a_4 = 0)$. We find that the anticipated posteriors are not very sensitive to date 2 effort choices. We call this bias the non-belief in the propensity to learn. The severity of the bias in our participants is reflected in the difference between the actual and anticipated improvement in predictions as we move from priors to posteriors. The former captures how much better a participant is able to predict her date 4 effort once she experienced date 2 and the latter her expectation of this improvement at date 1. We find that the actual improvement is 8 times higher than the anticipated improvement.

Finally, we introduce a treatment to investigate whether participants are able to transport what they learn from their behavior in one environment to a different environment. While all participants work on the sliders task at date 2, the treatment varies whether date 4 features the sliders or the counting zeroes task. We find that subjects learn equally well and underestimate their future learning to the same extent in the two conditions.

In our data, present-biased behavior may be caused by a lack of self-control (i.e. by time-inconsistent preferences) or by an initial underestimation of the effort costs associated with completing the task. Likewise, initial naiveté may be the result of subjects overestimating their self-control or underestimating their future effort costs. Our main analysis asks whether participants are good intuitive statisticians in revising their predictions based on their past behavior, but it does not, by itself, pin down the exact variable that subjects learn about. However, we present a number of additional results that show that subjects are learning about both their effort costs and their self-control.

This paper makes three contributions. First, we find that persistent naiveté is unlikely to be the result of either a fundamental bias in how people learn from their own behavior in simple settings[3] or of an inability to transport what they learn to different environments. Taken together with theoretical work that shows that optimism about self-control should be self-limiting in dynamic environments (Ali, 2011; Hestermann and Le Yaouanq, 2019), our results therefore greatly diminish the space of possible explanations for persistent naiveté. Two remaining candidates are the misattribution of failures in richer settings and the imperfect or biased recall of past behavior in settings that feature larger time lags.

Second, our paper is the first to not just study retrospective learning, but also the prospective learning implied by Bayes' rule. In doing so, we uncover a hitherto unexplored bias in participants' expectations: The non-belief in the propensity to learn. An agent subject to this bias will underinvest in experimentation. She may also fail to activate self-regulation mechanisms, emphasized by Ainslie (1975), whereby achieving self-control is facilitated by seeing each self-control choice as being correlated with many similar future choices.

Third, we propose a simple methodology for the analysis of learning from naturally occurring signals (e.g., past savings decisions, school grades, health or employment status, etc.). This may allow economists and psychologists to take the old question of whether people are good intuitive statisticians from the laboratory to the field, where any such intuition would have more plausibly developed. In its simplest form, our methodology imposes only two requirements on the collection of data. First, it requires that the researcher partitions the outcome space into two events and measures their occurrence at two points in time. Second, she needs to measure probabilistic beliefs over all future events before and after the first event has been observed.[4]

Our paper connects two sizable literatures, one on time discounting and one on updating biases. We follow recent experiments on present bias in studying the intertemporal allocation of effort, a non-fungible carrier of utility for which the resulting utility flow can be dated precisely.[5] Experiments by Ariely and Werten-

[3]In Appendix A.1 we show how the inferential biases that our methodology would have allowed us to uncover could in principle account for persistent naiveté in an infinite-horizon setting with repeated learning opportunities.

[4]Our approach is similar in spirit to that of Augenblick and Rabin (2018), who detect deviations from Bayesian inference by comparing belief movements with uncertainty reduction in people's beliefs. A key difference is our reliance on measuring the data generating process as well as participants' beliefs over it.

[5]Early studies in the literature on present bias elicit preferences over time-dated monetary payments (see Frederick et al., 2002, for a survey), but several papers have argued that the monetary domain is not appropriate for the measurement of preferences over utility streams due

broch (2002), Augenblick et al. (2015), Kaur et al. (2015) and Bisin and Hyndman (2018) all find substantial present bias in the intertemporal allocation of consumption events and some demand for commitment, suggesting that participants are at least partially aware of their present bias.[6] In the experiment most closely related to ours, Augenblick and Rabin (2019) compare incentivized predictions and ex-ante choices with on-the-spot effort choices and estimate the parameters of a quasi-hyperbolic discounting model (Laibson, 1997) to uncover near-complete naiveté. Possibly owing to our elicitation of probabilistic beliefs rather than point estimates, our measurements suggest that participants are only partially naive.

Our paper goes beyond the current literature on present bias in a number of ways. By collecting data about the actual and perceived correlation of intertemporal decisions, we are able to retrieve and analyze the actual and perceived information structure underlying participants' behavior. By using probabilistic predictions and outcomes instead of point estimates, we are able to apply Bayes' rule, which does not constrain the evolution of subjective point estimates. Finally, our use of a non-parametric model allows for the detection and measurement of present bias and naiveté independently of functional form assumptions about preferences.

A series of experimental papers, starting with Phillips and Edwards (1966) and culminating in Eil and Rao (2011) and Möbius et al. (2014), analyze individuals' updating behavior compared to a Bayesian benchmark.[7] These experiments tend to generate noisy feedback about an objective variable (e.g., the performance in an IQ test) and ask participants to report both their prior and their posterior beliefs. Since the information structure that generates the signal is exogenously given and communicated to subjects, it is easy for the researcher to derive a Bayesian benchmark. At the same time, the updating task that subjects face is artificial and may draw on mathematical ability as much as it draws on intuition. In our setting, the signal structure is initially unknown and we need to infer it from the data. However, this allows us to ask whether people are good intuititive statisticians when they learn from familiar, naturally-occuring information like their own behavior.

In a similar vein, our work relates to experiments by Eyster and Weizsäcker (2010), Enke and Zimmermann (2017) and Hossain and Okui (2018), who use ar-

to the fungibility of money (Cubitt and Read, 2007; Chabris et al., 2008; Augenblick et al., 2015; Cohen et al., 2019).

[6]Relatedly, Toussaert (2018) documents that experimental subjects demand commitment, not only in order to implement their preferred decision, but also to avoid the experience of future temptation and the associated self-control costs.

[7]Also see Ertac (2011); Buser et al. (2018); Schwardmann and van der Weele (2018); Coutts (2019); Zimmermann (2020) for updating in the domain of ego-relevant information, Gotthard-Real (2017); Coutts (2019); Barron (2016) for updating in the financial domain, and Benjamin (2019) for a recent review.

tificial, exogenous information structures to document systematic misperceptions of correlation in participants' updating behavior and attitudes towards information sources.[8] To our knowledge, our paper is the first to document an underestimation of the autocorrelation of intertemporal decisions and to draw out its implications. Our participants' non-belief in their propensity to learn is in line with results in Charness et al. (2018), who find that, while experimental subjects are able to learn from complicated information structures, this does not translate into them choosing the correct information structure.

The next section introduces the framework on which our analysis of learning is based. Section 3 describes our experimental design and Section 4 our results. Section 5 discusses the implications of our results and suggests avenues for future research.

2 Theoretical framework

Participants in the experiment work on real effort tasks at dates 2 and 4 and make predictions about their future effort choices at dates 1 and 3. Figure 1 depicts this timeline. We denote effort choices at date t by $a_t = 1$ for high and $a_t = 0$ for low effort. Then, $q(a_2, a_4)$ is the probability distribution over the four possible intertemporal events $(a_2, a_4) \in \{0, 1\}^2$, which we construct from the frequency of each combination of effort levels in the data. Participants' prior beliefs, at date 1, are given by the probability distributions $p_1^i(a_2, a_4)$ and their posterior beliefs, at date 3, by the probability distributions $p_3^i(a_4 \mid a_2)$, which are conditioned on realized effort a_2. The superscript i marks individual-level variables and is dropped when we refer to average quantities. From the primitive q we can construct the marginal distributions $q(a_t)$ and, whenever $q(a_2) > 0$, the conditional distributions $q(a_4 \mid a_2)$. We can also construct marginal and conditional belief distributions from the individual prior belief distributions p_1^i. For ease of exposition, we assume that q, p_1^i and p_3^i have full support.

While our methodology is agnostic about the underlying model generating the effort decisions, one possible foundation is that these effort choices reflect idiosyncratic preference parameters. For instance, consider a population of quasi-hyperbolic discounters for whom the present-bias parameter $\beta \in [0, 1]$ is distributed according to the cdf $F(.)$.[9] However, individual i believes that her idiosyncractic type β_i is dis-

[8]See DeMarzo et al. (2003), Ortoleva and Snowberg (2015), and Levy and Razin (2015) for applications of correlation neglect and Spiegler (2016) for a general framework for analyzing misperceptions of causal or statistical relationships between decision-relevant variables.

[9]Under the quasi-hyperbolic discounting model (Laibson, 1997), the decision-maker's valu-

tributed according to $\hat{F}_i(.)$. An individual with type β has a probability $\lambda(a_2, a_4|\beta)$ of choosing an (intertemporal) effort equal to (a_2, a_4). Under these assumptions, the prior beliefs and the true data-generating process (in an infinite population) are given by

$$\begin{cases} p_1^i(a_2, a_4) = \int \lambda(a_2, a_4|\beta) d\hat{F}_i(\beta) \\ q(a_2, a_4) = \int \lambda(a_2, a_4|\beta) dF(\beta). \end{cases}$$

Sophistication is the correct belief about (average) future effort ($\hat{F}_i$ coincides with F on average), and naiveté the statistical overestimation of future effort ($\hat{F}_i$ first-order stochastically dominates F on average). Naiveté can be detected at the sample level by comparing participants' average beliefs about future effort with the probability with which high effort is actually realized.

Our objective is to make normative statements about the learning of the population, measured by the movement from the prior beliefs to the conditional posterior beliefs. We ask two questions:

1. Is this learning conducive to sophistication in the long run? More precisely, would aggregate beliefs converge to the true distribution if the task were repeated infinitely often?

2. Is this learning consistent with individuals' own forecast at date 1? In other words, do individuals correctly anticipate how much information they will infer from their effort choice?

We construct two distinct benchmarks to answer these questions. These benchmarks are based on a rewriting of Bayes' rule using likelihood ratios. Following the reception of a signal I, the posterior beliefs can be expressed as the product of the prior and the informativeness of the signal:

$$\underbrace{\frac{p_3(a_4 = 1 \mid I)}{p_3(a_4 = 0 \mid I)}}_{\text{posterior likelihood ratio}} = \underbrace{\frac{p_1(a_4 = 1)}{p_1(a_4 = 0)}}_{\text{prior likelihood ratio}} \underbrace{\frac{p(I \mid a_4 = 1)}{p(I \mid a_4 = 0)}}_{\text{signal likelihood ratio}} .$$

Our two benchmarks replace the signal likelihood ratio in this formula with expressions that measure, respectively, the *actual informativeness* of the effort level a_2 (at the aggregate level), and the *anticipated informativeness* of a_2 (at the individual level).

ation of consumption streams $(x_0, x_1, \cdots)$ is given by the functional $V(x_0, x_1, \cdots) = u(x_0) + \beta \sum_{t \geq 1} \delta^t u(x_t)$, where u is the agent's utility function and δ is the long-run discount factor.

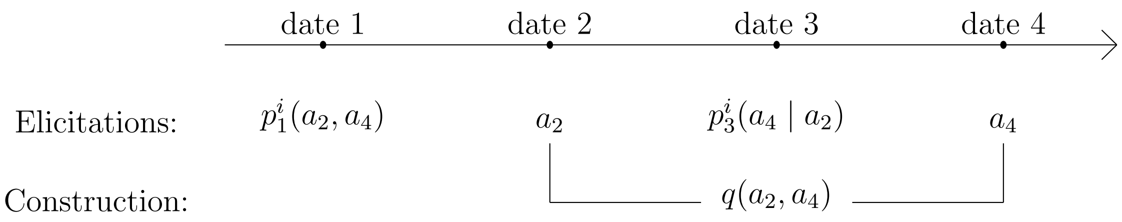


Figure 1 – Timeline.

Informed posterior beliefs To answer the first question above, we investigate the structural properties of the population's average learning. We are interested in whether individuals learn enough from their past effort choices for the population to become sophisticated after many repetitions of the task. At the aggregate level, the *actual informativeness* of effort choice a_2 can be measured by the likelihood ratio $q(a_2 \mid a_4 = 1)/q(a_2 \mid a_4 = 0)$. Our benchmark combines the actual informativeness of a_2 and average prior beliefs $p_1(a_4)$.

Definition 1. Given the average prior beliefs p_1, the *informed posterior beliefs* p_3^I, conditional on a_2, are defined by

$$\underbrace{\frac{p_3^I(a_4 = 1 \mid a_2)}{p_3^I(a_4 = 0 \mid a_2)}}_{\text{informed posterior likelihood ratio}} = \underbrace{\frac{p_1(a_4 = 1)}{p_1(a_4 = 0)}}_{\text{prior likelihood ratio}} \underbrace{\frac{q(a_2 \mid a_4 = 1)}{q(a_2 \mid a_4 = 0)}}_{\text{actual informativeness}} . \tag{1}$$

For each $a_2 \in \{0, 1\}$, our analysis will compare the average elicited posterior beliefs $p_3(a_4 \mid a_2)$ in this subgroup with the informed posterior beliefs $p_3^I(a_4 \mid a_2)$ implied by the signal a_2. This comparison has two virtues. First, it allows us to decompose posterior naiveté into two expressions: prior naiveté and a residual that we interpret as the inferential naiveté. To illustrate, rearranging Equation 1 yields

$$\underbrace{\frac{\frac{p_3(a_4 = 1 \mid a_2)}{p_3(a_4 = 0 \mid a_2)}}{\frac{q(a_4 = 1 \mid a_2)}{q(a_4 = 0 \mid a_2)}}}_{\text{posterior naiveté}} = \underbrace{\frac{\frac{p_1(a_4 = 1)}{p_1(a_4 = 0)}}{\frac{q(a_4 = 1)}{q(a_4 = 0)}}}_{\text{prior naiveté}} \underbrace{\frac{\frac{p_3(a_4 = 1 \mid a_2)}{p_3(a_4 = 0 \mid a_2)}}{\frac{p_3^I(a_4 = 1 \mid a_2)}{p_3^I(a_4 = 0 \mid a_2)}}}_{\text{inferential naiveté}} .$$

In a population whose elicited posterior beliefs are equal to the informed posterior, naiveté at date 3 is thus entirely due to an inflated prior.

Second, as we proove in Appendix A.1, a population that updates in line with the informed posterior beliefs will become sophisticated after infinite iterations of the task. In contrast, a deviation from this benchmark can potentially explain the persistence of naiveté. For instance, a population that under-reacts to low effort, as reflected by $p_3(a_4 = 1 \mid a_2 = 0) > p_3^I(a_4 = 1 \mid a_2 = 0)$, can maintain naive expectations forever.

Anticipated posterior beliefs To answer the second question, we analyze, at the individual level, how participants forecast their future updating from the perspective of date 1. For each individual i, we measure the *anticipated informativeness* of the signal a_2 by means of the subjective likelihood ratio $p_1^i(a_2 \mid a_4 = 1)/p_1^i(a_2 \mid a_4 = 0)$. This measure captures the intensity of the updating that individual i expects to perform conditional on a_2. Applying this ratio to the prior beliefs pins down the (Bayesian) posterior beliefs that participant i initially expects to form.

Definition 2. Given a subject's prior beliefs p_1^i, the *anticipated posterior beliefs* $p_3^{i,A}$, conditional on a_2, are defined by

$$\underbrace{\frac{p_3^{i,A}(a_4 = 1 \mid a_2)}{p_3^{i,A}(a_4 = 0 \mid a_2)}}_{\text{anticipated posterior likelihood ratio}} = \underbrace{\frac{p_1^i(a_4 = 1)}{p_1^i(a_4 = 0)}}_{\text{prior likelihood ratio}} \underbrace{\frac{p_1^i(a_2 \mid a_4 = 1)}{p_1^i(a_2 \mid a_4 = 0)}}_{\text{anticipated informativeness}} . \tag{2}$$

The anticipated posterior beliefs simulate the learning process of a participant who, at date 2, extracts as much information from a_2 as she expected at date 1. A rational population learns in such a way that, for each signal a_2, the average elicited posterior $p_3(a_4 \mid a_2)$ coincides with the average anticipated posterior $p_3^A(a_4 \mid a_2)$.[10] In our experiment, we document that anticipated posterior beliefs are closer to the prior beliefs than they are to the elicited posterior beliefs. This violation of Bayesian updating implies that participants, on average, underestimate how much they will learn from their experience with the task.

Our measures of actual and anticipated informativeness leverage the (actual and anticipated) statistical relationships between the variable subjects learn from (a_2) and the variable they learn about (a_4). As a result, our framework is agnostic

[10]This is not necessarily true at the individual level, since subjects might receive private information between dates 1 and 3. For example, consider a subject who is affected by an adverse and durable productivity shock just before date 3. She would legitimately report a posterior belief about a_4 that is more pessimistic than her anticipated posterior and our test would falsely categorize her as an erroneous learner. However, rational learning from sources unknown to the analyst cannot explain a deviation from the Bayesian posterior in the aggregate because, assuming that these private signals are uncorrelated to each other, the law of iterated expectations precludes any systematic effect of private information on average posterior beliefs. Therefore, our analysis focuses on average updating, just as naiveté and sophistication are statements made about average beliefs. To abate concerns about correlated information shocks, which any analysis of learning or naiveté is vulnerable to, our design randomizes the exact dates at which participants work on their tasks.

about the underlying model that generates the effort decisions. Let us highlight two consequences of this fact.

First, our framework can be applied in any setting in which individuals learn from natural signals. For example, our conceptual apparatus can be used to analyze how students learn from their grade in one semester (a_2) about their likely grade in the next semester (a_4). In this paper, we use our methodology to detect biases in learning about one's effort, as mispredictions of future behavior have been shown to be particularly problematic (O'Donoghue and Rabin, 1999) and pronounced (Augenblick and Rabin, 2019) in this domain.

Second, our framework does not require assumptions about why a_2 and a_4 are correlated. The standard foundation in learning models is that a_2 and a_4 both depend on a fundamental preference parameter that is initially unknown. But it could also be the case that a_2 and a_4 are affected by a common shock that occurs before date 2, or that a_2 has a causal effect on effort a_4 due to habit formation. Our measures of informativeness are independent of the exact foundation and our detection of biases does not require knowledge of the true data-generating process of effort choices. The flipside of this generality is that, by itself, our methodology cannot pin down what exactly it is that subjects are learning about. In section 4.6 we therefore supplement our analysis and show that participants in the experiment are learning both about their effort costs and about their self-control.

3 Experimental design

The experiment was conducted in June 2018 and preregistered. Subjects were recruited via standard ORSEE procedures (Greiner, 2015) and participated in five experimental sessions across three weeks. The initial session took place at the Munich Experimental Laboratory for the Social Sciences and the remaining four sessions took place online. The four online sessions are labeled $t = 1, 2, 3, 4$ and featured either one of two real effort tasks (at dates 2 and 4) or the elicitation of predictions of and preferences over future effort choices (at dates 1 and 3).

At the initial session, we told subjects the dates of all future sessions and the content of the sessions at dates 2 and 4. We also provided information on payment rules and technical requirements for accessing the experimental website. Subjects then had to complete 5 practice screens of the first and 5 practice screens of the second real effort task, before being introduced to the BDM mechanism used to elicit beliefs and answering some comprehension questions about the mechanism. After the first session in the lab, subjects received an email summarizing all relevant information.

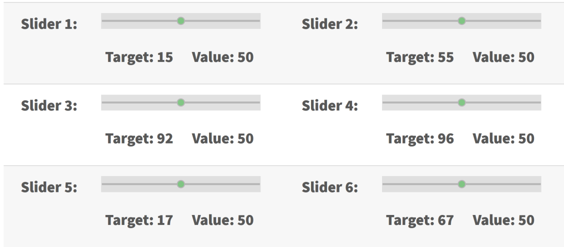
On the day of an online session a participant had a 24-hour window, from midnight to midnight, to log on to the experimental website and make her choices. Experimental instructions are provided in the online appendix, and the complete decision environments can be found on a "tourist version" of the website we set up for the experiment (`https://www.lsc-experiment.com`).

Subjects who missed a session were excluded from the experiment and did not receive any payment, irrespective of their previous decisions and earnings. Subjects who completed all sessions received a participation fee of 25 Euro and a bonus payment determined by their effort choices in the real effort tasks and their earnings from the belief elicitations. On the day of an online session, we sent three reminder emails. All payment rules were made transparent at the beginning of the experiment and participants were reminded of them every time it was relevant for their decisions. Participants were paid via bank transfer a few days after the last session.

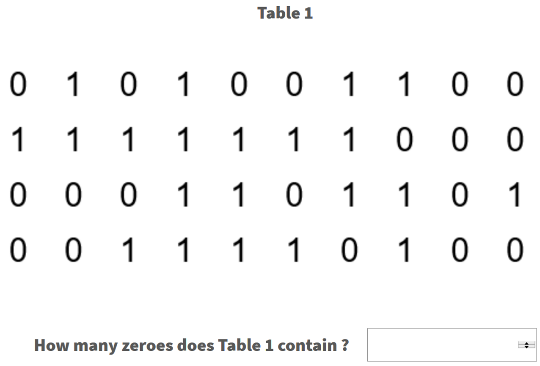
Timeline. Date 1 took place two days after the initial session. The two-day lag between the initial session in the lab and the first elicitation of beliefs is intended to eliminate the possible effect of projection bias (Loewenstein et al., 2003; Kaufmann, 2018), i.e. to avoid that subjects report a lower willingness to engage in the task in the future and pessimistic predictions because they very recently exerted a lot of effort and feel tired. Date 2 took place roughly one week after the initial session. We randomized the exact date at the individual level within a window of three consecutive days in order to reduce the incidence of correlated shocks on subjects' effort cost (e.g., due to weather). A participant's date 3 session took place exactly two days after her date 2 session. Date 4 took place in the following week and we again randomized the date between three possible dates, independently of date 2.[11]

Experimental tasks. Our real effort tasks are depicted in Figures 2a and 2b. In the *sliders* task subjects saw a screen with 40 slider bars, initially positioned on 50 and associated with a random target between 0 and 100. Completing one screen required positioning all 40 sliders on their target with the mouse or the keyboard. In the *counting zeroes* task subjects saw a screen with 10 matrices of 4 rows and 10 columns of ones and zeroes each. Completing one screen required counting the number of zeroes in each matrix and reporting this number in a text area. A screen submitted with incorrect entries had to be redone. On average, subjects completed

[11]These dates were not consecutive because we excluded one day (June 27th, 2018) due to a scheduled football world cup game involving Germany which could have affected subjects' aggregate effort.



(a) Sliders



(b) Counting zeroes

Figure 2 – Experimental tasks (translated from German).

one screen in 3 minutes and 26 seconds for the sliders task, and in 3 minutes and 54 seconds for the counting zeroes task.

We construct a binary variable a_2 (a_4) equal to 1 if the subject completed more than 20 screens at date 2 (4) and 0 otherwise. The threshold of 20 screens was calibrated to obtain balanced subsamples for whom $a_2 = 0$ and $a_2 = 1$ respectively, as we need to analyze learning separately for these two subgroups. The calibration was based on the results of a small pilot that featured only dates 1 and 2 of the experiment.

Participants were paid for every batch of 5 screens they completed successfully, according to the concave payment scheme in Table 1.

Treatments. We implemented two experimental conditions. In the Same Tasks condition, subjects worked on the sliders at dates 2 and 4. In the Different Tasks condition, they worked on sliders at date 2 and on the counting zeroes task at date 4.

Number of screens completed	Payment for this batch of 5 screens	Cumulative payment
5	5 Euros	5 Euros
10	4 Euros	9 Euros
15	3 Euros	12 Euros
20	2 Euros	14 Euros
25	1.5 Euros	15.5 Euros
30	1 Euro	16.5 Euros
35	0.5 Euros	17 Euros
40	0.1 Euros	17.1 Euros

Table 1 – Payment scheme

Date 1: Ex-ante choices and predictions. First, subjects committed to the number of screens (between 0 and 40) that they wanted to complete at future dates 2 and 4. We call this object the *ex-ante choice*. We randomly selected whether this decision was binding or not and communicated the outcome of the randomization immediately after subjects made their choice.

If the decision was binding (with probability 5%), then at both dates 2 and 4, subjects had to complete the exact number of screens they committed to in order to receive the bonus for that session. Completing more screens was not permitted. These committed subjects are excluded from the analysis, since we do not observe their effort choices under the same incentive scheme at different dates.

If the decision was not binding (with probability 95%), then subjects were free to choose how much to work on the task at the future date. For this group, we elicited predictions of future effort. We described the four possible future events (a_2, a_4) (called *scenarios*) in a table and asked subjects to report their subjective probability distribution p_1 (see Figure 3). We required subjects to state four non-negative integers that sum up to 100. The order of the events presented in the table was randomized at the individual level.

We incentivized predictions with a BDM mechanism (Karni, 2009) associated with a 3 Euro prize. We selected one of the joint events $(a_2, a_4) \in \{0,1\}^2$ at random and paid the subject according to the BDM mechanism applied to the stated probability.[12]

One potential concern about the belief elicitation mechanism is its possible lack of incentive-compatibility due to the endogeneity of effort decisions: Subjects could report a probability of high effort that is more confident than their true belief as a commitment strategy to work longer in future sessions. To solve this issue, the BDM mechanism was implemented with probability 50% and the uncertainty was

[12] Suppose that the subject states a subjective probability equal to x percent for a given event. The BDM mechanism for this event selects a random integer y between 0 and 100 with uniform probability. If $x < y$, then the subject receives 3 Euro with probability y, otherwise the subject receives 3 Euro if and only if the event occurs.

realized and communicated immediately after the elicitation of beliefs. We test for the (successful) strategic use of the elicitation mechanism by measuring whether subjects whose beliefs were payoff-relevant exerted higher effort in the corresponding work session. As an additional step we asked subjects to state whether they reported their true beliefs or not in the post-experimental survey.

	Session 2 (18.06.2018) Sliders		**Session 4 (26.06.2018) Counting zeroes**	**Your prediction**
Scenario 1	20 screens or more	**AND**	20 screens or more	%
Scenario 2	20 screens or more	**AND**	fewer than 20 screens	%
Scenario 3	Fewer than 20 screens	**AND**	20 screens or more	%
Scenario 4	Fewer than 20 screens	**AND**	fewer than 20 screens	%

Figure 3 – Elicitation of prior beliefs (translated from German).

Date 2: Real effort task. All subjects were required to log on and then decide how long to work on the task, with no minimum effort requirement and a maximum of 40 screens. While this is not crucial for the study of naiveté and learning, we required all subjects to log on even if they did not wish to complete any screen in order to interpret their effort choice as an active decision. Otherwise, we would not be able to tell whether a subject who skipped a session had decided not to work on the task or simply forgot to log on. This would confound the identification of present bias in effort choice (Ericson, 2011, 2017). Each screen was numbered (from 1 to 40) and contained a "Finish" button that terminated the session. Upon submission of a correctly entered screen, the next screen was automatically called up.[13] Taking breaks was not allowed.

Subjects who were committed to the number of screens chosen at date 1 earned nothing before they reached their target and their session was automatically terminated if they reached the target. This information was provided to them on every screen. Subjects who were not committed saw the payment scheme as well as their

[13]Subjects faced a stopping problem, since the decision of terminating a session was made at every point in time. In contrast, Augenblick et al. (2015), Augenblick and Rabin (2019) and Fedyk (2018) ask subjects to commit to their effort choice at the beginning of the session. These two paradigms are equivalent if time preferences inside the experimental session are dynamically consistent, but not if subjects prefer higher effort when they start working than when they have already completed some screens. We deemed the stopping problem attractive because it better captures most real-life behaviors and because it is arguably the more natural choice for participants to form expectations over.

accumulated bonus on every screen. Upon clicking on "Finish" a dialog box opened and the participant was required to confirm that she wanted to stop working and receive her current earnings.

Date 3: Ex-ante choices and predictions. Subjects that were not committed to their date 1 effort choice selected a number of screens that they committed to completing at date 4. The decision was binding with probability 5% and its consequence was identical to the ex-ante choice made at date 1. Subjects whose ex-ante choice was binding were excluded from the analysis at this stage. Our final sample thus consists only of participants for whom we observe ex-ante choices at dates 1 and 3 and unconstrained effort choices at dates 2 and 4. These subjects reported their posterior beliefs p_3 over the two events $a_4 \in \{0, 1\}$ (in a random order), in a table similar to that used at date 1. We incentivized the prediction with the BDM mechanism with a 3 Euro prize, and made it payoff-relevant with probability 50%.

Date 4: Real effort task. This session mirrors the session at date 2, except that subjects in the Different Tasks condition worked on the counting zeroes task instead of the sliders task. After they worked on the task, earnings were announced and we asked subjects to fill in a short post-experimental survey (e.g. about their gender, age, parents' income and feedback about their decisions).

Sample size. In total, 201 subjects came to the initial session, where 11 decided not to take part in the experiment, e.g. because of scheduling conflicts with their randomly assigned sequence of dates for the online sessions. Only 3 subjects started the experiment but subsequently missed an online session. Therefore, attrition was minor. Our sample thus includes 187 participants. 19 of these subjects were committed to their effort decisions made in advance (either at date 1 or 3) and are thus excluded from the analysis. Our final sample consists of 168 individuals, 88 in the Same Tasks condition and 80 in the Different Tasks condition.

4 Results

In what follows we provide evidence for present bias and naiveté about present bias and then study how subjects learn and anticipate to learn from their behavior. Throughout, we pool data from our two conditions, before analyzing treatment differences in section 4.5.

4.1 Present bias and naiveté

Figure 4 compares ex-ante choice, beliefs about future effort, and effort relative to the 20 screen threshold. Since beliefs are probabilistic over the binary state of the world that a subject completes 20 screens or more, we make ex-ante and on-the-spot effort choices comparable by coding them as equal to 100 percent if the threshold is passed (high effort) and 0 otherwise (low effort). In Appendix A.2 we also document present bias using the exact number of screens.

At both dates, more participants commit to high effort than end up exerting high effort on the spot (two-sided t-test, dates 1-2: $p < 0.001$; dates 3-4: $p < 0.001$), confirming that participants are present biased.[14] Participants' average beliefs are also significantly higher than their effort choice (two-sided t-test, dates 1-2: $p = 0.005$; dates 3-4: $p < 0.001$), but significantly lower than their ex-ante choice (two-sided t-test, dates 1-2: $p = 0.002$; dates 3-4: $p = 0.003$), revealing partial naiveté about self-control.

Result 1 (**Present bias and naiveté).** *Effort choices are time-inconsistent at both dates, and participants are partially naive about their future effort.*

We find that 90 out of 168 subjects complete 20 or more screens at date 2. This variation in effort choices assures that our analysis of updating, where we will need to separately look at participants who exerted high effort (good signal) and low effort (bad signal) at date 2, is well-powered. We also observe that aggregate effort decreases over time, with the proportion of participants exerting high effort being significantly lower at date 4. This vindicates the usefulness of a methodology that allows us to study learning without relying on the stability of aggregate behavior.

4.2 Do subjects learn from their behavior?

Is there scope for learning? While the unconditional probability of high effort at date 4 equals 43.5%, this probability goes up to 73.3% if we condition it on high effort at date 2, and goes down to 9.0% if we condition it on low effort at date 2. A Fischer's exact test confirms that $q(a_4 = 1 \mid a_2 = 1)$ is larger than $q(a_4 = 1 \mid a_2 = 0)$ ($p < 0.001$). Therefore, behavior is highly correlated across periods and a_2 is highly predictive of a_4.

We measure the amount of information contained in a_2 by computing the likelihood ratios $LR^q(a_2) = q(a_2 \mid a_4 = 1)/q(a_2 \mid a_4 = 0)$ based on the full sample. We

[14] Among the participants who commit to high effort at date 1 (date 3), 31.36 % (27.27 %) end up exerting *low* effort at date 2 (date 4).

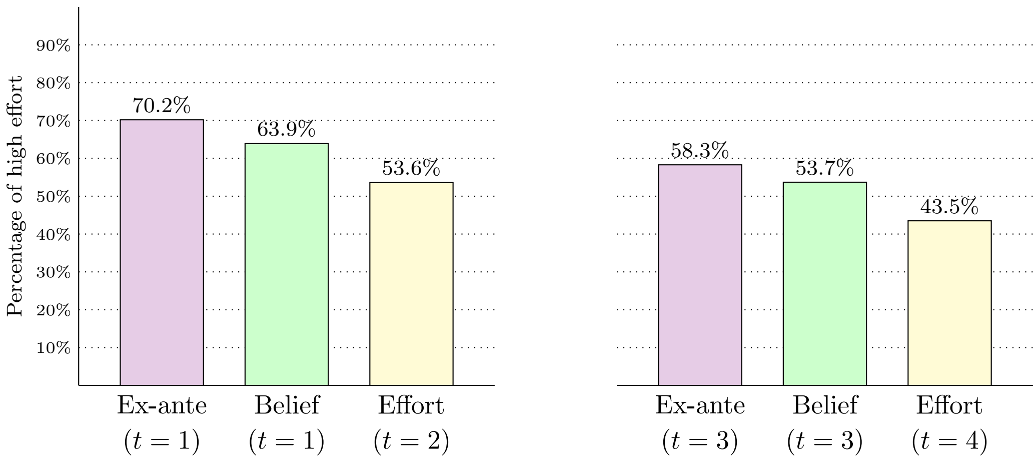


Figure 4 – Average ex-ante choice, beliefs about effort and on-the-spot effort choice.

find that $LR^q(a_2 = 1) = 3.57$, and that $LR^q(a_2 = 0) = 0.12$. To give an element of comparison, consider a hypothetical information structure generated by an oracle who observes a_4 and sends a probabilistic, informative message to the participant in the past: The oracle speaks the truth with probability x% and lies with the complementary probability. In our dataset, the signal $a_2 = 1$ contains as much information as a message for which $x = 78.1\%$, while the signal $a_2 = 0$ contains as much information as a message for which $x = 89.3\%$.

Do subjects learn appropriately? As defined in Section 2, the informed posterior beliefs against which we compare the elicited posterior beliefs are constructed by updating the average prior belief of the population $p_1(a_4 = 1)$ (which equals 61.4%) at the rate prescribed by the likelihood ratio $LR^q(a_2)$, for each a_2.[15] The elicited posterior after high effort at $t = 2$ is equal to 83.6%, while the informed posterior is equal to 85.0% (two-sided t-test, $p = 0.615$). The elicited posterior after low effort at $t = 2$ is equal to 19.2%, while the informed posterior is equal to 16.9% (two-sided t-test, $p = 0.526$). Therefore, we can not reject the null hypothesis that the elicited posterior is equal to the benchmark provided by the informed posterior.[16]

Result 2 (Actual learning). *Effort at date 2 is highly informative about effort at*

[15]That is,

$$p_3^I(a_4 = 1) = \frac{p_1(a_4 = 1)LR^q(a_2)}{p_1(a_4 = 1)LR^q(a_2) + p_1(a_4 = 0)}.$$

[16]With this construction we only test whether subjects learn well from the information inherent to their binary effort a_2. However, it is plausible that the precise effort level contains additional information. For instance, completing 40 screens at date 2 is likely to be a stronger signal of a high effort at date 4 than completing only 20 screens. We discuss this point and construct informed posterior beliefs using a finer information structure in Appendix A.5.

date 4. Participants incorporate the appropriate amount of information into their posterior beliefs.

4.3 Do subjects anticipate their future learning?

We construct the anticipated posterior beliefs $p_3^{i,A}$ at the individual level as a function of prior beliefs p_1^i and effort a_2^i.[17] We only do this for subjects for whom $p_1^i(a_2^i) > 0$, as Bayes' rule is silent about updating after a zero-probability event. For this section, we therefore exclude 16 subjects from the dataset, 11 who provided low effort and 5 who provided high effort. Their updating behavior is reported in Appendix A.6.

Figure 5 compares participants' average prior, anticipated posterior, elicited posterior, informed posterior and true likelihood of exerting high effort at date 4.[18] Panel 5a depicts the case of a bad signal, i.e. low effort, at date 2. Panel 5b depicts the case of a good signal, i.e. high effort, at date 2. Participants who exert high effort at date 2 have substantially higher priors, as reflected in the respective first bars in the panels.

For the case of a bad signal, we see that the anticipated posterior reflects less learning than both the elicited and the informed posterior. On the other hand, elicited posteriors are not significantly different from the informed posterior. All posteriors lie above the true likelihood of high effort. Therefore, subjects learn more than they anticipated and arrive at posteriors that are naive but consistent with the informed posterior benchmark.

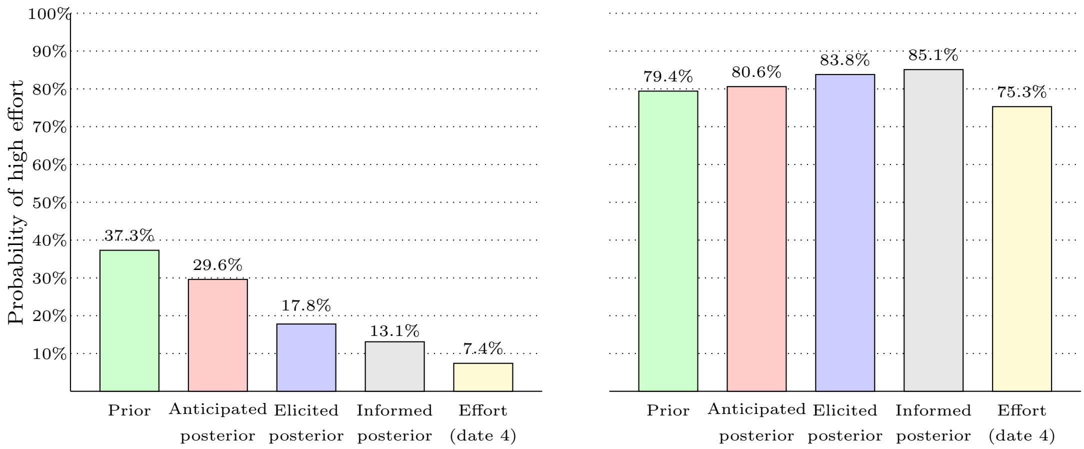
In the case of a good signal, we see that the elicited posterior is consistent with the anticipated posterior and the informed posterior. Owing to the inflated priors, all posteriors once again are significantly higher than the true likelihood of high effort.

The statistical relationships mentioned above are summarized and made precise in Table 2. The main message emerging from this table is that subjects update slightly less than they should following both low and high effort, but not significantly so. However, after a failure they update substantially more than they expect initially. Subjects learn rather well once they are confronted with the information

[17] That is,

$$p_3^{i,A}(a_4 = 1) = \frac{p_1^i(a_2^i, a_4 = 1)}{p_1^i(a_2^i)}.$$

[18] For the sake of comparability, we compute these measures based on the sample consisting only of the subjects for whom the anticipated posterior beliefs are defined. This explains why some variables have different values than the ones reported in section 4.2.



(a) Beliefs and effort after *low* effort at date 2 (b) Beliefs and effort after *high* effort at date 2

Figure 5 – Priors, average posterior beliefs and actual effort at date 4 after both low (panel a) and high (panel b) effort at date 2.

Low effort at date 2 $(N = 67)$						***High effort at date 2*** $(N = 85)$					
Variable 1		**Variable 2**		**Diff.**	**p-value**	**Variable 1**		**Variable 2**		**Diff.**	**p-value**
Prior	37.3	Ant. post.	29.6	7.7	0.004	Prior	79.4	Ant. post.	80.6	-1.2	0.113
		Elic. post.	17.8	19.5	< 0.001			Elic. post.	83.8	-4.5	0.155
		Inf. post.	13.1	24.2	< 0.001			Inf. post.	85.1	-5.7	0.033
		Effort	7.4	29.9	< 0.001			Effort	75.3	4.1	0.122
Ant.	29.6	Elic. post.	17.8	11.8	0.002	Ant.	80.6	Elic. post.	83.8	-3.2	0.293
post.		Inf. post.	13.1	16.5	< 0.001	post.		Inf. post.	85.1	-4.5	0.095
		Effort	7.4	22.2	< 0.001			Effort	75.3	5.3	0.048
Elic.	17.8	Inf. post.	13.1	4.7	0.173	Elic.	83.8	Inf. post.	85.1	-1.2	0.658
post.		Effort	7.4	10.3	0.003	post.		Effort	75.3	8.6	0.002

Table 2 – Pairwise comparisons of different posteriors with the informed posterior and the true probability of high effort at date 4. P-values derive from a two-sided t-test under the null hypothesis that the difference between the two variables is equal to zero.

inherent in their effort choice, but they underappreciate this fact ex ante.[19]

Result 3 (Non-belief in the propensity to learn). *At date 1, participants underestimate their future learning.*

4.4 Anticipated and actual improvement in predictions

Next we analyze anticipated and actual learning from a perspective that explicitly takes the quality of individual predictions into account. We measure the

[19]This result cannot be driven by a taste for consistency (Falk and Zimmermann, 2016), as subjects who want to make consistent reports would state posterior beliefs that are aligned with their anticipated posterior beliefs.

mistake inherent in a belief-effort pair $(p(a_4 = 1), a_4)$ by the absolute distance between prediction and realized effort $|p(a_4 = 1) - a_4|$.

Figure 6a compares the mistakes implied by our different elicited and hypothetical beliefs. Comparing the mistakes implied by participants' priors with the mistakes implied by their elicited posteriors, we see that subjects' predictions become much more accurate between date 1 and 3 (two-sided t-test, $p < 0.001$). We also see that the average mistake implied by the anticipated posterior is only a little smaller than the mistake implied by the prior (two-sided t-test, $p = 0.003$), but much larger than the mistake implied by the elicited posterior (two-sided t-test, $p < 0.001$).[20]

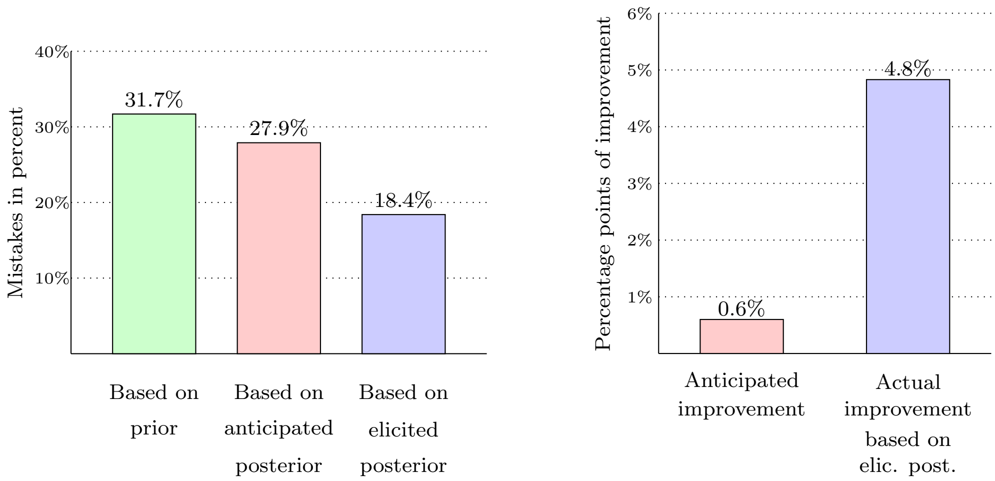
A consequence of the non-belief in the propensity to learn is that participants would be less willing to experiment than they should be. In particular, an individual who does not believe in the informativeness of her own behavior will deem it less worthwhile to first engage in a task before abandoning it or committing to it.

To make these ideas more precise, we calculate participants' anticipated improvement in their predictions as they move from their prior to their posterior. We measure this improvement as the increase in the probability of winning the prize of the BDM belief elicitation from being paid for a date 3 rather than an unconditional date 1 belief.

Figure 6b compares by how much participants anticipate their predictions to improve, and by how much their predictions actually improve. We see that the actual value of information is more than 8 times higher than the improvement implied by participants' perceived informativeness of effort at date 2 (two-sided t-test, $p < 0.001$).

Result 4. *Subjects initially underestimate the value of information.*

[20] It could be the case that participants receive some information about a_4 that is uncorrelated with a_2. Because this information is not reflected in the anticipated posterior beliefs, the comparison of the mistakes implied by the elicited posteriors and anticipated posteriors would be unfair. In Appendix A.7 we provide a way to eliminate the information orthogonal to a_2 in the posterior beliefs, and we show that the conclusion that subjects' elicited posterior beliefs are better calibrated than their anticipated posterior beliefs is robust.



(a) Absolute distance to effort at date 4

(b) Improvement in predictions

Figure 6 – Quality of predictions and improvement in predictions

4.5 Treatment comparison

To see whether a change in the environment is an obstacle to learning, we first compute the actual informativeness of a_2 in both conditions. In the Same Tasks condition we find that $LR^q(a_2 = 1) = 5.08$ and $LR^q(a_2 = 0) = 0.07$, while in the Different Tasks condition we find that $LR^q(a_2 = 1) = 2.80$ and $LR^q(a_2 = 0) = 0.13$. The fact that the likelihood ratios are closer to one in the Different Tasks condition implies that a_2 is less predictive of a_4 than in the Same Tasks condition. This means that our treatment manipulation was successful at increasing the "distance" between the decision problems. We compute the informed posterior beliefs separately for each treatment group based on the likelihood ratios computed in this condition.

Table 3 contains the treatment comparison of key variables.[21] There are no significant treatment differences in effort choice at date 4, the anticipated posterior, or the elicited posterior. We construct two measures that allow us to compare, from a normative viewpoint, the learning in both conditions without dividing each subsample again into the two subgroups $a_2 = 0$ and $a_2 = 1$. The first measure is the difference between the elicited posterior and the informed posterior, equal to $p_3^i(a_4 = 1) - p_3^I(a_4 = 1 \mid a_2^i)$. The second is a measure of under-reaction to information, equal to $p_3^i(a_4 = 1) - p_3^I(a_4 = 1 \mid a_2 = 0)$ for the subgroup $a_2^i = 0$ and equal to $p_3^I(a_4 = 1 \mid a_2 = 1) - p_3^i(a_4 = 1)$ for the subgroup $a_2^i = 1$. It measures by how much subjects under-update relative to the informed posterior benchmark (in both directions). We find no significant difference in either of these measures

[21] Table 9 in Appendix A.8 shows that treatment groups are balanced according to gender, age, mathematical ability, and date 2 effort.

Variable	Same Tasks ($N = 79$)	Different Tasks ($N = 73$)	Difference	p-value
Effort (date 4)	45.6	45.2	0.4	>0.999
Prior	63.2	58.3	4.9	0.404
Elicited posterior	56.7	52.6	4.1	0.550
Informed posterior ($a_2 = 1$)	89.7	79.6	10.1	
Informed posterior ($a_2 = 0$)	10.5	15.8	-5.3	
Elic. post. - inf. post.	4.1	-0.8	4.9	0.256
Underreaction to information	5.12	-0.13	5.25	0.223
Anticipated posterior	59.1	57.1	2.0	0.742
Elic. post. - ant. post.	-2.4	-4.5	2.1	0.669
Underestimation of learning	5.78	5.85	-0.06	0.990

Table 3 – Treatment comparisons of key variables. For the comparison of effort choice the p-value is based on Fischer's exact test, for all other comparisons a two-sided t-test was used.

between the two treatment groups, which suggests that subjects learn equally well in both conditions.

To assess whether the underestimation of future learning is more severe in one condition, we construct two measures again. The first is the difference between the elicited posterior and the anticipated posterior, equal to $p_3^i(a_4 = 1) - p_3^{i,A}(a_4 = 1 \mid a_2^i)$. The second expression is a measure of the underestimation of future learning, equal to $p_3^i(a_4 = 1) - p_3^{i,A}(a_4 = 1 \mid a_2 = 0)$ for the subgroup $a_2^i = 0$ and equal to $p_3^{i,A}(a_4 = 1 \mid a_2 = 1) - p_3^i(a_4)$ for the subgroup $a_2^i = 1$. We find no significant difference in either of these measures between the two experimental conditions, which suggests that the underestimation of future updating affected participants in both conditions equally. We therefore conclude that the change in the environment that we implemented had no effect on subjects' learning.

Result 5 (Treatment effect). *Subjects learn equally well and underestimate their learning to a similar degree in both conditions.*

4.6 Do subjects learn about their self-control problem, their effort cost, or both?

Subjects' underestimation of future effort may be caused by them underestimating their self-control problem or underestimating how difficult they will find the task, i.e. their effort costs. The underestimation of effort costs results in apparent present bias. Subjects take on too many tasks in advance and believe that they will complete them. But once they are in the midst of the task and realize how painful its completion will be, they decide to give up prematurely. Understanding the source of subjects' misperception is important. If subjects suffer from a self-control problem and are naive about it, then committing them to their plans can be welfare-improving. If apparently present biased behavior is driven by optimism about future effort costs, then committing subjects to their ex-ante preference decreases their welfare.

This section is concerned with understanding whether subjects are learning about effort costs or self-control, or both. The fact that subjects' aggregate learning is appropriate is already indicative of them learning about all sources of prior naiveté. Similarly, the fact that subjects are able to transport what they learn in one task to another task indicates that what they learn is not maximally task-specific, as would be expected if they only learned about task-specific effort costs.

However, to study the distinction between learning about self-control and learning about effort costs more deeply it is useful to look at the evolution of our subjects' ex-ante preferences. Any information a subject obtains about effort costs while working on the task at date 2 will manifest itself in the ex-ante preference she states at date 3. In particular, a substantial reduction in the ex-ante preference over date 4 effort as we move from date 1 to date 3 would reflect learning about unexpectedly high effort costs. We see this reduction in our data (ex-ante preference at $t = 1 = 23.29$ screens, ex-ante preference at $t = 3 = 20.44$ screens, two-sided t-test, $p<0.001$), indicating that subjects did initially underestimate their effort costs and then learned about them.

To see whether subjects also learned about their self-control, we check for the residual updating once we control for learning about effort costs, as captured by the change in ex-ante preferences over date 4 effort, $c(a_4)$, as we move from date 1 to date 3, i.e.

$$\Delta c = c_1(a_4) - c_3(a_4).$$

The variable Δc is independent of an agent's self-control because both ex-ante preferences are stated several days before the work date.[22]

Table 4 presents the results of linear regression models. In the main specification of columns 2 and 5, we estimate the following linear regression model for high and

[22]According to the quasi-hyperbolic discounting model, the self-control problem arises because consumption at any future date is discounted by an additional factor β. But decisions that have consequences only in the future are not affected by present bias. Augenblick (2018) estimates how discount rates evolve as the effort date gets closer, and finds that most of the decline in the discount rate indeed happens during the day preceding effort. This implies that ex-ante preferences elicited more than a day before the effort session should be time-consistent, and thus that Δc should be independent of self-control in our data.

	(1) Update	(2) Update	(3) Update	(4) Update	(5) Update	(6) Update
Δc		3.437***	2.000*		2.580***	2.644***
		(0.558)	(1.188)		(0.487)	(0.501)
Δc^2			-0.0883			0.0247
			(0.0645)			(0.0430)
(Net) Update	-26.47***	-9.465**	-10.35**	8.656**	11.32***	10.32***
	(4.602)	(4.683)	(4.702)	(3.603)	(3.195)	(3.646)
Observations	78	78	78	90	90	90
R^2	0.000	0.333	0.349	0.000	0.242	0.245

Table 4 – OLS regressions of belief updates on the change in ex-ante preferences and a constant that captures updates net of learning about error costs; robust standard errors in parentheses; $^{*}p < 0.10, ^{**}p < 0.05, ^{***}p < 0.01$.

low effort at $t = 2$ respectively.

$$(p_3(a_4) - p_1(a_4))_i = \beta_1 \Delta c_i + Net\, Update_i + \epsilon_i$$

where ϵ_i is an error term. The variable *Net Update* is the intercept of the regression model and captures learning about self-control, i.e. the residual updating we see once we control for learning about effort costs.

Columns 1 through 3 of Table 4 focus on updating after low $t = 2$ effort. Column 1 presents the unconditional update of -26.47 percentage points. Once we control for learning about effort costs in column 2, the net update of -9.47 is still significantly negative, indicating learning about self-control. Moreover, the significant coefficient of Δc in column 2 indicates that subjects also integrate what they learned about effort costs into their updates. In column 3, we then also control for the square of Δc, to allow for a marginal effect of Δc on the update that is either increasing or decreasing. Our results are robust to the inclusion of the square of Δc. Columns 4 through 6 tell a similar story for the case of high effort at $t = 2$. Subjects are learning about both their self-control and their effort costs.

Table 5 presents an additional set of tests. It features linear regressions of participants' beliefs on both their ex-ante choice and their effort choice. The regressions indicate whether and by how much subjects' beliefs are reflective of their long-term preferences and their ultimate effort choice. The first column features beliefs and ex-ante preferences at date 1 and effort at date 2, whereas the second column features beliefs and ex-ante preferences at date 3 and effort at date 4. We see that the coefficient of the ex-ante choice stays constant as subjects move from date 1 to date 3 and learn. On the other hand, the coefficient of effort increases in size and significance.

Column 3 features a regression that pools data across dates and adds a time

Dep. Variable	(1) Belief ($t = 1$)	(2) Belief ($t = 3$)	(3) Belief ($t = 1$ & $t = 3$)
Ex-ante choice	0.637*** (0.0428)	0.660*** (0.0346)	0.637*** (0.0428)
Effort	0.0671* (0.0361)	0.198*** (0.0344)	0.0671* (0.0361)
$t = 3$ (d)			-8.911** (3.873)
$t = 3$ * Ex-ante			0.022 0.061
$t = 3$ * Effort			0.131** 0.056
Constant	15.53*** (3.464)	6.62*** (1.910)	15.53*** (3.464)
Observations	168	168	168
R^2	0.689	0.865	0.793

Table 5 – OLS regressions of the determinants of beliefs with robust standard errors in parentheses; $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

dummy and its interactions with the previous explanatory variables. This regression confirms that effort, but not ex-ante choice, becomes more predictive of beliefs after learning takes place. Therefore, our results suggest that, after the learning opportunity, participants' beliefs reflect a greater awareness of their ultimate effort choice conditional on the assessment of effort costs inherent in their ex-ante choice. This, in turn, is indicative of learning about present bias.

4.7 Are belief elicitations used as a soft commitment?

It is possible that a sophisticated individual could use the belief elicitations as soft commitment devices. By stating a high belief, a subject makes it more expensive for her future self to only work a little, thereby incentivizing her to exert more effort. If belief elicitations are used as commitment, participants no longer state their true beliefs and what looks like naiveté may in fact be a sophisticated commitment strategy. Augenblick and Rabin (2019) and Fedyk (2018) test for the strategic use of belief elicitations by varying the incentives of the elicitation. They argue that if elicitations were used as a soft commitment device, then stated beliefs should increase in the incentives. Both papers find no such effect.

We implement a different test of this potential confound. Before every belief elicitation, participants in our experiment were told that their beliefs would be payoff-relevant with probability 50%. After they stated their beliefs, they were then told whether they were randomly selected to be in the group for whom this belief elicitation was payoff-relevant. We can then compare two groups who are identical in terms of their beliefs, but whose beliefs differ in whether they constitute

Dep. Variable:	(1) Effort (date 2)	(2) Effort (date 2)	(3) Effort (date 4)	(4) Effort (date 4)
Belief paid	1.702	1.165	-0.259	-0.227
	(1.675)	(1.431)	(1.807)	(1.186)
Ex-ante choice		0.602***		0.858***
		(0.0782)		(0.0681)
Constant	18.68***	4.534***	17.18***	-0.362
	(1.142)	(1.690)	(1.415)	(1.464)
Observations	168	168	168	168
R^2	0.006	0.282	0.000	0.542

Table 6 – OLS regressions of the determinants of effort choice with robust standard errors in parentheses; $^{*}p < 0.10,^{**} p < 0.05,^{***} p < 0.01$.

a monetary commitment to high effort. Then, if participants did in fact use their beliefs as a soft commitment, effort should be higher in the group whose beliefs were payoff-relevant.

Table 6 shows OLS regressions of effort choice on the payoff relevance of stated beliefs. The first column shows that effort at date 2 was not significantly higher if beliefs were paid. Column 2 confirms this, while controlling for participants' ex-ante preferences. Columns 3 and 4 focus on effort at date 4. The coefficients of the belief paid variable are now slightly negative and insignificant. If anything, we would have expected that the sophisticated deployment of the belief elicitation as a soft commitment would have increased after subjects had a chance to learn. As an additional step, we asked subjects in the post-experimental survey how their reported beliefs compared to their best prediction.[23] A vast majority (91.4%) indicated that they tried to report their best estimate, while only 6 subjects (3.6%) stated that they reported a larger likelihood of high effort than the one they had in mind. Among these 6 subjects only one justified this decision by the desire to affect future effort incentives. Our results therefore suggest that belief elicitations were not used as a soft commitment device.

5 Discussion

Naiveté about self-control is one of the best documented phenomena in behavioral economics. Our paper studies its evolution and asks whether individuals are able to learn from their past behavior to make better predictions about their future

[23]We asked the following question (translated from German): "In the past weeks we asked you several times to report the likelihood with which you believed you would complete more than a given number of screens in the future. What did you think when you answered these questions? 1. I tried to report my best estimate. 2. I did not think much of it and reported whatever answer came to my mind. 3. I reported a lower likelihood than the one I had in mind. 4. I reported a larger likelihood than the one I had in mind."

self-control.

Explanations for persistent naiveté. We find no evidence of an inferential bias that hinders learning. Instead, we show that individuals learn well and are able to transport their acquired self-knowledge from one environment to another environment. Therefore, our data does not resolve the puzzle of persistent naiveté. Neither is the puzzle resolved in theoretical work by Ali (2011) and Hestermann and Le Yaouanq (2019), who find that naiveté about self-control should be self-limiting if agents learn from their experience. A naive individual fails to commit to future effort and thereby exposes herself to the opportunity to learn about her self-control problem (Ali, 2011). Moreover, a naive individual may partially attribute her failure to exert effort to her current environment and therefore be compelled to change her environment which in turn enables her to see that her lack of self-control was the culprit all along (Hestermann and Le Yaouanq, 2019).[24]

Taken together with these papers, our findings greatly diminish the space of possible explanations for persistent naiveté. Future experiments could enrich the decision-making environment and thereby increase the scope for participants' misattribution of their failures to external factors.[25] It is also possible that individuals' learning is hampered by imperfect memory and its hard-wired (Bordalo et al., 2020) and self-servingly manipulated (Zimmermann, 2020) features. The role of memory can easily be accommodated in our framework by increasing the time lag between dates.

Implications of the non-belief in the propensity to learn. We show that participants fail to anticipate their learning ex ante. This novel result raises the important question of whether individuals experiment inefficiently little in their lives. For instance, workers might fail to try out different work arrangements (e.g., how to organize their daily schedule) and miss out on the opportunity to learn what makes them most productive.

The non-belief in the propensity to learn might also play a role in erroneous decisions that the literature has commonly attributed to naiveté only. Consider evidence in DellaVigna and Malmendier (2006) for the excess demand for annual

[24]These theories are based on the common assumption of incidental learning. Christensen and Murooka (2018) show that naive present-biased agents might procrastinate forever at learning if this requires an active and costly decision.

[25]Gagnon-Bartsch et al. (2018) explain mislearning by arguing that individuals might fail to attend to important data (e.g., their own behavior in the past) if this data is not valuable according to their incorrect model of the world. However, our purposefully stripped-down experimental design might make attending to all relevant data sufficiently simple to mute this mechanism of mislearning.

gym memberships, relative to individuals' actual attendance. Naiveté does not by itself predict the take-up of an incorrect membership. Individuals who overestimate their future attendance but believe in their propensity to learn might indeed find it optimal to experiment with a pay-per-visit scheme before committing to a long-term contract. In this and other settings, the welfare loss due to insufficient experimentation can be measured by contrasting individuals' willingness to pay for a trial period before they make a long-term decision with the actual welfare improvement that would result from this experimentation.

Another important consequence of the non-belief in the propensity to learn might be the failure to activate self-regulation mechanisms that help individuals overcome self-control problems. Ainslie (1975) argues that some of people's most important means of self-control are internal. In choosing between a smaller sooner reward (being lazy, eating a fatty food, getting angry etc.) and a larger later reward (reaping the pecuniary benefits of our effort, being in good shape, having good relationships etc.), our impulsivity might drive us to choose the smaller sooner reward. But if we are able to see our choice as a first in a long sequence of similar choices between smaller sooner rewards and larger later rewards and if we have the conviction that our making the impulsive choice contains a cue that we will end up choosing the smaller reward again in the future, then this bundling of rewards may tip the scale in favor of avoiding the impulsive choice. Bénabou and Tirole (2004) provide a theory for why our lack of self-control today should affect our prediction of similar choices in the future. They argue that people have imperfect recall when it comes to their deep preferences and have to infer them from their past behavior. For this reason, failing to exert self-control has a negative effect on our self-image and our attempts at future self-regulation.

Our results cannot speak to the causal effect of avoiding the present biased choice today on effort in the next period, nor do we measure whether participants' subjective model reflects such causality. However, a direct implication of the bundling of rewards à la Ainslie (1975) is that people should view their behavior in this period and the next as highly correlated. Put differently, a belief in the informativeness of behavior is a necessary condition for achieving self-control by bundling rewards. The near-complete absence of this belief in our subjects therefore indicates that this particular self-regulation mechanism will be difficult for people to deploy. Future work could test the causal effect of a non-belief in the propensity to learn on first period present bias by exogenously shifting beliefs about the autocorrelation of effort levels.

Applications for our methodology. Our technique for retrieving the perceived and actual information structure generated by naturalistic signals is well-suited to be applied in the field. For example, consider a longitudinal survey that elicits respondents' savings goals for the next two years, their subjective likelihood of meeting them before and after the first year, and whether goals were actually met. This dataset would allow a researcher to ask whether expectations were updated appropriately in light of the actual information structure, which in turn would inform appropriate policy measures aimed at, for example, alleviating undersaving. A policy maker can trust a population with *informed* posteriors to shed biased beliefs about its saving behavior by itself and to eventually commit to save more. But if a population is found to underweigh the signal contained in its past savings decisions, then it may make sense to target it with information campaigns.

References

Ainslie, G. (1975). Specious reward: a behavioral theory of impulsiveness and impulse control. *Psychological Bulletin 82*(4), 463.

Ali, S. N. (2011). Learning self-control. *Quarterly Journal of Economics 126*(2), 857–893.

Ariely, D. and K. Wertenbroch (2002). Procrastination, deadlines, and performance: Self-control by precommitment. *Psychological Science 13*(3), 219–224.

Augenblick, N. (2018). Short-term discounting in unpleasant tasks. Working paper.

Augenblick, N., M. Niederle, and C. Sprenger (2015). Working over time: Dynamic inconsistency in real effort tasks. *Quarterly Journal of Economics 130*(3), 1067–1115.

Augenblick, N. and M. Rabin (2018). Belief movement, uncertainty reduction, and rational updating. Working paper.

Augenblick, N. and M. Rabin (2019). An experiment on time preference and misprediction in unpleasant tasks. *Review of Economic Studies 86*(3), 941–975.

Barron, K. (2016). Belief updating: Does the 'good-news, bad-news' asymmetry extend to purely financial domains? Working Paper.

Bénabou, R. and J. Tirole (2004). Willpower and personal rules. *Journal of Political Economy 112*(4), 848–886.

Benjamin, D. J. (2019). Errors in probabilistic reasoning and judgment biases. In *Handbook of Behavioral Economics: Applications and Foundations 1*, Volume 2, pp. 69–186. Elsevier.

Bisin, A. and K. Hyndman (2018). Present-bias, procrastination and deadlines in a field experiment. Working paper.

Bordalo, P., N. Gennaioli, and A. Shleifer (2020). Memory, attention and choice. Working paper.

Buser, T., L. Gerhards, and J. van der Weele (2018). Responsiveness to feedback as a personal trait. *Journal of Risk and Uncertainty 56*(2), 165–192.

Chabris, C. F., D. Laibson, and J. P. Schuldt (2008). Intertemporal choice. In S. N. Durlauf and L. Blume (Eds.), *The New Palgrave Dictionary of Economics.* London: Palgrave Macmillan.

Charness, G., R. Oprea, and S. Yuksel (2018). How do people choose between biased information sources? Evidence from a laboratory experiment. Working Paper.

Christensen, E. G. B. and T. Murooka (2018). Procrastination and learning about self-control. Working paper.

Cohen, J. D., K. M. Ericson, D. Laibson, and J. M. White (2019). Measuring time preferences. Forthcoming.

Coutts, A. (2019). Good news and bad news are still news: Experimental evidence on belief updating. *Experimental Economics 22*(2), 369–395.

Cubitt, R. P. and D. Read (2007). Can intertemporal choice experiments elicit time preferences for consumption? *Experimental Economics 10*(4), 369–389.

DellaVigna, S. (2009). Psychology and economics: Evidence from the field. *Journal of Economic literature 47*(2), 315–72.

DellaVigna, S. and U. Malmendier (2006). Paying not to go to the gym. *American Economic Review 96*(3), 694–719.

DeMarzo, P. M., D. Vayanos, and J. Zwiebel (2003). Persuasion bias, social influence, and unidimensional opinions. *Quarterly Journal of Economics 118*(3), 909–968.

Eil, D. and J. M. Rao (2011). The good news-bad news effect: Asymmetric processing of objective information about yourself. *American Economic Journal: Microeconomics 3*(2), 114–38.

Enke, B. and F. Zimmermann (2017). Correlation neglect in belief formation. *Review of Economic Studies 86*(1), 313–332.

Ericson, K. M. (2017). On the interaction of memory and procrastination: Implications for reminders, deadlines, and empirical estimation. *Journal of the European Economic Association 15*(3), 692–719.

Ericson, K. M. M. (2011). Forgetting we forget: Overconfidence and memory. *Journal of the European Economic Association 9*(1), 43–60.

Ertac, S. (2011). Does self-relevance affect information processing? Experimental evidence on the response to performance and non-performance feedback. *Journal of Economic Behavior & Organization 80*(3), 532–545.

Eyster, E. and G. Weizsäcker (2010). Correlation neglect in financial decision-making. Working paper.

Falk, A. and F. Zimmermann (2016). Consistency as a signal of skills. *Management Science 63*(7), 2197–2210.

Fedyk, A. (2018). Asymmetric naivete: Beliefs about self-control. Working paper.

Frederick, S., G. Loewenstein, and T. O'Donoghue (2002). Time discounting and time preference: A critical review. *Journal of Economic Literature 40*(2), 351–401.

Gagnon-Bartsch, T., M. Rabin, and J. Schwartzstein (2018). Channeled attention and stable errors. Working paper.

Giné, X., D. Karlan, and J. Zinman (2010). Put your money where your butt is: A commitment contract for smoking cessation. *American Economic Journal: Applied Economics 2*(4), 213–35.

Gotthard-Real, A. (2017). Desirability and information processing: An experimental study. *Economics Letters 152*, 96–99.

Greiner, B. (2015). Subject pool recruitment procedures: Organizing experiments with orsee. *Journal of the Economic Science Association 1*(1), 114–125.

Hestermann, N. and Y. Le Yaouanq (2019). Experimentation with self-serving attribution biases. Working paper.

Hossain, T. and R. Okui (2018). Belief formation under signal correlation. Working paper.

John, A. (2018). When commitment fails - evidence from a field experiment. *Management Science*. Forthcoming.

Karni, E. (2009). A mechanism for eliciting probabilities. *Econometrica 77*(2), 603–606.

Kaufmann, M. (2018). Projection bias in effort choice. Working paper.

Kaur, S., M. Kremer, and S. Mullainathan (2015). Self-control at work. *Journal of Political Economy 123*(6), 1227–1277.

Kőszegi, B. (2014). Behavioral contract theory. *Journal of Economic Literature 52*(4), 1075–1118.

Laibson, D. (1997). Golden eggs and hyperbolic discounting. *Quarterly Journal of Economics 112*(2), 443–478.

Laibson, D. (2015). Why don't present-biased agents make commitments? *American Economic Review 105*(5), 267–72.

Levy, G. and R. Razin (2015). Correlation neglect, voting behavior, and information aggregation. *American Economic Review 105*(4), 1634–45.

Loewenstein, G., T. O'Donoghue, and M. Rabin (2003). Projection bias in predicting future utility. *Quarterly Journal of Economics 118*(4), 1209–1248.

Möbius, M. M., M. Niederle, P. Niehaus, and T. S. Rosenblat (2014). Managing self-confidence. Working paper.

O'Donoghue, T. and M. Rabin (1999). Doing it now or later. *American Economic Review 89*(1), 103–124.

Ortoleva, P. and E. Snowberg (2015). Overconfidence in political behavior. *American Economic Review 105*(2), 504–35.

Phillips, L. D. and W. Edwards (1966). Conservatism in a simple probability inference task. *Journal of Experimental Psychology 72*(3), 346.

Schwardmann, P. and J. van der Weele (2018). Deception and self-deception. Working paper.

Spiegler, R. (2016). Bayesian networks and boundedly rational expectations. *Quarterly Journal of Economics 131*(3), 1243–1290.

Toussaert, S. (2018). Eliciting temptation and self-control through menu choices: A lab experiment. *Econometrica 86*(3), 859–889.

Zimmermann, F. (2020). The dynamics of motivated beliefs. *American Economic Review 110*(2), 337–61.

Appendix

The appendix is organized as follows. Appendix A.1 investigates the convergence of beliefs under different updating biases, and establishes that alignment of elicited posterior with the informed posterior (as in our experiment) is sufficient for the population to become realistic in the long run. Appendices A.2 and A.3 provide additional information on subjects' present bias and the stability of their behavior across time. Appendix A.4 features histograms of prior, posterior and anticipated posterior beliefs, and the relationship between prior and posterior beliefs at the individual level. In Appendix A.5, we provide another construction of informed posterior beliefs using a finer information structure than the binary signal space considered in the paper. Appendix A.6 reports the updating behavior of subjects who stated a misspecified prior at date 1. Appendix A.7 establishes the robustness of results in section 4.4 and Appendix A.8 demonstrates that treatment groups are balanced according to observable characteristics.

A.1 Convergence of posterior beliefs

In this appendix we illustrate the relationship between inferential naiveté and long-run learning in a simple setting. Consider a population of individuals who learn about themselves by observing their effort at any period t of a discrete infinite horizon. Their proclivity to exert effort is either *high* ($\beta = \beta_H$) or *low* ($\beta = \beta_L$), independently across individuals. While the true probability that $\beta = \beta_H$ equals q_0, on average the population initially assigns a (potentially incorrect) probability p_0 to it. At any given date, each individual exerts a high or low effort. Conditional on the individual's type, effort choices are independently and identically distributed across periods. At any date, the probability of a high effort equals σ_H for individuals with $\beta = \beta_H$, and σ_L for individuals with $\beta = \beta_L$, where $0 < \sigma_L < \sigma_H < 1$.

The informed posterior beliefs considered in Section 2 update the population's incorrect prior beliefs with the actual informativeness of behavior. For the individuals who exerted a high effort n times in the first t periods, the informed posterior belief $p_t(n)$ attached to the hypothesis $\beta = \beta_H$ satisfies the following equation:

$$\underbrace{\frac{p_t(n)}{1-p_t(n)}}_{\text{informed posterior LR}} = \underbrace{\frac{p_0}{1-p_0}}_{\text{prior LR}} \underbrace{(\frac{\sigma_H}{\sigma_L})^n(\frac{1-\sigma_H}{1-\sigma_L})^{t-n}}_{\text{actual informativeness}}$$

To model the possibility of inferential naiveté, we also allow the population to deviate from this benchmark by reacting anomalously to high or low effort.

Formally, the average belief of the population after exerting a high effort n times in the first t periods is then:

$$\frac{p_t(n)}{1-p_t(n)} = \frac{p_0}{1-p_0}(\frac{\sigma_H}{\sigma_L})^{\lambda_H n}(\frac{1-\sigma_H}{1-\sigma_L})^{\lambda_L(t-n)}. \tag{3}$$

The parameters λ_H and λ_L characterize the population's reaction to high effort and to low effort, respectively. The case of $\lambda_L = \lambda_H = 1$ corresponds to the informed posterior benchmark without inferential naiveté defined in Section 2. The population is *overreacting* (respectively, *underreacting*) to a signal, if the corresponding λ is larger (respectively, smaller) than 1. We assume that $\lambda_L > 0$ and $\lambda_H > 0$, meaning that the updating is always in the right direction. We are interested in predicting the population's posterior beliefs about future effort choices at the limit when t becomes very large.

Proposition 1 then establishes this convergence as a function of the inferential biases, characterized by (λ_H, λ_L), and of the information structure, characterized by (σ_H, σ_L). We prove that asymptotic learning is incorrect if and only if the population deviates sufficiently from the informed posterior benchmark characterized by $\lambda_H = \lambda_L = 1$. A population who reacts more to good news than to bad news (λ_H/λ_L is sufficiently large) remains naive forever, while a population who reacts more to bad news (λ_H/λ_L is sufficiently small) becomes over-pessimistic at the limit. Between these two cases, a population that updates in line with our informed posterior benchmark forms realistic average beliefs in the long run, irrespective of the bias in its prior beliefs. In the experiment, we do not reject the hypothesis that elicited posterior beliefs are equal to the informed posterior beliefs following either signal. This suggests that the population's beliefs would converge to the true frequencies, provided that participants' updating behavior remains stable over time.

Let

$$\epsilon_L = \frac{(1-\sigma_H)\ln\left(\dfrac{1-\sigma_L}{1-\sigma_H}\right)}{\sigma_H \ln\left(\dfrac{\sigma_H}{\sigma_L}\right)} \quad \text{and} \quad \epsilon_H = \frac{(1-\sigma_L)\ln\left(\dfrac{1-\sigma_L}{1-\sigma_H}\right)}{\sigma_L \ln\left(\dfrac{\sigma_H}{\sigma_L}\right)}.$$

Note that $\epsilon_L < 1 < \epsilon_H$.

Proposition 1. *i) (Near-symmetry) If $\epsilon_L < \lambda_H/\lambda_L < \epsilon_H$, then the population's posterior beliefs are well-calibrated asymptotically.*

ii) (Upward asymmetry) If $\lambda_H/\lambda_L > \epsilon_H$, then the population's posterior beliefs are *(strictly) naive asymptotically.*

iii) (Downward asymmetry) If $\lambda_H/\lambda_L < \epsilon_L$, then the population's posterior beliefs are (strictly) pessimistic asymptotically.

Proof. Let us rewrite Equation 3 as

$$\frac{1}{t}\ln\left(\frac{p_t(n)}{1-p_t(n)}\right) = \frac{1}{t}\ln\left(\frac{p_0}{1-p_0}\right) + \lambda_H\frac{n}{t}\ln\left(\frac{\sigma_H}{\sigma_L}\right) + \lambda_L\big(1-\frac{n}{t}\big)\ln\left(\frac{1-\sigma_H}{1-\sigma_L}\right) \quad (4)$$

By the law of large numbers, the ratio n/t converges almost surely to σ_H (for individuals with type β_H) or to σ_L (for individuals with type β_L). In the former case, the right-hand side of Equation 4 converges to a finite limit equal to

$$\lambda_H\sigma_H\ln\left(\frac{\sigma_H}{\sigma_L}\right) + \lambda_L(1-\sigma_H)\ln\left(\frac{1-\sigma_H}{1-\sigma_L}\right).$$

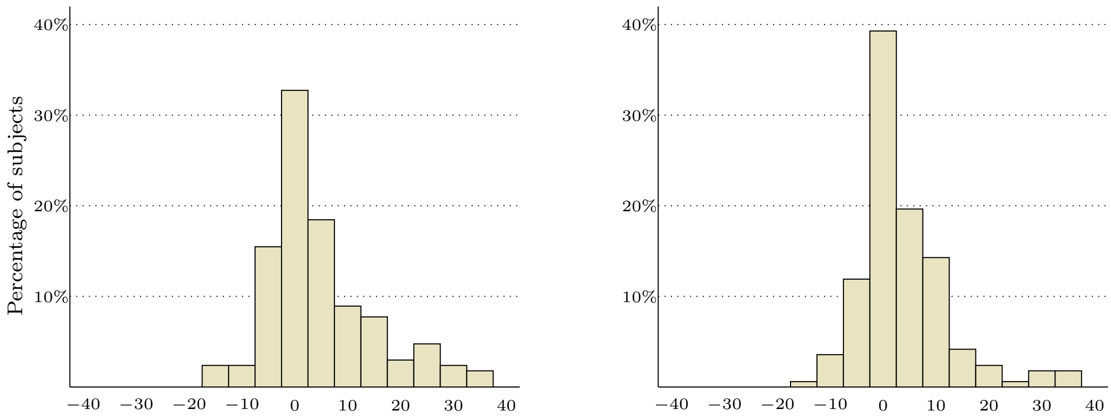
If $\lambda_H/\lambda_L > \epsilon_L$, then this limit is strictly positive, which implies (by Equation 4) that $p_t(n)$ converges to 1, that is, the beliefs converge to the true value of β. If $\lambda_H/\lambda_L < \epsilon_L$, then the limit is strictly negative, which implies that $p_t(n)$ (incorrectly) converges to 0.

Similar arguments show that, if $\beta = \beta_L$, $p_t(n)$ converges to 0 almost surely if $\lambda_H/\lambda_L < \epsilon_H$, and to 1 if $\lambda_H/\lambda_L > \epsilon_H$.

Consider then the case of near-symmetry (item *i*) in the proposition). A fraction q_0 of the population has average posterior beliefs $p_t(n)$ that converge to 1, while a fraction $1-q_0$ has average posterior beliefs that converge to 0. This implies that the average belief about future effort is well-calibrated asymptotically, as the average probability attached to a high effort in the future converges to $q_0\sigma_H + (1-q_0)\sigma_L$, which is the correct value. Under upward asymmetry (item *ii*)), all posterior beliefs converge to 1, which implies asymptotic naiveté. Under downward asymmetry (item *iii*)), all posterior beliefs converge to 0, which implies pessimism. This completes the proof. $\square$

A.2 Present bias

Here we document present bias using the number of screens (between 0 and 40) as our measure of ex-ante choices and on-the-spot effort. At date 1, participants commit to completing an average of 23.94 screens at date 2. But they complete only 19.53 screens, significantly fewer screens than they intended to (two-sided t-test, p-value<0.01). We observe a similar time inconsistency between date 3 and date 4, where participants commit to an average of 20.44 screens in advance, but end up completing only 17.04 screens (two-sided t-test, p-value<0.01). Figure 7 shows histograms of the present bias inherent in the effort choices. Our measure of present bias is obtained by subtracting the actual effort choice from the ex-ante choice. Out of our 168 participants, 79 exhibit at least some present bias between dates 1 and 2 and 75 between dates 3 and 4.



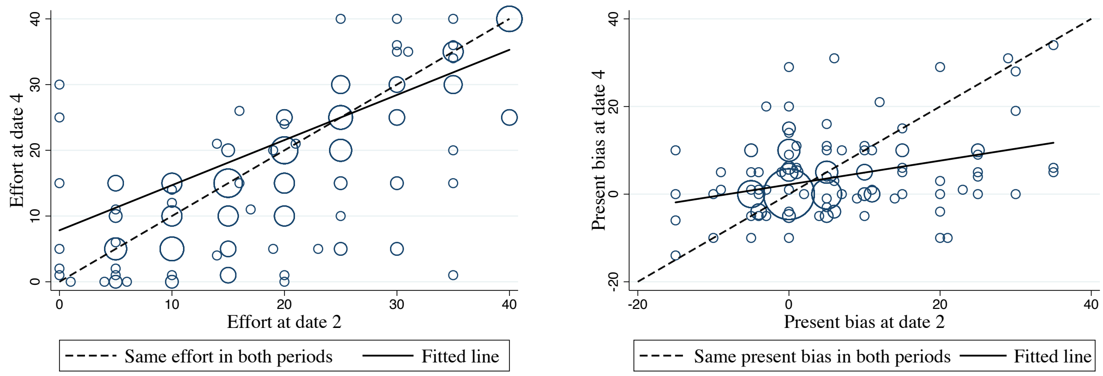
(a) Present bias (dates 1-2) (b) Present bias (dates 3-4)

Figure 7 – Distribution of present bias (ex-ante choice - actual effort).

A.3 Stability of behavior

The stability of behavior can be seen on the scatter plot of effort choice at date 2 and effort choice at date 4 depicted in Figure 8a. The correlation between the number of screens completed at the first work date and the number of screens completed at the second work date is 0.73. Figure 8b exhibits the positive relationship between present bias at both dates. The correlation between the two variables is 0.34.

In the main text we show that effort a_2 is highly predictive of effort a_4. It turns out that present bias between dates 1 and 2 is also predictive of present bias between dates 3 and 4. Indeed, coding present bias as a binary measure equal to 1 if the ex-ante choice is strictly larger than the actual effort, we find that the fraction of subjects who are present-biased between dates 3 and 4 equals 44.6% in the full sample, while it goes up to 55.7% for subjects who exhibit present bias between dates 1 and 2, and down to 34.8% for subjects who are not present-biased between dates 1 and 2. A Fischer's exact test confirms that the probability of present bias between dates 3 and 4 differs between these two subgroups ($p = 0.008$).



(a) Stability of effort

(b) Stability of present bias

Figure 8 – Scatter plot: correlation between behavior at dates 2 and 4

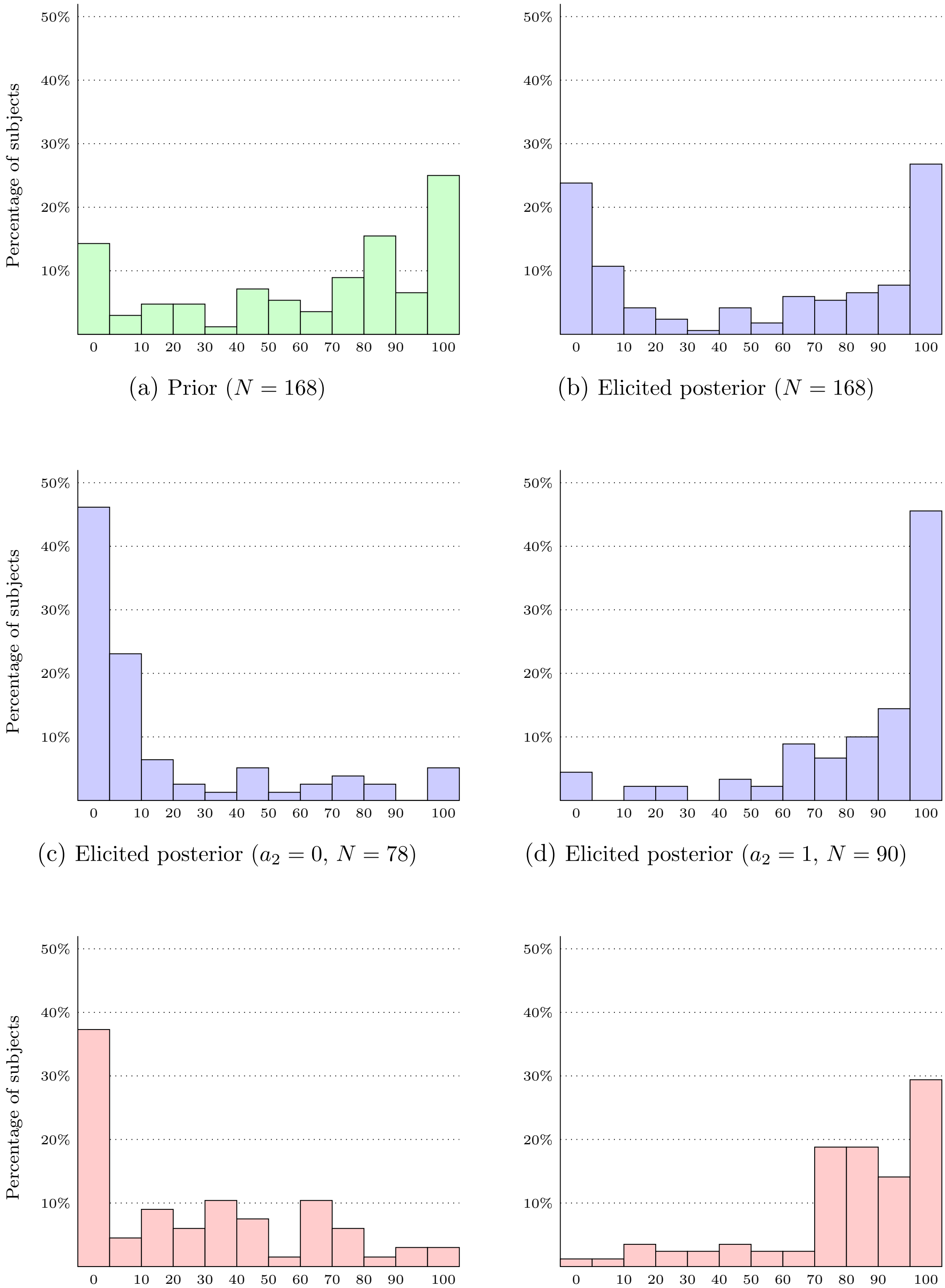
A.4 Distributions of beliefs

Figure 9 displays the distributions of prior, elicited posterior, and anticipated posterior. Figure 10 displays the updating behavior (i.e., both prior and posterior beliefs) at the individual level, as a function of the signal received (high or low effort).

A.5 A finer construction of informed posterior beliefs

The construction of the informed posterior beliefs outlined in Section 2 can be generalized to a finer information structure than the one provided by the binary effort level a_2. That is, for any observable individual-level event E realized at date 2, we can compute the actual likelihood ratio $q(E \mid a_4 = 1)/q(E \mid a_4 = 0)$ and construct the associated informed posterior belief conditional on E. For instance, we could in principle condition posterior beliefs on the exact number of screens completed by the participant at date 2. This would then allow us to retrieve the actual informational content of the precise effort level, which might contain more information about a_4 than the binary effort a_2.

In practice, constructing informed posterior beliefs for the finest possible partition of the signal space would lead to underpowered statistical tests. We therefore partition date 2-effort into four categories only: Very low effort, moderately low effort, moderately high effort, and very high effort. From the empirical frequencies of a_4 in each of these four categories we construct the likelihood ratio of the date 2 effort and combine it with the average prior belief of the population to obtain the informed posterior beliefs. We can thus detect whether the alignment of elicited posterior with informed posterior after $a_2 = 0$ uncovered in section 4.2 masks opposing



(a) Prior ($N = 168$)

(b) Elicited posterior ($N = 168$)

(c) Elicited posterior ($a_2 = 0$, $N = 78$)

(d) Elicited posterior ($a_2 = 1$, $N = 90$)

(e) Anticipated posterior ($a_2 = 0$, $N = 67$)

(f) Anticipated posterior ($a_2 = 1$, $N = 85$)

Figure 9 – Distributions of beliefs about effort at date 4. Belief distributions are described by the weight attached to $a_4 = 1$.

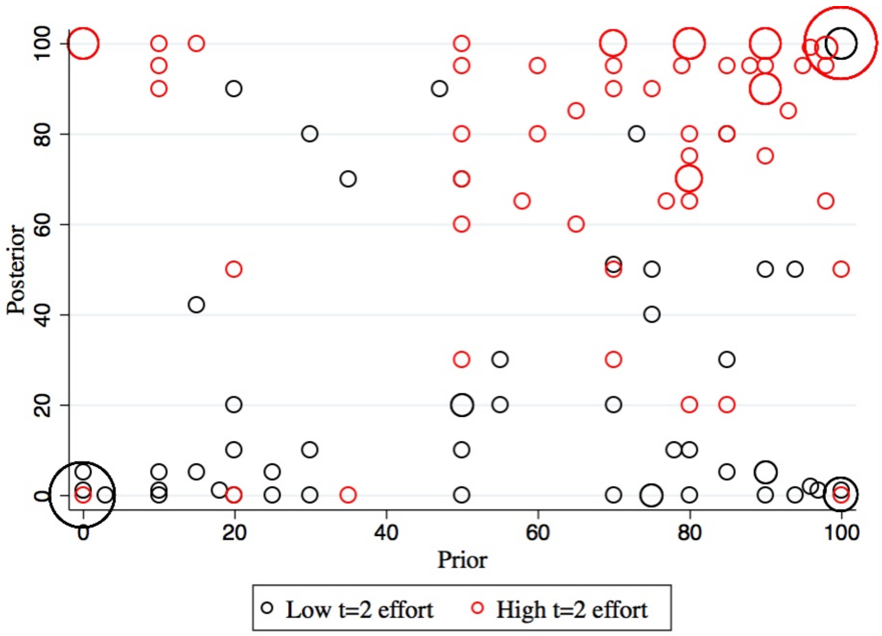


Figure 10 – Scatter plot: Updating behavior at the individual level

anomalies in the reaction to a very low effort and moderately low effort.

For each effort level, Table 7 shows the corresponding likelihood ratio, the conditional frequency of date 4-effort, and compares the elicited posterior beliefs with the informed posterior. Three messages emerge. First, there is indeed more information in this finer partition than in the binary one used in section 4.2. In particular, a very high effort is a much stronger signal of subsequent high effort than a moderately high effort. Second, subjects learn equally well from a moderately high effort and from a very high effort. Third, the insignificant underreaction to a bad signal ($a_2 = 0$) uncovered in section 4.2 is driven by insufficient learning after very low effort. Completing fewer than 10 screens is a very negative signal of one's propensity to exert a high effort at date 4. But following this event, subjects report posterior beliefs which are more optimistic than the informed posterior benchmark. This difference seems quantitatively important, but is not significant, which might be owed to the small sub-sample that this analysis is based on.

A.6 Updating behavior after a zero-probability event

Here we report the updating behavior of the 16 subjects who are excluded from our analysis of learning as their prior beliefs p_1^i assign a probability zero to their actual effort a_2^i. 11 subjects exerted a low effort ($a_2^i = 0$) in spite of initially reporting prior beliefs that satisfy $p_1^i(a_2 = 1) = 100\%$. Their average prior beliefs

Date 2-effort (Number of screens)	Very low (0-9)	Moderately low (10-19)	Moderately high (20-29)	Very high (30-40)
Actual informativeness	0.11	0.14	2.31	7.37
Elicited posterior	20.3	18.6	75.2	94.2
Informed posterior	14.7	18.0	78.6	92.1
Frequency of $a_4 = 1$	7.7	9.6	64	85
Elic. post. - inf. post.	5.6	0.6	-3.4	2.1
p-value	0.389	0.892	0.443	0.324
N	26	52	50	40

Table 7 – Comparison of elicited and informed posterior with a 4-element signal space. P-values derive from a two-sided t-test under the null hypothesis that the difference between elicited and informed posterior is equal to zero.

$p_1(a_4 = 1)$ equal 96.4%, while their average posterior beliefs go down to 27.4%. 5 subjects exerted a high effort ($a_2^i = 1$) in spite of initially reporting $p_1(a_2 = 1) = 0\%$. Their average prior beliefs $p_1(a_4 = 1)$ equal 0%, while their average posterior beliefs equal 80%. In total, 11 of these 16 subjects reported posterior beliefs that put probability one on an effort a_4 equal to their realized effort a_2^i, while 4 subjects stated a posterior belief equal to their prior.

A.7 Quality of predictions

Here we discuss further the issue mentioned in Footnote 20. Consider a rational agent with well-calibrated prior beliefs $p_1(a_2, a_4) = 0.25$ for all (a_2, a_4), and who receives perfect information about a_4 just before date 3. The elicited posterior beliefs of this agent are more precise than her anticipated posterior beliefs, even though this agent has rational expectations. The issue is that the information received before date 3 is uncorrelated with a_2 and can therefore not be reflected in the prior beliefs.

To deal with this issue and provide a better comparison of the mistakes implied by the anticipated and actual learning, we eliminate any information orthogonal to a_2 in the elicited posterior beliefs. That is, we calibrate two hypothetical likelihood ratios, $LR^h(a_2 = 1)$ and $LR^h(a_2 = 0)$, such that, if all subjects in the subgroup a_2 updated their prior beliefs with the (common) likelihood ratio $LR^h(a_2)$, their resulting posterior beliefs would be equal on average to their elicited posterior beliefs. We then replace the elicited posterior belief of each subject by the posterior belief constructed from the prior of this subject and the calibrated likelihood ratio, which we call the *common LR posterior*, and we compute the associated mistake at the individual level. This procedure eliminates the information orthogonal to a_2 by assuming that the rate of updating is the same for all subjects who exerted the same effort level a_2.

Table 8 shows the average mistake implied by the different beliefs. The findings confirm that subjects' learning from a_2 itself is better than their anticipated learning from a_2, as the mistake implied by the common LR posterior is smaller than the mistake implied by the anticipated posterior. These results confirm that participants learn well from their experience ex post but that their prior beliefs underestimate the magnitude of the information contained in a_2.

Mistake 1		**Mistake 2**		**Difference**	**p-value**
Prior	31.7	Ant. post.	27.9	3.8	0.003
		Common LR. post.	23.6	8.1	<0.001
		Elic. post.	18.4	13.3	<0.001
Ant. post.	27.9	Common LR. post.	23.6	4.2	0.003
		Elic. post.	18.4	9.5	<0.001
Common LR. post.	23.6	Elic. post.	18.4	5.2	0.02

Table 8 – Mistakes implied by the different beliefs. For all comparisons a two-sided t-test was used.

A.8 Baseline balance across treatment groups

Variable	**Same Tasks** ($N = 79$)	**Different Tasks** ($N = 73$)	**Difference**	**p-value**
Binary effort (date 2)	53.1	58.9	-5.7	0.516
Continuous effort (date 2)	19.7	20.9	-1.2	0.490
Female	64.6	61.6	2.9	0.738
Age	24.7	24.0	0.7	0.492
Mathematics score	4.2	3.9	0.3	0.656

Table 9 – Baseline balance across treatment groups. For the comparison of binary effort choice and gender (a binary variable equal to 1 for a female participant) the p-value is based on Fischer's exact test, for all other comparisons a two-sided t-test was used. The mathematics score was measured by the average mathematics grade in high school.

Online Appendix: Experimental Instructions

The following instructions apply to a participant in the Different Tasks condition who is not committed by the choices made in sessions 1 and 3. The complete decision environments with figures can be found on a tourist version of the experimental website at `https://www.lsc-experiment.com`. All instructions are translated from German.

O.1 Initial session

O.1.1 General instructions (1/2)

Thank you for participating in the experiment. We will now give you more detailed information about the experiment. Please read these instructions thoroughly and raise your hand if you have any question.

Schedule The initial meeting, which is taking place now in the lab, will last approximately one hour. The experiment involves 4 additional online sessions:

- Session 1 will take place on 13.06.2018 and will last approximately 15 minutes.
- Session 2 will take place on 18.06.2018. You will be free to decide how long to work on that session (minimum: 1 minute).
- Session 3 will take place on 20.06.2018 and will last approximately 10 minutes.
- Session 4 will take place on 26.06.2018. You will be free to decide how long to work on that session (minimum: 5 minutes).

Participating in all sessions is mandatory.

Online sessions To participate in an online session you must log in to the experimental platform during the day and follow the instructions. Your login information is your email address and a secret password that you will choose later today. The platform will be available from 00:00 to 24:00 at the following address: www.lsc-experiment.com. Should you have any question or encounter any technical difficulty, please send an email to the following address: contact@lsc-experiment.com.

Reminders It is your responsibility to remember to log in for every online session. We will send you some emails to help you. An email sent today will include all the information provided on this page (schedule, address of the platform and contact email address). Please save this email and mark your calendar with the dates of the sessions. You can also take notes on the sheet of paper placed on your desk. On the day of each online session you will also receive some reminder emails from us.

Rules and technical requirements You should be alone in front of your computer every time you participate in a session. You should use a desktop or laptop equipped with Google Chrome, Mozilla Firefox oder Safari (not Internet Explorer). The website is not suitable for smartphones and tablets, as you will need a proper mouse and keyboard (or touchpad) for some tasks. Please do not reload the webpage, do not use the backwards button, and do not stay inactive on a page for more than 20 minutes. This would finish your session and your participation would be lost.

O.1.2 General instructions (2/2)

Payment At the end of the experiment you will receive your payment by bank transfer. The bank transfer will be ordered between 27.06.2018 and 30.06.2018, and the money should be available on your bank account one or two days later. Your earnings will only depend on your own decisions. Decisions made by other participants will not influence your earnings.

Bonus payment It is mandatory to participate in all sessions in order to receive a payment. If you validate all sessions, you will receive:

- A baseline fee of 25 Euro, independently of your answers.
- On top of the baseline fee, a bonus fee between 0 and 35 Euro, which will depend on your decisions.

Missed sessions If you fail to validate one of the sessions—for instance, if you forget to log in to the experimental platform or if you do not complete all mandatory tasks for that date—you will not receive any payment, irrespective of your earnings made in other sessions. In this case you will receive an email informing you that you have been excluded from the experiment. By registering to the experiment you commit to participating in all sessions.

O.1.3 Today's session

Today's session will last approximately one hour and is divided into three sections.

Section 1: registration form We will first ask you a few questions about yourself (name, email address, etc.) and generate your login information for the experimental platform.

Section 2: computerized tasks In sessions 2 (on 16.06.2018) and 4 (on 26.06.2018) of the experiment you will have the opportunity to increase your bonus by working on some computerized tasks. We will present the tasks to you today, show you some examples and ask you to perform the tasks a few times to get familiar with them.

Section 3: explanation of a payment mechanism In this section we will explain to you a mechanism that will be used later in the experiment to ask you to estimate the likelihood of a future event.

O.1.4 Section 1: registration

Registration form (Ms / Mr, first name, last name, email address, password for the website).

O.1.5 Section 2

Computerized tasks In sessions 2 and 4 you will have the opportunity to increase your bonus by working on a computerized task. On these dates you will have to log in to the experimental platform to validate your participation. You will then be free to decide how long to work on the task to increase your bonus. You will have the opportunity to work on one of the following tasks: the sliders task, or the counting zeroes task.

The task(s) that will be offered to you in session 2 and in session 4 will be chosen randomly. You will be informed today of the task(s) which has been chosen for you. Before then, we will introduce both tasks to you.

Let us start with the first task.

Sliders The sliders task consists of a sequence of screens with 40 sliders each. Each slider is associated with a target number between 0 and 100. The task consists of positioning every slider on its target number with the mouse, the keyboard or the touchpad. Every slider is initially positioned on 50.

Example A screen looks like the following example: *here, a screen of 40 sliders incorrectly positioned.*

To complete the screen you must position each slider on its target, as follows: *here, the screen with all sliders correctly positioned.*

You will see 40 sliders on every screen. Once you have correctly positioned all sliders please click on “Submit”. Be careful: if you click on “Submit” while some sliders are not correctly positioned, the webpage will be reloaded automatically and you will have to start again.

Practice screens To get familiar with the task we will now ask you to complete 5 screens with 40 sliders each. Please call the experimenter if you have any question. Otherwise please click on “Proceed”: the next webpage will display the first practice screen.

O.1.6 Practice screens

This is screen number 1. You need to complete 5 screens of sliders today.

Please position all sliders and click on “Submit” when you are done.

Be careful! If you click on “Submit” while some sliders are not correctly positioned, the webpage will be automatically reloaded and you will have to start again.

Here, the screen with 40 sliders.

O.1.7 Second task

We will now introduce the second task: counting zeroes. In this task you must count the number of zeroes in a table of ones and zeroes.

Example A screen looks like the following example. The screen contains 10 tables. Every table contains 40 numbers. *Here, a screen with 10 tables of 40 numbers each.*

To complete one screen you must count the number of zeroes in each of the 10 tables, report it in the corresponding text area, and click on “Submit”. Be careful: if you click on “Submit” while some of your answers are incorrect, the webpage will be automatically reloaded and you will have to start again.

To get familiar with the task we will now ask you to complete 5 screens with 10 matrices each. Please call the experimenter if you have any question. Otherwise please click on "Proceed": the next webpage will display the first practice screen.

O.1.8 Practice screens

This is screen number 1. You need to complete 5 screens today.

Please count the number of zeroes in each table and report it in the corresponding text area, and click on "Submit" when you are done.

Be careful! If you click on "Submit" while some of your answers are incorrect, the webpage will be automatically reloaded and you will have to start again.

Here, the screen with 10 tables.

O.1.9 Section 2

Online sessions We will now give you more information about sessions 2 and 4.

In session 2 (on 18.06.2018) you will have the opportunity to work on the sliders task in order to increase your bonus. In session 4 (on 26.06.2018) you will have the opportunity to work on the counting-zeroes task in order to increase your bonus.

On 18.06.2018 the website will contain 40 screens with 40 sliders each. On 26.06.2018 the website will contain 40 screens with 10 tables each. You do not have to complete all screens. You will be free to choose whether:

- you leave the platform directly after logging in, thereby completing 0 screens, or;
- you start the task, complete a given number of screens, and then leave the platform, or;
- you complete all 40 screens.

Every screen will contain a button "Submit" to submit your answers and a button "Finish" to terminate the session.

As soon as you submit a screen with correct answers, this screen counts as completed. You will be rewarded for every batch of 5 screens that you complete, even if you need several attempts for one (or several) of these 5 screens.

For the first batch of 5 screens you receive 5 Euros. For screens 6-10 you receive 4 Euros. Hence, if you complete 10 screens you will receive 9 Euros. The following table displays the bonus associated with every batch of 5 screens, as well as the cumulative earnings. This table will be shown to you again every time it is relevant for your decisions.

Here, Table 1.

O.1.10 Section 3: payment mechanism

Payment mechanism for reporting your estimation In the course of the experiment we will ask you your estimation of the likelihood with which an uncertain event will occur. The likelihood that you will report will influence your earnings. All the payment rules are designed in such a way that you will maximize your chances of earning 3 Euros if you give your best estimation. Please report in all cases the likelihood with which you truly believe that the event will occur.

In the following we will explain you the payment rules. We take as an example the event "Germany wins over Mexico in the group stage of the Football World Cup". This example is for illustrative purposes only and will be replaced by other events in the experiment.

Please report the likelihood (in percentages) with which you believe that Germany will win over Mexico.

Answer: %. (Please use an integer number, e.g. 1, 2, 3, , 99, 100)

After you report your answer, the computer will generate a random integer between 0 and 100. Every integer between 0 and 100 will be chosen with the same probability. Let us call this number X.

- If the likelihood that you report is larger than X, then you will receive 3 Euros if Germany wins over Mexico.
- If the likelihood that you report is smaller than X, then you will receive 3 Euros with probability X

According to these rules, it is always beneficial for you to report the likelihood that you truly believe.

Suppose for instance, that you believe that Germany will win over Mexico with probability 62%. Instead of reporting 62%, suppose that you report a likelihood of 50%. Then it can happen that the computer selects X=55. In this case you will receive 3 Euros with probability 55%. If you had reported 62% instead, you would have earned 3 Euros with probability 62%, that is, if and only if Germany wins.

Imagine now that you report a likelihood of 69%. Then it can happen that the computer selects X=66. In this case you will earn 3 Euros with probability 62%, that is, if Germany wins. If you had truthfully reported your likelihood of 62%, then you would have received 3 Euros with probability 66% instead.

Questions To check your understanding of the mechanism we will now ask you two control questions. Your answers have no influence on your earnings in the experiment, but we will proceed only when you have answered all questions correctly.

Question 1: Suppose that you believe that Germany will win over Mexico with probability 75%. Which likelihood should you report in order to maximize your chances of earning 3 Euros?

- 50%
- 75%
- 100%

Question 2: Suppose now that we are interested in the likelihood of the following scenarios:

- Scenario a) Germany wins over Mexico and Sweden.
- Scenario b) Germany wins over Mexico but not over Sweden.
- Scenario c) Germany does not win over Mexico but wins over sweden.
- Scenario d) Germany neither wins over Mexico nor over Sweden.

You must report the likelihood of all four scenarios. After you report your estimation, the computer will select one scenario and implement the mechanism described above. You know that one (and only one) of these scenarios will happen. Suppose that you believe that the likelihood of Scenario a) is 50%, that the likelihood of Scenario b) is 10 %, and that the likelihood of Scenario c) is 20 %.

Which answer will maximize your chances of earning 3 Euros?

- a) 25% b) 25% c) 25% d) 25%
- a) 50% b) 10% c) 20% d) 10%
- a) 50% b) 10% c) 20% d) 20%
- a) 100% b) 0% c) 0% d) 0%

O.1.11 Today's session is over

This is the end of the initial session. Thank you for your participation. Please contact the experimenter if you have any question. Otherwise, please leave the lab silently. You should have received an email from us with all the relevant information about the future online sessions. Please contact us if you have not received this email.

O.2 Session 1

O.2.1 Welcome

You successfully logged on to participate in session 1 of the experiment. Please read the following instructions carefully. You will be informed when your participation in today's session is validated.

Please do not reload the webpage, do not use the backwards button, and do not stay inactive on a page for more than 20 minutes.

As explained to you in the laboratory, the experiment will give you the opportunity to work on the sliders task in session 2 (on 18.06.2018) and on the counting zeroes task in session 4 (on 26.06.2018). The two tasks correspond to the practice screens that you completed in the laboratory. On 18.06.2018 the website will contain 40 screens of sliders; each screen will contain 40 sliders to be positioned. On 26.06.2018 the website will contain 40 screens with tables of ones and zeroes; each screen will contain 10 tables with 40 numbers each, and you will have to count the number of zeroes in each table.

As a reminder, a slider looks as follows: *here, a slider.*

A table looks as follows: *here, a table.*

We will now remind you of the payment rules for these tasks. Each screen will comprise a button "Submit" to submit your current answers, and a button "Finish" to leave the task and terminate the session. As soon as you submit a screen with correct answers, this screen counts as completed. You will be rewarded for every batch of 5 screens that you complete, even if you need several attempts for one (or several) of these 5 screens.

For the first batch of 5 screens you receive 5 Euros. For screens 6-10 you receive 4 Euros. Hence, if you complete 10 screens you will receive 9 Euros. The following table displays the bonus associated with every batch of 5 screens, as well as the cumulative earnings. This table will be shown to you again every time it is relevant for your decisions.

Here, Table 1.

There is an additional rule. Today we will already ask you how many screens of sliders (for session 2) and how many screens of tables (for session 4) you would like to complete. Then the computer will choose randomly which of the following payment rules will apply.

- Rule 1 ("Binding") — with probability 5% : The decision that you make today will be binding. This means that you will not be able to complete more screens in the corresponding session that the number you choose today, and that you will not receive any bonus for that session if you complete fewer screens than you decide today. Example: Suppose that you decide today to complete 10 screens on 18.06.2018 and 15 screens on 26.06.2018. If Rule 1 applies, this means that you will have to complete exactly 10 screens of sliders on 18.06.2018 and exactly 15 screens of tables on 26.06.2018. If you complete fewer than 10 screens in session 2, you will not receive any earnings for that session. If you complete 10 screens in session 2, you will receive the bonus corresponding to 10 screens. You will not have the opportunity to complete more than 10 screens in session 2. Similarly, if you complete fewer than 15 screens in session 4, you will not receive any earnings for that session. If you complete 15 screens in session 4, you will receive the bonus corresponding to 15 screens. You will not have the opportunity to complete more than 15 screens.

- Rule 2 ("Free") — with probability 95% : The decision that you make today will not be binding. This means that you will be free to choose how many screens to complete on 16.06.2018 and 26.06.2018 irrespective of the decision you make today. You will then be paid for each batch of 5 screens completed according to the payment scheme described above.

You will report your decision on the next screen, and we will then immediately inform you of the rule (1 or 2) selected by the computer.

Questions To check your understanding and before we ask you to report your decision, we will ask you three questions. Your answers have no influence on your earnings, but we will proceed to the next screen only when you answer all questions correctly. *Note: the numbers of screens used in the example were randomized.*

To answer the next questions, suppose that you decide today to complete 10 screens in session 2 and 15 screens in session 4.

Suppose that the computer selects Rule 1 ("Binding") and you complete 8 screens on 16.06.2018. How much will you earn for session 2?

- 5 Euros
- 0 Euro

Suppose that the computer selects Rule 2 ("Free") and you complete 8 screens on 16.06.2018. How much will you earn for session 2?

- 5 Euros
- 0 Euro

Suppose that the computer selects Rule 1 ("Binding") and you complete 15 screens on 16.06.2018. How much will you earn for session 2?

- 9 Euros
- 12 Euros
- It will not be possible to complete 15 screens.

O.2.2 Choose your number of screens

For the case where your decision today is biding (5% probability), please indicate below how many screens of sliders you want to complete in session 2 and how many screens of tables you want to complete in session 4. The following table reminds you of the payment for every batch of 5 screens.

- Choose your number of screens with sliders for session 2 (minimum 0 and maximum 40):
- Choose your number of screens with tables of ones and zeroes for session 4 (minimum 0 and maximum 40):

Here, Table 1.

O.2.3 Your estimation

The computer has chosen Rule 2 ("free"). You will thus choose how many screens to complete in sessions 2 and 4 regardless of the decision you just made.

Your estimation We will now ask you to report four estimates. These estimates are related to the likelihood with which you think you will complete a certain number of screens in session 2 (on 16.06.2018) and in session 4 (on 26.06.2018).

Payment rule: with probability 50%, your answers will be payoff-relevant. In this case the computer will select one of the four scenarios randomly, and you will receive the chance to earn 3 Euros for this answer following the payment mechanism introduced in the laboratory. You maximize your chances of earning 3 Euros if you report your best estimation of the likelihood for all four questions.

Remember that you will have the opportunity to work on the sliders task in session 2 and on the counting zeroes task in session 4.

The following table describes four scenarios. Each scenario describes a number of screens that you complete in session 2 and a number of screens that you complete in session 4. Please imagine yourself in both sessions and think about the likelihood that you will complete 20 screens or more in the corresponding session. Then, think about how likely you are to complete 20 screens or more in both sessions, in only one of the sessions, or in none of the two sessions.

Please indicate in the last column of the table how likely you think that each of the four scenarios will occur. Please choose a likelihood between 0 and 100 percent for each scenario and make sure that the sum of your answers equals 100.

Here, the table displayed in Figure 3 where the order of the scenarios was randomized, and Table 1 below.

O.2.4 Payoff relevance

On the last screen we informed you that your estimation would be payoff-relevant with probability 50%. The computer flipped a "digital coin" to determine whether this is the case: it turns out that your estimation is payoff-relevant.

O.2.5 Session 1 is over

Session 1 is over. Thank you for your participation. The schedule of the experiment is displayed below. You can close this window.

- Session 1 took place today. You have validated this session.
- Session 2 will take place on 18.06.2018.
- Session 3 will take place on 20.06.2018.
- Session 4 will take place on 26.06.2018.

O.3 Session 2

O.3.1 Welcome

You successfully logged on to participate in session 2 of the experiment. Please read the following instructions carefully. You will be informed when your participation in today's session is validated.

Please do not reload the webpage, do not use the backwards button, and do not stay inactive on a page for more than 20 minutes.

Today you have the opportunity to increase your bonus by completing some screens with the sliders task. Each of the following screens contains 40 sliders. To complete one screen you must position each slider on its target and click on "Submit". Be careful! If you click on "Submit" while some sliders are incorrectly positioned the webpage will be reloaded with a new set of sliders and you will have to start again.

As soon as you successfully complete one screen (without any mistake) this screen will be validated. You will be rewarded for every batch of 5 screens that you complete even if you need several attempts for one or several of these screens. The following table reminds you of the payment associated with every batch of 5 screens as well as the cumulative payment. This table will be displayed on every screen. The screen number (between 1 and 40) and your earnings so far will also be displayed on the screen. You can freely decide how many screens you complete today (between 0 and 40). Every screen comprises a button "Finish" to leave the task. If you click on "Finish" your earnings so far will be added to your bonus earnings for the experiment.

Here, Table 1.

Next week (on 26.06.2018) you will have the opportunity to increase your bonus again by completing some screens with the counting-zeroes task.

Please click on "Proceed" when you are ready to start the task.

O.3.2 Task

This is screen number 1. You have earned 0 Euros so far.

Please position each slider on its target and click on "Submit" when you are ready, or on "Finish" if you want to leave the task.

When you click on "Finish", your participation will be validated and your bonus earnings for today will equal Euros.

Be careful! If you click on "Submit" while some sliders are incorrectly positioned, the webpage will be reloaded with a new set of sliders and you will have to complete this screen again.

Here, a button entitled "Show the payment table" to display Table 1, and the screen with 40 sliders.

O.3.3 Session 2 is over

Session 2 is over. Thank you for your participation. The schedule of the experiment is displayed below. You can close this window.

- Session 1 took place on 13.06.2018. You have validated this session.
- Session 2 took place today. You have validated this session.
- Session 3 will take place on 20.06.2018.
- Session 4 will take place on 26.06.2018.

O.4 Session 3

O.4.1 Welcome

You successfully logged on to participate in session 3 of the experiment. Please read the following instructions carefully. You will be informed when your participation in today's session is validated.

Please do not reload the webpage, do not use the backwards button, and do not stay inactive on a page for more than 20 minutes.

As explained to you already, the experiment will give you the opportunity to work on the counting zeroes task in session 4 (on 26.06.2018). The website will contain 40 screens with tables of ones and zeroes; each screen will contain 10 tables with 40 numbers each, and you will have to count the number of zeroes in each table.

As a reminder, a table looks as follows: *here, a table with 40 numbers.*

We will now remind you of the payment rules for these tasks. Each screen will comprise a button "Submit" to submit your current answers, and a button "Finish" to leave the task and terminate the session. As soon as you submit a screen with correct answers, this screen counts as completed. You will be rewarded for every batch of 5 screens that you complete, even if you need several attempts for one (or several) of these 5 screens.

For the first batch of 5 screens you receive 5 Euros. For screens 6-10 you receive 4 Euros. Hence, if you complete 10 screens you will receive 9 Euros. The following table displays the bonus associated with every batch of 5 screens, as well as the cumulative earnings. This table will be shown to you again every time it is relevant for your decisions.

Here, Table 1.

There is an additional rule. Today, we will already ask you how many screens of tables (for session 4) you would like to complete. Then the computer will choose randomly which of the following payment rules will apply. You have already seen these rules in session 1.

- Rule 1 ("Binding") — with probability 5% : The decision that you make today will be binding. This means that you will not be able to complete more screens in session 4 than the number you choose today, and that you will not receive any bonus for that session if you complete fewer screens than the number you choose today. Example: Suppose that you decide today to complete 10 screens on 26.06.2018. If Rule 1 applies, this means that you will have to complete exactly 10 screens of the counting zeroes task on 26.06.2018. If you complete fewer than 10 screens you will not receive any earnings for that session. If you complete 10 screens you will receive the bonus corresponding to 10 screens. You will not have the opportunity to complete more than 10 screens.

- Rule 2 ("Free") — with probability 95% : The decision that you make today will not be binding. This means that you will be free to choose how many screens to complete on 26.06.2018 irrespective of the decision you make today. You will then be paid for each batch of 5 screens completed according to the payment scheme described above.

You will report your decision on the next screen, and we will then immediately inform you of the rule (1 or 2) selected by the computer.

Questions Before we ask you to report your decision, we will ask you three questions to check your understanding. Your answers have no influence on your earnings, but we will proceed to the next screen only when you answer all questions correctly. *Note: the number of screens used in the example was randomized.*

To answer the next questions, suppose that you decide today to complete 10 screens in session 4.

Suppose that the computer selects Rule 1 ("Binding") and you complete 8 screens on 26.06.2018. How much will you earn for session 4?

- 5 Euro

- 0 Euro

Suppose that the computer selects Rule 2 ("Free") and you complete 8 screens on 26.06.2018. How much will you earn for session 4?

- 5 Euro
- 0 Euro

Suppose that the computer selects Rule 1 ("Binding") and you complete 15 screens on 26.06.2018. How much will you earn for session 4?

- 9 Euro
- 12 Euro
- It will not be possible to complete 15 screens.

O.4.2 Choose your number of screens

For the case where your decision today is biding (5% probability), please indicate below how many screens of the couting zeroes task you want to complete in session 4. The following table reminds you of the payment for every batch of 5 screens.

Choose your number of screens with tables of ones and zeroes for session 4 (minimum 0 and maximum 40):

Here, Table 1.

O.4.3 Your estimation

The computer has chosen Rule 2 ("free"). You will thus choose how many screens to complete in session 4 regardless of the decision you just made.

We will now ask you to report two estimates. These estimates are related to the likelihood with which you think you will complete a certain number of screens in session 4 (on 26.06.2018).

Payment rule: with probability 50% your answers will be payoff-relevant. In this case the computer will select one of the two scenarios randomly and you will receive the chance to earn 3 Euros for this answer following the payment mechanism introduced in the laboratory. You maximize your chances of earning 3 Euros if you report your best estimation of the likelihood for all four questions.

Remember that you will have the opportunity to work on the counting zeroes task in session 4.

The following table describes two scenarios. Each scenario describes a number of screens that you complete in session 4. Please imagine yourself in that session and think about the likelihood that you will complete 20 screens or more.

Please indicate in the last column of the table how likely you think that each of the two scenarios will occur. Please choose a likelihood between 0 and 100 percent for each scenario and make sure that the sum of your answers equals 100.

Here, a table similar to the one displayed in Figure 3 but with only two scenarios, in a random order.

O.4.4 Payoff relevance

On the last screen we informed you that your estimation would be payoff-relevant with probability 50%. The computer flipped a "digital coin" to determine whether this is the case: it turns out that your estimation is not payoff-relevant.

O.4.5 Session 3 is over

Thank you for your participation. The schedule of the experiment is displayed below. You can close this window.

- Session 1 took place on 13.06.2018. You have validated this session.
- Session 2 took place on 18.06.2018. You have validated this session.
- Session 3 took place today. You have validated this session.
- Session 4 will take place on 26.06.2018.

O.5 Section 4

O.5.1 Welcome

You successfully logged on to participate in session 4 of the experiment. Please read the following instructions carefully. You will be informed when your participation in today's session is validated.

Please do not reload the webpage, do not use the backwards button, and do not stay inactive on a page for more than 20 minutes.

Today's session Today you have the opportunity to increase your bonus by completing some screens with the counting zeroes task. Each of the following screens contains 10 tables. Each table comprises 40 numbers. To complete one screen you must count the number of zeroes in each table, report it in the associated text area, and click on "Submit". Be careful! If you click on "Submit" while some answers are incorrect the webpage will be reloaded with a new set of tables and you will have to start again.

As soon as you successfully complete one screen (without any mistake) this screen will be validated. You will be rewarded for every batch of 5 screens that you complete even if you need several attempts for one or several of these screens. The following table reminds you of the payment associated with every batch of 5 screens as well as the cumulative payment. This table will be displayed on every screen. The screen number (between 1 and 40) and your earnings so far will also be displayed on the screen. You can freely decide how many screens you complete today (between 0 and 40). Every screen comprises a button "Finish" to leave the task. If you click on "Finish" your earnings so far will be added to your bonus earnings for the experiment.

Here, Table 1.

Please click on "Proceed" when you are ready to start the task.

O.5.2 Task

This is screen number 1.

You have earned 0 Euros so far.

Please count the number of zeroes in each table and report it in the associated text area. click on "Submit" when you are ready, or on "Finish" if you want to leave the task.

When you click on "Finish", your participation will be validated and your bonus earnings for today will equal 0 Euros.

Be careful! If you click on "Submit" while some answers are incorrect, the webpage will be reloaded with a new set of tables and you will have to complete the screen again.

Here, a button entitled "Show the payment table" to display Table 1, and the screen of 10 matrices.

O.5.3 Survey

The experiment is almost over. You only need to fill out a short survey. Your answers will not have any influence on your earnings. Please answer all questions honestly.

How old are you?

What is your religion?

- Catholic.

- Protestant.
- Muslim.
- Other:
- I don't have any religion.

What is your occupation?

In case you are a student, what is your field of study?

What was your last grade in math in high school (e.g. at the A-level)?

What is your citizenhip?

What is the approximate annual income of your household?

How many people live in your household?

In the past weeks, we asked you several times to report the likelihood with which you believed you would complete more than a given number of screens in the future. What did you think when you answered these questions?

- I tried to report my best estimate.
- I did not think much of it and reported whatever answer came to my mind.
- I reported a lower likelihood than the one I had in mind.
- I reported a larger likelihood than the one I had in mind.

Please justify your answer:

O.5.4 Feedback

The experiment is almost over. We would like to allow you to give your feedback about this experiment. Were the instructions and rules easy to understand? Or did you find some points confusing? Please give your feedback in the text area below. This is not mandatory and you can leave this space blank if you prefer. Your feedback will have no influence on your earnings.

O.5.5 The experiment is over

Thank you for your participation. Overall you have earned:

- 25 Euros as a participation fee;
- 6 Euros with the payment mechanism for your estimations;

- 9 Euros for your work on the sliders in session 2;
- 12 Euros for your work on the counting zeroes task in session 4.

Thus you have earned 52 Euros in total. The bank transfer will be ordered in the next 48 hours. You can close this window.