RATIONALITY & COMPETITION
CRC TRR 190

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Discussion Paper No. 471

December 03, 2023

Collaborative Research Center Transregio 190 | www.rationality-and-competition.de
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Abstract

Traditionally, economic models have attributed procrastination to present bias. However, procrastination may also arise when individuals derive anticipatory utility from holding motivated, overly optimistic beliefs about the workload they need to complete. This study provides a rigorous empirical test for this notion of 'motivated procrastination'. In a longitudinal experiment over four weeks, individuals have to complete a cumbersome task of unknown length. They are exposed to exogenous variation in i) their expectation regarding their workload and ii) scope for motivated reasoning. We find that scope for motivated reasoning allows workers to hold substantially more optimistic beliefs and identify a causal link between the exogenous variation in beliefs and the deferral of work to the future. This systematic belief-based delay of work (motivated procrastination) turns out to be robust to accounting for decision-makers' time preferences and emotional responses, and looms largest for decision makers who tend to not acquire information that may include negative news.

JEL codes: C91, D83, D84, D90, D91
Keywords: anticipatory utility, beliefs, memory, motivated cognition, procrastination, real effort, task allocation

*We thank Andrea Amelio, Peter Andre, Kai Barron, Valeria Burdea, Alexander Coutts, Christoph Drobner, Urs Fischbacher, Tilman Fries, Katrin Goedker, Sebastian Goerg, Thomas Graeber, Hans-Peter Grüner, Martin Haas, Boon Han Koh, Orestis Kopsacheilis, Spencer Kwon, Giacomo Lanzani, Johannes Maier, Pierfrancesco Mei, Friederike Mengel, Zahra Murad, Frieder Neunhoeffer, Davide Pace, Cameron Peng, Marcel Quint, Peter Schwardmann, Frederik Schwerter, Sonja Settele, Alice Solda, Michael Thaler, Heidi Thysen, Johannes Wohlfart, Yves Le Yaouanq, Sili Zhang, and Florian Zimmermann for helpful comments and suggestions. We are also grateful for helpful suggestions which we received from participants of the 2023 ESA World Meeting in Lyon, the SABE-IAREP conference 2023 in Nice, the Workshop on Narratives, Beliefs, and Memory in Herrsching, the Copenhagen Summer School on Beliefs, the Interdisciplinary Workshop on Motivated Beliefs in Raitenhaslach, and various research seminars. Financial support by Deutsche Forschungsgemeinschaft through CRC TRR 190 (project number 280092119) is gratefully acknowledged.

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1 Introduction

Procrastination can engender substantial detrimental consequences for individuals (Akerlof, 1991). People may fail to accumulate optimal levels of savings (Thaler and Benartzi, 2004), plan but fail to exercise (Della Vigna and Malmendier, 2006), or misallocate work load across time (Ariely and Wertenbroch, 2002). While traditionally, economic models have understood procrastination as a result of time inconsistent preferences or present bias (O'Donoghue and Rabin, 1999; Augenblick et al., 2015; Augenblick and Rabin, 2019), more recently, belief-based explanations for procrastination have been proposed (Brunnermeier et al., 2017; Breig et al., 2023). These models feature the general notion that agents may hold overly optimistic beliefs about future workload to enjoy anticipatory utility (when the total workload is uncertain) but then fall prey to the planning fallacy (Kahneman and Tversky, 1982), as they systematically reduce effort in the present, and consequently have to complete many tasks only shortly before important deadlines at high marginal costs (or even miss deadlines). While belief-based explanations for procrastination are appealing and relevant for welfare considerations, empirical evidence on whether and how agents sustain such overly optimistic beliefs in work environments is missing. In particular, it appears crucial to better understand how agents can form and sustain overly optimistic beliefs when they receive informative signals about their workload, and whether such beliefs indeed result in systematic delay of work.

In this project, we study empirically whether the systematic delay of work to the future can solely result from agents holding motivated, overly optimistic beliefs about the total workload they will encounter. We term this belief-based delay 'motivated procrastination', as it simply results from convex effort costs and motivated beliefs about the total workload but does not require any suboptimal allocation decision conditional on agents' (wrong) beliefs. Based on overly optimistic beliefs about their total workload, agents complete fewer tasks in the present and plan to do so in the future. While conditional on the agent's belief, this allocation choice is rational, the agents decision to reduce effort in the present eventually results in higher than expected workload to be completed in the future, shortly before the deadline because the agent's beliefs about their total workload were overly optimistic.

Our empirical approach focuses on motivated memory (i.e., the selective retrieval of past information based on self-serving criteria) as a channel through which workers may sustain overly optimistic beliefs which may result in a systematic delay of work to the future. Recent work on the dynamics of motivated beliefs documents an asymmetry regarding the processing of informative signals which may allow agents to sustain such overly optimistic beliefs. For example, in an environment, where agents may form overoptimistic beliefs about their ability or skills, Zimmermann (2020) finds that positive signals about ego-relevant outcomes have a persistent effect on agents' beliefs while negative signals influence agents' beliefs only in the short run. This renders motivated memory a likely channel through which agents may sustain overly optimistic beliefs in work contexts – even though they have been exposed to informative signals about their future workload. At the same time, it has also been shown that there are limits to belief-based 'ego utility' when uncertainty about agents' ability is resolved (within a short period of time, Drobner, 2022). Hence, it is an open question whether beliefs can cause 'motivated procrastination' in working environments in which uncertainty about agents' workload is naturally resolved.

The current study aims to substantially advance the understanding of motivated procrastination (i.e., the systematic delay of work based on overly optimistic beliefs). In a longitudinal experiment, we document the dynamics of motivated beliefs about future workload over a time span of four weeks and study the causal impact of variation in beliefs on the allocation of work. The experiment consists of three sessions scheduled two weeks apart and contains four key features necessary to identify the causal role of motivated reasoning for procrastination. First, we randomly assign different work loads of a cumbersome transcription task that has to be completed by the end of the third session to receive any payment.[1] This setting provides room for participants to enjoy anticipatory utility by holding motivated, overly optimistic beliefs about low future workload. Second, we exogenously vary participants' expectations about their future workload, henceforth called their beliefs, by sending participants, at the end of the first session, noisy but informative signals about the randomly assigned workload they have to complete.[2] Third, we introduce exogenous variation in the scope that participants have to hold motivated beliefs in Session 2. While participants across treatments hold the same information and face the same incentives, those assigned to our HighScope condition are not reminded of the signal they received two weeks before (in Session 1) whereas those assigned to the LowScope condition are reminded. In consequence, HighScope participants face lower costs of suppressing negative news from the past than LowScope participants.[3] That is, there is scope for motivated memory. Finally, after eliciting participants' posteriors in Session 2, we give participants the previously unannounced opportunity to complete some of their work on the day of Session 2 instead of completing the full workload two weeks later in Session 3. This surprise work allocation decision allows us to study whether moti-

[1]To render effort costs salient, participants are required to try the task in the first session and a majority of of our participants (66 percent) consider the task very (34.6 percent) or somewhat (31.5) unpleasant.

[2]We focus on the belief regarding the probability a participant assigns to the event that her workload is low, i.e., in the bottom half of possible workloads that can be assigned. Participants were informed that different workloads are randomly assigned and have all equal likelihood such that the rational prior belief is $p_1 = 50$. The rational benchmark for agents posteriors in Session 2 (p_2) and 3 (p_3) is derived from the prior based on Bayesian updating with respect to the signal received.

[3]The suppression of negative news through 'motivated memory' has been documented as an important channel for motivated beliefs in other decision environments (see, e.g., Zimmermann, 2020; Gödker et al., 2021).

vated beliefs are action-relevant while ensuring that elicited beliefs are not biased through the allocation decision nor bias the latter.[4]

Our findings provide robust evidence for motivated reasoning through motivated memory and document that negative news suppression can indeed cause procrastination independent of potential present bias. Participants in HighScope who receive negative news hold substantially more optimistic posterior beliefs in Session 2 than participants who received negative news in the LowScope condition. Participants in HighScope consider it on average 10 percentage points (24 percent) more likely to have been assigned a low workload than participants in the LowScope condition even though they have been provided with the same signal in Session 1. For positive news, being assigned to the HighScope condition does not significantly affect posteriors. Hence, the effects of positive news about future workload persist, whereas negative news affect posteriors much less when participants are given time to 'forget'.

In a next step, we provide evidence that these belief distortions result in systematic delay of work. We establish a causal relationship between beliefs and the number of tasks completed in Session 2 by leveraging the exogenous variation in signals and treatment conditions that systematically alter beliefs. Using our exogenous variation in an instrumental variables (IV) approach, we find that a 10 percentage point increase in the subjective posterior of the probability of facing low future workload leads to completing 7 percent fewer tasks in Session 2 and reduces the likelihood of completing the maximum possible number of tasks in Session 2 by 18 percent. Hence, even though participants receive informative signals and know that uncertainty about their workload will be resolved in Session 3, motivated memory allows them to uphold overly optimistic beliefs about their total workload. Based on their beliefs, they complete fewer tasks in Session 2 and, presumably, also expect to complete fewer tasks in Session 3. However, because participants' beliefs are systematically biased, they eventually have to solve more tasks than expected. That is, they systematically delay work as compared to participants holding accurate beliefs. This results constitutes clear evidence for 'motivated procrastination'.

Our work contributes to several strands of the literature and provides important implications for theory and policy (see also Section 6). First, we contribute to a nascent empirical literature on belief-based procrastination. In contrast to traditional economics approaches which have attributed procrastination to time-inconsistent preferences (for a detailed review, see Ericson and Laibson, 2019) and emotion regulation based approaches from psychology (see Pychyl and Sirois, 2016), theoretical work on belief based procrastination provides a rationale for procrastination rooted in the anticipatory utility derived

[4] As participants do not know that part of the workload can be completed in Session 2 when we elicit their beliefs, they can neither use beliefs as commitment (and thus bias them downwards) nor be tempted to biased their beliefs up-wards as a consequence of the opportunity to delay work to the future (see also Bénabou and Tirole, 2016).

from expecting a low workload in the future. For example, Brunnermeier et al. (2017) develop a model in which agents may rationally hold overly optimistic beliefs about future workload and, based on these wrong beliefs, procrastinate. While these models are in line with beliefs and behavioral patterns observed in laboratory and field settings (Konečni and Ebbesen, 1976; Buehler et al., 1994, 1997; Byram, 1997; Roy et al., 2005; Ariely and Wertenbroch, 2002), direct empirical evidence for the link between motivated beliefs and procrastination is scarce. A rare and recent exception that indirectly sheds light on the role of beliefs for procrastination is the work by Breig et al. (2023). The authors study in a clever experimental design how feedback about agents own past procrastination behavior alters their effort allocations and commitment demand. Such feedback should affect present-biased procrastinators differently than belief-based procrastinators. Their findings are in line with the idea that both, present bias and beliefs are underlying reasons for procrastination. However, they do not model nor measure the source of incorrect beliefs but instead take them as given and focus on studying their implications. Our approach complements the work by Breig et al. (2023) by exogenously varying beliefs and actual future workloads independent of workers' tendencies to procrastinate; as well as workers scope for motivated reasoning. Thereby, we substantially advance the understanding of the source of optimistic beliefs and how they can be sustained. We provide direct empirical evidence on a causal chain from scope for motivated reasoning (due to memory) to procrastination. As we manipulate the scope for motivated reasoning by providing but not reminding all agents with relevant and informative news about their future workload, we shed new light on the emergence of overly optimistic beliefs. Further, our exogenous variation in assigned workloads and signals allows us to establish a direct link between beliefs and procrastination without the need to rely on information about the participants' past procrastination behavior.

Moreover, our study also links to recent work in which the existence of an excuse may cause procrastination. Specifically, Drucker and Kaufmann (2022) and Lepper (2022) analyze how the existence of an excuse for postponing a behavior leads to procrastination. In contrast to our approach, their work focuses on excuses that result from the possibility of not having to do any work in the future (Drucker and Kaufmann, 2022), or from explicit information avoidance, i.e., situations in which participants can stay uninformed (Lepper, 2022). We focus instead on situations in which workers receive informative signals but may exploit the possibility to forget or suppress informative signals regarding their future workload as an 'excuse' to procrastinate.

We further provide direct evidence that procrastination can persist when agents receive informative signals, as long as they have scope to forget or suppress information they received.[5] Hence, our results also provide a jigsaw piece to the puzzle of why we continuously observe procrastination although workers have plenty of possibilities to learn from past behavior and improve their work organization. For example, Le Yaouanq and Schwardmann (2022) show that participants do learn from their past behavior in a real-effort task, and become more sophisticated over time. Workload, however, is deterministic in their setting. Uncertainty about the actual workload (or the time needed to complete a task) – which is a realistic assumption in most real-life settings – and the resulting motivated beliefs may explain why we still often observe procrastination despite potential room for such learning processes.[6]

More broadly, our longitudinal study of motivated reasoning in a work context complements the literature on the dynamics of motivated reasoning and memory errors in different environments. Zimmermann (2020) finds that people form motivated beliefs by suppressing negative news about their performance in an IQ test in the course of roughly a month.[7] Gödker et al. (2021) document memory biases in the financial domain. Roy-Chowdhury (2022) provide evidence on memory biases in school grades and Müller (2022) shows that memory biases also exist for past fertility desires. Our results complement these approaches and provide clean and robust evidence of a causal effect of memory on negative news suppression in work environments. Additionally, we document the action relevance of such motivated beliefs, by identifying a causal relationship between beliefs and procrastination.

Finally, from a methodological point of view, our approach highlights a novel possibility to exogenously vary the extent of motivated reasoning by varying whether or not participants are reminded of informative signals they received before. In contrast to other approaches, which have varied the extent of motivated beliefs through the associated costs and benefits by, e.g., manipulating the strength of perceived ego-relevance (Drobner and Goerg, 2022), the resolution of uncertainty (Drobner, 2022), responsibility (Bosch-Rosa et al., 2023), anxiety motives (Engelmann et al., 2019), or incentives (Zimmermann, 2020; Gödker et al., 2021), we vary the scope for holding motivated beliefs by varying whether or not participants are reminded about signals they received before. This approach holds

[5] As our treatments and signals are exogenously assigned, the observed causal effect is orthogonal to potential additional excuses some participants may hold (e.g., motivated 'hopes' that the Session 3 may not take place due to some technical error or similar excuses).

[6] The theory by Heidhues et al. (2023) offers another interesting perspective on why (repeated) procrastination may occur: individuals may accurately recall their past actions (here: work decision) but not what led to their decision (here: signals received). As a result, actions and beliefs are consistent and procrastination can occur in equilibrium without individuals learning over time. Although this model does not speak to the asymmetry in posteriors due to negative news in HighScope in Session 2, it may explain why we do not see an adjustment to more realistic beliefs in Session 3 (shortly before the uncertainty about total workload is resolved). Overly optimistic beliefs in Session 3 may simply appear consistent to participants who chose to work rather little in Session 2.

[7] This tendency to suppress negative news can be mitigated by increasing incentives, suggesting that these news are not completely deleted from the memory.

information, risk, anxiety, and incentives constant but still affects the extent of motivated reasoning.

The rest of the paper is structured as follows. We present the details of our experimental design in Section 2. In Section 3, we derive our main hypotheses based on a simple theoretical framework. Section 4 presents our main results regarding motivated memory, negative news suppression, and procrastination. In Section 5, we discuss whether our findings depend on participants' time preferences or their emotion regulation strategies, and we highlight that participants' information preferences are predictive for biased reactions to negative news. Section 6 concludes.

2 Experimental design

2.1 Overview

To study the dynamics of motivated beliefs about future workload and their implications for procrastination, we conduct a longitudinal experiment (n=367) over four weeks. The experiment consists of three online sessions, two weeks apart, and has four key features. First, we create a common prior of an uncertain future workload that is independent of participants' innate tendencies to procrastinate. Second, we exogenously vary participants' expectations about their future workload. Third, we manipulate participants' ability to hold motivated beliefs by reminding or not reminding them about an informative signal they have received two weeks before. Fourth, we include a belief-dependent work decision that allows us to study whether motivated beliefs result in the systematic delay of work. Figure 1 illustrates the timeline of the experiment and the main contents of the three consecutive sessions participants must complete to receive payment.[8]

In Session 1, participants are informed that by the end of Session 3, they must have completed a transcription task, in which they see sequences of numbers to be transcribed to letters with the help of a coding key (see Appendix Figure A.4). Each sequence consists of six numbers. Once a participant has entered the associated letters of a sequence correctly, she is prompted with a new sequence of numbers to be transcribed using a new coding key until she has completed the total number of sequences assigned to her. Ex-ante, the total number of sequences to be completed is unknown to participants. To familiarize participants with the task, participants need to complete 10 practice sequences

[8]Participants receive 14€ for the completion of all sessions. In addition, they can earn another 6€ depending on the accuracy of their beliefs. This incentive structure rendered attrition relatively low. Out of 517 participants who completed the first session, 403 participants completed the third session, leaving us with a sample of N=367 after applying our preregistered exclusion criteria. Details on attrition and exclusion can be found in Appendix B.2. The pre-registration can be found at https://aspredicted.org/SHS_XD6 and includes an additional experiment on the dynamics of motivated reasoning regarding IQ (akin to the work by Zimmermann, 2020) which we ran in parallel but discuss in a companion paper.

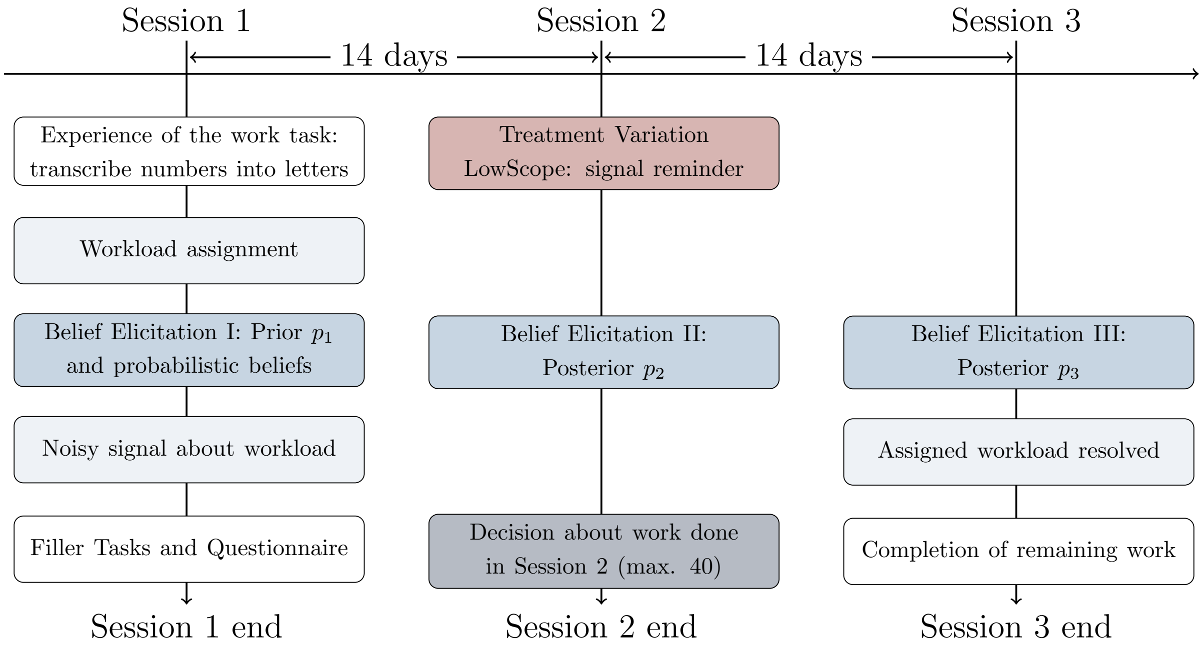

Figure 1: Timeline of the experiment

in Session 1. Going through the instructions and the practice session of the task ensures that participants become aware of the fact that the task is unpleasant and involves real effort costs. We then inform participants that they will be randomly assigned a specific number of sequences which they must have completed by the end of Session 3. Participants learn that they must solve 40 sequences plus x_i additional sequences to complete the entire experiment and receive payment, where $x_i \in \{8, 16, 24, 32, 40, 48, 56, 64, 72, 80\}$. It is common knowledge that each possible value of x_i is equally likely to be assigned to a participant, but the assigned value x_i (and thus the total number of sequences to be completed) remains unknown to participants until Session 3. To be able to calculate individual Bayesian benchmarks for participants' posteriors, we then elicit participants' prior beliefs regarding the additional workload in two steps: First, participants have to state the probability with which they believe that they have to solve at most 40 additional tasks ($p_1 = Pr(x_i \leq 40)$).[9] Second, we ask participants how likely they consider each additional workload out of the 10 possible workloads (8, 16, ... 80).[10] We incentivize the belief elicitation using the binarized scoring rule with a price of €6 paid for one randomly chosen belief elicitation (Hossain and Okui, 2013).[11] After the elicitation of priors, we provide participants with a noisy but unbiased signal of their assigned workload (x_i). This signal informs participants about how many out of three randomly chosen

[9]Note that at this stage, the objective prior beliefs would be a uniform distribution, i.e. the rational belief is $Pr(x_i \leq 40) = 0.5$.

[10]Following previous work by Zimmermann (2020), we enforce consistency of these beliefs with the participant's belief stated in the first step.

[11]Following Danz et al. (2022), we explain to participants in simple language that reporting their actual beliefs maximizes the probability of receiving the price of €6 and offer them the additional opportunity to inform themselves about the exact incentive structure.

possible workloads that have not been assigned to them are higher (lower) than their assigned workload.[12] Finally, participants complete a series of additional (filler) tasks which obscure the purpose of the experiment and provide additional insights for our analyses.[13]

In Session 2, we elicit participants' posterior regarding the probability with which they believe that they have to solve at most 40 additional tasks ($p_2 = Pr(x_i \leq 40)$). Afterwards, and unexpectedly, we offer them the opportunity to complete up to 40 sequences already in Session 2. They commit to the number of sequences they want to solve and have to complete them by the end of the day of Session 2. Due to heterogeneous opportunity costs of time, participants may commit to very different numbers of sequences to be solved in Session 2.[14] Yet, the fact that the work decision comes as a surprise, allows us to study how exogenous shifts in beliefs due to the exogenous variation in the scope for motivated beliefs and in the signals participants received (see subsection 2.2) affect participants' procrastination behavior.[15] After participants have completed the number of sequences they committed to, Session 2 ends.

At the end of Session 3, participants eventually learn how many sequences they have been assigned. To study whether participants hold more rational beliefs close to the resolution of uncertainty (see Drobner, 2022, for evidence on such behavior in ego-relevant environments), we again elicit participants' posterior belief regarding the probability with which they believe that they have to solve at most 40 additional tasks ($p_3 = Pr(x_i \leq 40)$) at the beginning of Session 3. Then, participants complete several psychological questionnaires.[16] Finally, participants learn how many additional sequences they were assigned and complete all remaining sequences of their total workload (taking into account the

[12] To ease understanding, we implemented and explained this signal structure as follows: participants were randomly assigned to a group of 10 in which every group member was assigned exactly one of the ten unique workloads. Participants received information with respect to whether three randomly selected group members had to solve more or fewer tasks than they themselves.

[13] The filler tasks included: i) a dot-spot task in which participants saw a graph of 400 red and blue dots for 8 seconds and had to estimate the percentage of blue or red dots, for which we randomized, whether participants saw more red dots (65%) or more blue dots as well as whether we asked for the percentage of red or blue dots, ii) measures for risk and time preferences, iii) status preferences, iv) the 10-item version of the Big5 personality questionnaire and v) basic demographics.

[14] Note that due to high set-up costs, a rational decision maker may also choose not to complete any task in Session 2 and such behavior might be misunderstood as "procrastination" from an ex-post perspective. However, setup costs (by design) do not differ across our treatment conditions and we find that, both, in the HighScope and in the LowScope condition, less than 3% of our participants complete zero tasks in Session 2. Hence, potential set-up costs appear to play a minor role in our setting.

[15] Specifically, this design feature excludes the possibility that i) participants bias their beliefs to use them as a commitment for working more in Session 2 (as they do not know that they will have the opportunity to work in Session 2 when reporting their belief) and ii) that a participant's work allocation decision biases the elicited beliefs (e.g., measured overoptimism in beliefs cannot result from a participants decision not to work in Session 2) which both would make identification fail.

[16] These include questionnaires on emotion regulation (Gross and John, 2003) and irrational procrastination (Steel, 2010) as well as questions regarding general preferences for information revelation (Ho et al., 2021) and competition (Helmreich et al., 1978). In the second wave of data collection, we additionally included a psychological questionnaire on defensive pessimism (Norem and Cantor, 1986).

number of sequences completed already in Session 2). After the completion of all assigned sequences, participants are informed about their payments. Recall that no payment is made if participants do not complete this last session. That is, participants forfeit any payment even if they have completed all questionnaires but failed to solve all sequences they had been assigned (which is common knowledge).[17]

2.2 Exogenous variation in the scope for motivated reasoning and in signals about workload

To study the causal role of motivated beliefs for procrastination, we exogenously manipulate the scope for motivated reasoning without changing the information decision makers receive. Our treatments vary whether participants receive a reminder of the noisy signal regarding their assigned workload (LowScope) or not (HighScope) at the beginning of Session 2 before they report their posterior and before they learn about the possibility to complete some of their workload in Session 2. Specifically, participants in LowScope are again shown the signal they saw already in Session 1 while those in HighScope are not reminded about their signal. Both, imperfect memory and biased updating may lead to distorted beliefs in the HighScope condition. In contrast, in the LowScope condition, any bias in the perception of signals comes from updating biases. Hence, differences across treatments identify the causal role of motivated memory in an environment in which agents receive informative signals about their potential workload.

In addition to the variation in scope for motivated reasoning, we exogenously vary the signals participants receive. Doing so creates exogenous variation in the beliefs participants hold, and allows us to study whether participants react asymmetrically to fully exogenously assigned negative news. The signal informs each participant about how many out of three possible workloads that have not been assigned to them are higher (lower) than their assigned workload. Thus, the signal ranges from very positive – all of the three non-assigned workloads were higher than the workload assigned to the participant – to very negative – all of the three non-assigned workloads were lower than the workload assigned to the participant.

2.3 Procedures

The online experiment was programmed in oTREE (Chen et al., 2016) and data collection took place in two waves (Wave 1: June-July 2022, Wave 2: October-November 2022). We recruited participants via ORSEE (Greiner, 2015) from the student subject pools of the Munich Experimental Laboratory of Economic and Social Sciences (MELESSA) and the TU-WZB-laboratory in Berlin. In the morning of the day a session was scheduled, each

[17]Note that we do not observe selective attrition based on negative news (see Appendix Figure A.12).

participant received an individual link to the online interface, which explained all tasks they had to complete within a given session. To remain in the study and qualify for final payment, participants had to complete a session by 10pm of the day at which they received the link. They were informed about this requirement and the exact dates of all three sessions at the recruitment stage. For our final sample, we follow our preregistration and exclude participants who did not complete all three sessions or did not pass the specified exclusion criteria.[18] The final sample consists of 367 participants. The median participant spent 103 minutes on the three sessions in total and participants earned on average 17.14€.[19]

3 Hypotheses

In this section, we derive hypotheses regarding belief formation and work allocation based on a simple theoretical framework in which an agent has to complete a job consisting of $b + x$ tasks, where a task is equivalent to one sequence of the transcription task in the experiment. It is $b > 0$ and the random variable x is distributed according to some known distribution function $F(\cdot)$ with $\text{supp}(F) \subseteq \mathbb{R}_+$, based on which the agent holds some prior about x. The agent updates her prior upon receiving a signal regarding the realization of x. Subsequently, the agent learns that she can spread working on the job across two dates and chooses how many tasks to solve on the first date. All remaining tasks will have to be solved at the second date; the work decision cannot be revised.

3.1 Belief formation

Akin to our experiment, suppose the agent starts out with a prior belief p_1, which is the probability that she assigns to the outcome of having a relatively low workload, e.g., facing $x \leq \frac{1}{2}\max(x)$. The agent then receives a signal which can be negative or positive , $s \in \{-1, 1\}$. A negative signal indicates that the agent is more likely assigned a relatively high workload ($x > \frac{1}{2}\max(x)$) whereas a positive signal points to a relatively low workload of $x \leq \frac{1}{2}\max(x)$.

[18]Following our preregistration, we excluded participants who stated to have only a poor level of understanding of English, which was the experimental language, participants who rushed through the first screens with explanations about the belief elicitation, and participants who failed at least one of two basic attention checks. In the Appendix, we provide a comprehensive overview of the exclusion criteria (see Appendix Figure A.12). Further, due to a technical problem in Session 1 of Wave 1, a subset of participants learned about the nature of the task (i.e., they saw the instructions for the task as intended) but did not have to complete the 10 practice tasks. As beliefs and belief dynamics did not differ significantly for these participants, we included them in our final data set. We obtain qualitatively similar results when excluding these participants (see Appendix A.3.2).

[19]The average time 'spent in the experiment' (169 minutes) appears relatively high, but participants were free to take breaks and allowed to complete the online sessions until 10pm of the session day.

A Bayesian agent with perfect memory will incorporate the signal into her assessment and form a posterior belief about her future workload according to her prior belief and Bayes' rule. However, an agent's belief may differ from this rational Bayesian benchmark for two reasons. First, an agent may suffer from imperfect memory and recall the signal inaccurately. Second, she may distort her belief toward expecting a relatively lower workload to enjoy anticipatory utility. We discuss both of these reasons below.

Consider first the idea of imperfect recall. An agent with limited memory may only imperfectly recall her signal or may misremember a negative signal as being positive (or vice-versa) and consequently form a biased posterior belief. If the agent imperfectly recalls her signals, this belief differs from the Bayesian posterior even if the recalled signal is taken into account in a Bayesian manner. Based on empirical evidence, it appears reasonable to assume that the probability that people forget or misremember a negative signal is higher than forgetting or misremembering a positive signal (Chew et al., 2020). If negative news are more likely to be forgotten or distorted, (biased) imperfect recall yields the following hypothesis.

Hypothesis 1. *If agents have more scope to forget or misremember the signal about future workload, average beliefs about total workload will be overly optimistic.*

Using data from our experiment, we can explicitly test for whether Hypothesis 1 holds, by testing whether the distance of the participants' posteriors from the Bayesian benchmark is larger in HighScope, where participants have the possibility to forget informative signals, than in LowScope, where they are reminded of the signal about their future workload.

Consider next the second possibility: agents may experience anticipatory utility and enjoy the expectation of having to work relatively little. If so, agents may benefit from distorting their beliefs as there is a trade-off between holding more optimistic beliefs about future workload and the costs associated with distorting beliefs. In our experimental setup, such costs can result from the cognitive effort of maintaining incorrect beliefs and from instrumental belief utility because participants are incentivized to report accurate beliefs. More generally, the costs from distorting beliefs also comprise utility losses that arise from a suboptimal work decision that the agent takes based on her incorrect beliefs. However, in our experiment agents do not know that they will be able to complete part of their job in Session 2 already. Thus, these costs from suboptimal work allocation cannot be part of the agents rationale when choosing beliefs in our experiment.[20] Hence, the

[20]In the model by Brunnermeier et al. (2017), the agent knows that she has to distribute her workload over two dates when she forms beliefs about her future workload. Therefore, their model includes these costs at the belief formation stage whereas ours does not.

agent is maximizing:

$$\max_{\hat{p}} \quad \alpha v(\hat{p}) - \frac{1}{2}\gamma(p_M - \hat{p})^2, \tag{1}$$

where α is the weight an individual places on anticipatory utility, $\hat{p}$ is the chosen belief that an individual forms, and p_M is the, possibly incorrectly, *recalled* signal. The weight α may vary for instance with the length of the time period during which anticipatory utility can be enjoyed. We assume that anticipatory belief utility is differentiable and increases in the belief that workload is low, $\partial v(\hat{p})/\partial \hat{p} > 0$. The cognitive and instrumental costs from distorting beliefs are subsumed into a quadratic loss function parameterized with γ. Solving the agent's maximization problem yields the following first order condition

$$\frac{\alpha}{\gamma}\frac{\partial v(\hat{p})}{\partial \hat{p}} = \hat{p} - p_M \tag{2}$$

which shows that an agent who enjoys the expectation of a low workload will more strongly distort her beliefs the higher the marginal utility from such beliefs relative to the cognitive or instrumental costs from holding these. This observation gives rise to the following hypothesis:

Hypothesis 2. *The more an agent benefits from anticipatory utility relative to distortion costs, i.e. the larger α/γ, the more optimistic her belief about her total workload will be.*

To test Hypothesis 2, we compare posteriors elicited in Session 2 and Session 3 before the resolution of uncertainty. In Session 3, agents cannot benefit from anticipatory utility any longer as their true workload will be resolved immediately afterwards. In contrast, when asked about their belief in Session 2, the pertaining anticipatory utility can be enjoyed throughout the fourteen days until Session 3 provides the resolution. Thus, we predict a stronger underestimation of future workloads in Session 2 as compared to Session 3.

3.2 Work decision

Akin to our experimental setting, we assume that the agent learns that she can split her work between two dates (today and a later date) after learning the signal and forming her (potentially motivated) belief. Taking the agent's subjective posterior $\hat{p}$ as given, the agent now maximizes her expected utility by allocating the expected total workload across the two possible working dates. We denote the amount of subtasks allocated to the first date by w_1. The agent completes w_1 immediately on the first date (Session 2 in the experiment) and has to complete the remaining subtasks on the second date (Session 3 in the experiment). We assume that the agent has quasi-hyperbolic time-preferences parameterized by an impatience factor $\beta \in (0, 1]$ and a discount factor $\delta \in (0, 1)$. We further assume that work gives her utility $u(w) = -cw^2$, which exhibits convex effort costs. Thus, when making the decision how much to work, an agent with the subjective belief $\hat{p}$ (about facing a low workload) faces the following maximization problem:

$$\max_{w_1} \quad u(w_1) + \beta\delta\mathbb{E}_{\hat{p}}[u(b + x - w_1)], \tag{3}$$

where b is the number of tasks that any agent has to solve for sure and x denotes the realization of the random number of additional subtasks to be solved. Using the subjective belief $\hat{p}$ and the simplification to binary workloads (with w_L denoting low workload and $w_H > w_L$ denoting high work load), the expected utility can be written as $\mathbb{E}_{\hat{p}}[u(b + x - w_1)] = \hat{p}u(b+w_L-w_1)+(1-\hat{p})u(b+w_H-w_1)$. The solution to the maximization problem (3) is characterized by the following condition

$$\frac{\partial u(w_1)}{\partial w_1} = \beta\delta \left(\hat{p}\frac{\partial u(b + w_L - w_1)}{\partial w_1} + (1 - \hat{p})\frac{\partial u(b + w_H - w_1)}{\partial w_1}\right). \tag{4}$$

It becomes clear that the right hand side increases when $\hat{p}$ (the subjective belief that workload is low) increases. Hence, for the equation to hold, increases in $\hat{p}$ must result in a decrease of the workload allocated to the first date, w_1, because we have assumed that effort cost are convex. Thus, the expectation about x (realized workload) affects the decision how much work to defer to the future in a very intuitive way:

Hypothesis 3. *Agents who hold more optimistic beliefs regarding the realization of x (and thus their total workload) will complete fewer tasks immediately.*

To test Hypothesis 3, we will instrument beliefs using the exogenous variation in signals and scope for motivated reasoning in our experiment and study their effects on the number of tasks completed in Session 2.

4 Results

Following Zimmermann (2020) and in line with our simple theoretical framework, we classify positive and negative news by subsuming the signals participants observed into a binary variable *Neg. News* when studying the role of motivated memory for overly optimistic beliefs and their consequences for the systematic delay of work empirically. This binary variable indicates negative news for participant i, when at least two of the three drawn non-assigned possible realizations of x are smaller than the assigned x_i. *Vice versa*, it indicates positive news, when at least two of the drawn non-assigned possible realizations of x are bigger than the assigned x_i.[21]

[21] In Appendix A.1, we provide qualitatively similar results using the non-simplified feedback.

4.1 Motivated beliefs, updating and imperfect recall

Figure 2 shows participants' average posterior beliefs in Session 2 (denoted p_2, which is the subjective assessment of $Pr[x_i \leq 40]$ as elicited in Session 2) by negative/positive news across treatments. The dashed line indicates the rational prior probability of 50 percent, which is also the modal response in the elicitation of prior beliefs in Session 1 of our experiment. Figure 2 reveals a striking effect of our variation in scope for motivated reasoning. When participants received negative news (left side) and have HighScope for motivated reasoning, participants hold beliefs close to 50 percent and thus appear to ignore the signal received. In contrast, with LowScope, i.e. when they are reminded of the signal before stating their belief, their posterior belief of facing low workload is substantially lower (p-value = 0.003, t–test). With positive news, instead, we observe very similar posteriors in HighScope and LowScope (p-value = 0.669, t–test).[22]

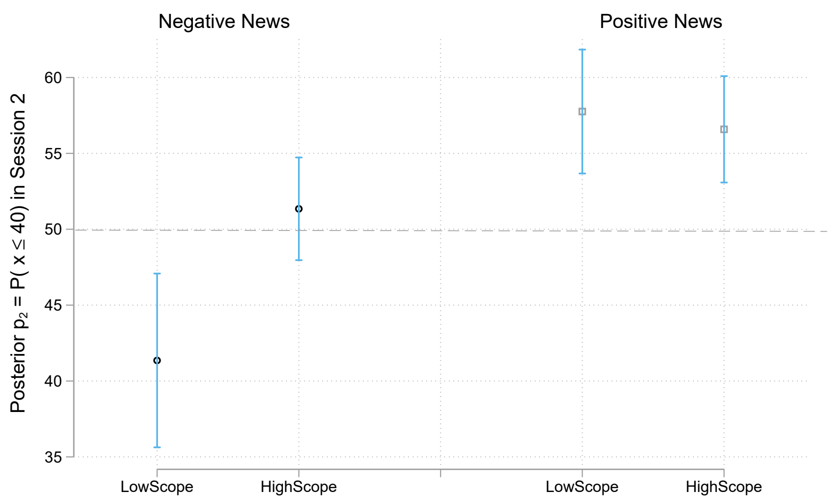

Figure 2: Posterior beliefs

Notes: The figure shows participants' posterior beliefs in Session 2 (p_2) across treatment conditions and news (positive vs. negative) received. The blue bars indicate 95%-confidence intervals.

To judge whether beliefs are overly optimistic (or pessimistic), we further compute the distance between participants' stated posterior p_2 and their Bayesian Posterior based on each participants' priors and signals received ($p_2 - p_{\text{bay}}$). Figure 3 shows that, on average, participants update conservatively: they are too optimistic after negative news and too pessimistic after positive news.[23] Comparing participants optimism by news across treat-

[22]Additional analyses in Appendix A.1 reveal that the difference between LowScope and HighScope after negative news is entirely driven by those participants who received very negative news (i.e., participants for whom none of the drawn non-assigned numbers is larger than the assigned number, see also Figure A.1).

[23]This finding of conservative updating is in line with earlier results obtained in laboratory experiments (see for example Coutts, 2019; Möbius et al., 2022) but it may hinge on the informativeness of the signals (Augenblick et al., 2023).

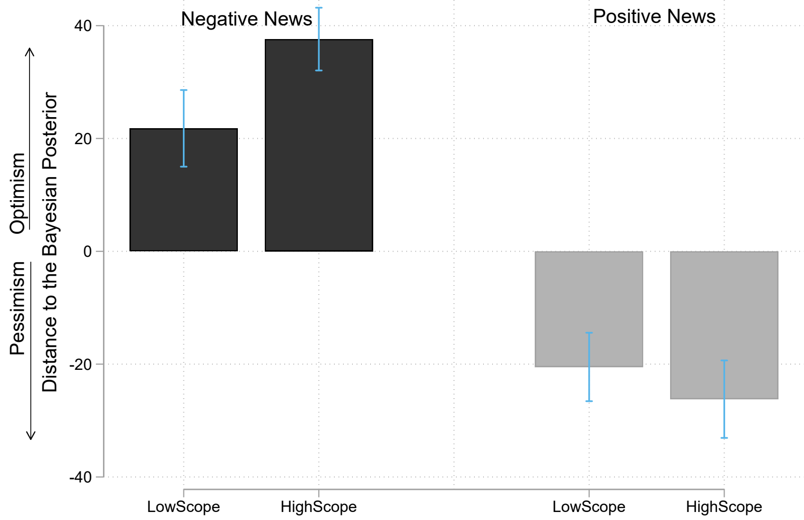

Figure 3: Optimism

Notes: The figure shows participants' optimism in Session 2 ($p_2 - p_{bay}$) across treatment conditions and news (positive vs. negative) received. The blue bars indicate 95%-confidence intervals.

ments mirrors our previous finding. Participants who received negative news (left panel) are substantially more overoptimistic when assigned to the HighScope condition than when assigned to the LowScope condition (p-value < 0.001, t–test). In HighScope, the distance of stated posteriors to the Bayesian posterior is on average 19 pp larger than in LowScope. For positive news, the scope for motivated reasoning does not substantially alter participants' pessimism (right panel, p-value $= 0.215$, t–test).[24]

These findings are also confirmed by the regression analyses presented in Table 1. In Panel A, Column (1), we regress participants posterior belief p_2 on our treatment indicator HighScope, a dummy for negative news (*Neg. News*) and their interaction. We subsequently add control variables related to participants time-preferences and emotion regulation strategies in Columns (2) to (7). In Column (2) we control for an aggregate measure *Patience* (derived from two submeasures: i) hypothetical choices between money now or later, and ii) the answer on a scale from 0 to 10 to the question *How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future?* from (Falk et al., 2023). The patience measure is the average of both normalized submeasures and takes a value between 0 and 1. In Column (3), we include a measure for the tendency to procrastinate (Steel, 2010). With respect to emotion regulation we include the two factors calculated from the answers of the emotion regulation scale (Gross and John, 2003): Suppression and Reappraisal (Columns 4 and 5). We also include the preferences for information scale by Ho et al. (2021) (Column (6)). The specification in Column (7) includes all these control variables. In Panel B, we repeat this

[24]This result is robust to using rational priors instead of individual priors (see Appendix A.3.1).

Table 1: Regression results: Effects on posterior beliefs and optimism

Panel A: Posterior belief p_2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HighScope	-1.173	-1.286	-1.144	-1.239	-1.235	-1.128	-1.360
	(2.71)	(2.73)	(2.71)	(2.72)	(2.71)	(2.73)	(2.77)
Neg. News	-16.403***	-16.328***	-16.382***	-16.482***	-16.500***	-16.454***	-16.609***
	(3.54)	(3.54)	(3.54)	(3.54)	(3.57)	(3.53)	(3.54)
HighScope*Neg. News	11.165***	11.310***	11.105**	11.177***	11.214***	11.127**	11.332***
	(4.31)	(4.32)	(4.31)	(4.31)	(4.32)	(4.33)	(4.35)
Patience		4.082					4.102
		(5.03)					(5.09)
Procrastination scale			-0.468				0.157
			(2.17)				(2.25)
Suppression factor				-0.852			-0.955
				(0.88)			(0.88)
Reappraisal factor					0.431		0.448
					(1.05)		(1.05)
Pref. for information						-0.748	-1.259
						(2.53)	(2.58)
Constant	57.758***	54.813***	59.292***	60.894***	55.872***	60.036***	59.673***
	(2.05)	(4.11)	(7.58)	(3.87)	(4.97)	(8.02)	(11.36)
N	367	367	367	367	367	367	367

Panel B: Optimism ($p_2 - p_{bay}$)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HighScope	-5.709	-5.476	-5.457	-5.697	-5.995	-5.538	-5.375
	(4.61)	(4.62)	(4.62)	(4.64)	(4.60)	(4.65)	(4.69)
Neg. News	42.295***	42.140***	42.486***	42.309***	41.855***	42.103***	41.741***
	(4.58)	(4.61)	(4.56)	(4.61)	(4.67)	(4.56)	(4.70)
HighScope*Neg. News	21.530***	21.231***	21.011***	21.528***	21.756***	21.388***	20.863***
	(6.39)	(6.40)	(6.39)	(6.40)	(6.37)	(6.44)	(6.45)
Patience		-8.413					-9.568
		(8.19)					(8.33)
Procrastination scale			-4.087				-3.560
			(2.85)				(3.02)
Suppression factor				0.151			0.199
				(1.27)			(1.31)
Reappraisal factor					1.974		1.941
					(1.98)		(2.01)
Pref. for information						-2.835	-1.896
						(4.01)	(4.07)
Constant	-20.501***	-14.431**	-7.107	-21.058***	-29.133***	-11.870	-5.380
	(3.05)	(6.85)	(9.95)	(5.80)	(9.08)	(12.62)	(17.29)
N	367	367	367	367	367	367	367

Notes: The table shows results from OLS regressions. The dependent variable in Panel A is participants' posterior belief about the probability to face low workload (p_2). The dependent variable in Panel B is participants' optimism ($p_2 - p_{\text{bay}}$). The main explanatory variables are the treatment dummy HighScope, a dummy for negative news (Neg. News) and their interaction. The control variables are continuous measures resulting from the respective questionnaires. Robust standard errors clustered at the day level reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

approach focusing on participants' optimism given by $p_2 - p_{bay}$ as the dependent variable. As can be seen, our results are robust and indicate strong reactions to HighScope when participants received negative news. Including control variables does not change the point estimates of the treatment effect observed for negative news. Hence, the effect of our exogenous scope variation on beliefs is robust to controlling for variation in participants' time preferences, emotion regulation strategies and information preferences. These findings are in line with Hypothesis 1.

Result 1. *Scope for motivated reasoning results in negative news suppression and thus to overly optimistic beliefs about future workload after receiving negative news.*

4.2 Belief dynamics and anticipatory utility

Following earlier empirical results (Drobner, 2022), we presume that the potential benefits from anticipatory utility become negligible shortly before the resolution of uncertainty (in Session 3), whereas participants in Session 2 may enjoy anticipatory utility from expecting low workload for the full two weeks preceding the resolution of uncertainty in Session 3. As participants face monetary incentives to report correct beliefs, participants who rationally bias their beliefs in Session 2 to enjoy anticipatory utility should become more realistic in Session 3, where the anticipatory motive disappears. To test Hypothesis 2, we compare participants posterior beliefs about low future workload in Session 2 and Session 3.

Participants' beliefs hardly change from Session 2 to 3 (mean(p_2) = 52.58, mean(p_3) = 51.57) and these changes are statistically insignificant ($p = 0.444$, t–test). This remains true even for the subsample of participants who received negative news in LowScope, where the belief difference appears largest (t-test, $p = 0.342$). Furthermore, we find that 42.5% of our participants exhibit "sticky beliefs". That is, they state exactly the same expectations in Session 2 and Session 3 ($p_2 = p_3$).

Figure 4 illustrates that also our measure for optimism is on average essentially the same in Sessions 2 and 3. Thus, in contrast to Hypothesis 2, we find no evidence that beliefs become more realistic in Session 3. This result implies that rational anticipatory utility or motivated recall alone cannot explain the observed belief patterns. However, as we discuss in Section 5, these motives in combination with belief adjustment costs or a misguided recall of the decision process, which both prevent the correction of beliefs when incentives change, may cause the observed negative news suppression and belief dynamics.

Result 2. *Being close to the resolution of uncertainty about future workload does not result in more realistic beliefs.*

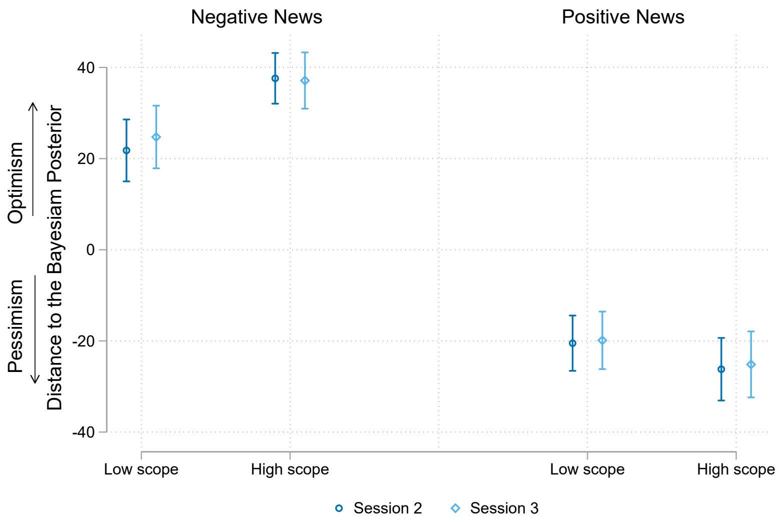

Figure 4: Belief dynamics

Notes: The figure shows participants' optimism in Session 2 ($p_2 - p_{bay}$) and Session 3 ($p_3 - p_{bay}$) across treatment conditions and news (positive vs. negative) received. The blue bars indicate 95%-confidence intervals.

4.3 The causal effect of motivated beliefs on work allocation

Finally, we shed light on the action relevance of motivated beliefs and show that more optimistic beliefs eventually result in the systematic delay of work. To do so, we study how beliefs affect the number of tasks participants complete already in Session 2.[25] In line with the idea that participants may be heterogeneous in their time-preferences and opportunity costs of time in Session 2 and 3, we find substantial heterogeneity in how many tasks participants complete in Session 2 (mean = 29.67, sd = 11.49).[26] To identify the causal role of beliefs for procrastination, we exploit the exogenous variation in beliefs induced by the signals and the treatment in scope for motivated reasoning. This variation is by design orthogonal to participants' opportunity costs of time and time-preferences.[27]

Table 2 shows the second stage of an instrumental variables (IV) approach, in which we instrument participants' beliefs with treatment assignment (HighScope vs. LowScope), the news received, and the interaction of these two variables. In Panel A of Table 2, the dependent variable is the number of sequences completed in Session 2. Panel B focus on the probability to complete the maximum possible number of tasks (i.e. 40

[25]Recall that we restricted this number to a maximum of 40. This restriction ensures that participants will have to complete at least some tasks in Session 3 and it prevents participants from working on more tasks than they were assigned to.

[26]The median participant chose to complete 30 tasks. 44.4% of participants chose to complete the maximum number of tasks, 40, which is also the modal choice.

[27]Note that we also observe meaningful adjustments in the number of tasks according to news and scope (see Figure A.3 in the Appendix).

Table 2: Regression results: Effect of beliefs on the work decision

Panel A: The number of tasks participants complete in Session 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Posterior p_2 (instrumented)	-0.232**	-0.230**	-0.232**	-0.232**	-0.225**	-0.231**	-0.222**
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Patience		-0.281					-0.629
		(3.23)					(3.24)
Procrastination scale			-0.452				-0.275
			(1.30)				(1.36)
Suppression factor				-0.019			-0.029
				(0.52)			(0.53)
Reappraisal factor					0.609		0.607
					(0.66)		(0.67)
Pref. for information						-0.249	-0.214
						(1.60)	(1.62)
Constant	41.650***	41.735***	43.166***	41.709***	38.539***	42.375***	40.496***
	(5.36)	(5.13)	(6.87)	(5.85)	(6.31)	(7.41)	(8.96)
Mean dependent variable	29.67	29.67	29.67	29.67	29.67	29.67	29.67
N	367	367	367	367	367	367	367

Panel B: The probability to solve the maximum number of tasks (40) in Session 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Posterior p_2 (instrumented)	-0.008*	-0.008*	-0.008*	-0.007*	-0.008*	-0.008*	-0.007*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Patience		-0.009					-0.013
		(0.13)					(0.13)
Procrastination scale			-0.013				-0.003
			(0.05)				(0.05)
Suppression factor				-0.028			-0.028
				(0.02)			(0.02)
Reappraisal factor					0.007		0.008
					(0.03)		(0.03)
Pref. for information						0.020	0.011
						(0.06)	(0.06)
Constant	0.836***	0.842***	0.880***	0.929***	0.798***	0.783***	0.874**
	(0.21)	(0.22)	(0.28)	(0.24)	(0.24)	(0.28)	(0.34)
Mean dependent variable	0.44	0.44	0.44	0.44	0.44	0.44	0.44
N	367	367	367	367	367	367	367

Notes: The table shows results from IV regressions using the general method of moments estimator. The posterior belief (p_2) about facing low workload is instrumented with the treatment dummy for HighScope, a dummy for negative news and the interaction of both (for the first stage, see Column (1) of Panel A in Table 1). The dependent variable in Panel A is participants' posterior belief about the probability to face low workload (p_2). The dependent variable in Panel B is participants' optimism ($p_2 - p_{\text{bay}}$). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

tasks) in Session 2. The first stage results can be found in Column (1) of Panel A in Table 1.

Our results reveal that a 10 percentage point increase in (instrumented) posteriors – that is, an increase in optimism about future workload – causes participants to solve 2.32 (7 percent) fewer tasks (see Column (1) of Panel A) and reduces the likelihood to complete the maximum possible number of tasks in Session 2 by 8 percentage points or 18 percent (see Column (1) of Panel B). As in Table 1, the additional specifications include control variables related to time preferences, the tendency to procrastinate, emotion regulation and information preferences. Again, we find that our point estimates are hardly affected when including these controls. In summary, these results are in line with Hypothesis 3.

Result 3. *More optimistic beliefs reduce the number of tasks completed immediately, resulting in a systematic delay of work.*

4.4 Heterogeneous treatment effects

Our treatment variation was designed such that causal shifts in motivated beliefs stem from the interaction of exogenously assigned negative news and exogenously assigned scope for motivated memory. As such, we induce variation in beliefs orthogonal to participants' time preferences, their emotional regulation strategies, and their general tendency to avoid receiving information that may reveal negative news. Our regression analyses confirm the robustness of the observed treatment effect as the inclusion of these additional control variables (Patience, Suppression, Reappraisal, and information preferences) does neither substantially alter the effect of HighScope on motivated reasoning after negative news (see Table 1) nor the impact of the exogenous variation in beliefs on the allocation of work (see Table 2).

As time preferences, emotion regulation, and information preferences may nevertheless moderate the observed treatment effect, we provide additional exploratory analyses for different subgroups of participants using median splits of the respective variables. We focus on optimism ($p_2 - p_{bay}$) as the outcome variable for these analyses, as it avoids potential biases due to imbalances in priors and signals across different subgroups of smaller sample size.

4.4.1 Time preferences

Preference-based time inconsistent behaviors have been put forward as a main reason for why people procrastinate. For example, present-biased individuals may wish to allocate more work to the future than non-present biased individuals. Thus, present bias may mediate the belief-based delay of work we identified. Addressing the potential role of present bias in our results, we use a twofold approach to analyze whether there are systematic dif-

Table 3: Regression results: Heterogeneity in optimism with respect to time preferences

	(1) Full sample	(2) High patience	(3) Low patience	(4) High procr.	(5) Low procr.
HighScope	-5.709	-7.826	-2.028	-2.713	-8.554
	(4.61)	(5.69)	(7.92)	(6.43)	(6.64)
Neg. News	42.295***	44.075***	40.765***	38.510***	44.930***
	(4.58)	(6.39)	(6.57)	(6.82)	(6.20)
HighScope*Neg. News	21.530***	19.685**	21.133**	21.854**	21.892**
	(6.39)	(8.45)	(10.09)	(9.57)	(8.63)
Constant	-20.501***	-20.513***	-20.490***	-22.869***	-18.283***
	(3.05)	(4.11)	(4.49)	(4.22)	(4.42)
N	367	182	185	172	195

Notes: All regressions are estimated using OLS. The dependent variable is participants' optimism ($p_2 - p_{\text{bay}}$). The explanatory variables are the treatment dummy HighScope, a dummy for negative news (Neg. News) and their interaction. Column (1) uses the full sample. Columns (2) and (3) use the subsample of high and low patience individuals each, determined by a median split of our measure for Patience (an aggregate measure derived from hypothetical choices between money now or later, and the stated willingness to give up something that is beneficial today in order to benefit in the future (Falk et al., 2023)). Columns (4) and (5) use the sample of procrastinators and non-procrastinators, based on a median split of our measure for the tendency to procrastinate (Steel, 2010)). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

ferences in the belief dynamics based on participants' time preferences. First, we present results from a median split with respect to participants' patience using a measure we construct based on Falk et al. (2023). Second, we present results using a median split with respect to participants' scores on the irrational procrastination scale (Steel, 2010).

We report the results of these exploratory analyses in in Table 3. Column (1) shows our original specification for optimism (see also Table 1, Panel A, Column (1)) as a benchmark. Columns (2) and (3) show that both impatient and more patient participants (median split) tend to suppress negative news when given scope to do so and the point estimates of the interaction term are very similar to the one from the full sample in Column (1) for both subgroups. Hence, our data suggests time preferences are not a strong mediator of belief adjustments based on scope for negative news suppression. Columns (4) and (5) in Table 3 show that the effect is also robust when splitting the sample by participants' tendency to procrastinate with almost identical coefficients on the interaction term for both subgroups and the full sample. Thus, neither time preferences nor the tendency to procrastinate are strong mediators of overoptimism resulting from motivated memory.

4.4.2 Emotion regulation

In psychology, emotion regulation has been proposed as a potential cause for the systematic delay of work (see Pychyl and Sirois, 2016). Depending on an individual's ability and their strategies to cope with negative emotions, individuals may be more or less inclined

Table 4: Regression results: Heterogeneity in optimism with respect to emotion regulation

	(1) Full sample	(2) High suppr.	(3) Low suppr.	(4) High reappr.	(5) Low reappr.
HighScope	-5.709	3.367	-14.768**	-1.315	-8.737
	(4.61)	(6.76)	(6.04)	(7.52)	(5.86)
Neg. News	42.295***	48.125***	36.967***	41.804***	43.576***
	(4.58)	(6.61)	(6.43)	(6.40)	(6.60)
HighScope*Neg. News	21.530***	11.158	31.640***	20.107**	21.265**
	(6.39)	(9.48)	(8.54)	(9.52)	(8.91)
Constant	-20.501***	-24.638***	-16.627***	-22.091***	-19.360***
	(3.05)	(3.66)	(4.79)	(4.41)	(4.21)
N	367	180	187	171	196

Notes: All regressions are estimated using OLS. The dependent variable is participants' optimism ($p_2 - p_{\text{bay}}$). The explanatory variables are the treatment dummy HighScope, a dummy for negative news (Neg. News) and their interaction. Column (1) uses the full sample, while the following columns use sample splits according to the two dimensions of emotion regulation: suppression and reappraisal (Gross and John, 2003). Column (2) uses the subsample of individuals that score above median on the suppression factor, while Column (3) uses the subsample that score at or below median on the suppression factor. Column (4) uses the subsample of individuals that score above median on the reappraisal factor, while Column (5) uses the subsample that score at or below median on the reappraisal factor. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

to distort their beliefs about the likelihood of unpleasant events (see, e.g. Engelmann et al., 2019). To speak to this idea, we study heterogeneous treatment effects in terms of two ways of regulating emotion: *Suppression* and *Reappraisal* using the respective scales by Gross and John (2003). *Suppression* measures to what extent people inhibit their emotion-expressive behavior. *Reappraisal* measures whether individuals deal with negative emotions by redirecting their thoughts to a positive situation. Both strategies may help participants to cope with negative news and thereby affect participants' need to bias their beliefs.

Table 4 reports the results of these additional exploratory analyses, again including the benchmark specification in Column (1). We find that individuals with an above median tendency to inhibit their emotion-expressive behavior become only insignificantly more optimistic after negative news in the HighScope treatment (Column (2)) and those who are less likely to suppress emotion-expressive behavior become significantly more optimistic (Column (3)). This is in line with the idea that people who are more likely to express their emotions are also more likely to form biased beliefs based on motivated memory. However, as shown in Column (3), this group is also more likely to be too pessimistic when not being reminded after positive news in HighScope. As such, they apparently tend to forget signals in general and hold posterior beliefs close to the priors. In contrast, we find no apparent heterogeneity when we split the sample based on participants' reappraisal strategies (see Columns (4) and (5)).

Table 5: Regression results: Heterogeneity in optimism with respect to information preferences

	(1) Full sample	(2) High info pref.	(3) Low info pref.
HighScope	-5.709	-7.140	-3.794
	(4.61)	(5.89)	(7.16)
Neg. News	42.295***	46.158***	39.926***
	(4.58)	(6.26)	(6.44)
HighScope*Neg. News	21.530***	11.823	27.113***
	(6.39)	(8.91)	(9.15)
Constant	-20.501***	-21.191***	-19.959***
	(3.05)	(4.10)	(4.43)
N	367	160	207

Notes: All regressions are estimated using OLS. The dependent variable is participants' optimism ($p_2 - p_{\text{bay}}$). The explanatory variables are the treatment dummy HighScope, a dummy for negative news (Neg. News) and their interaction. Column (1) uses the full sample, while the following columns use sample splits according to information preferences (Ho et al., 2021). Column (2) uses the subsample of individuals that score above median on the information preference scale, while Column (3) uses the subsample that score at or below median on information preference scale. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.4.3 Information preferences

Apart from participants' time-preferences and their general strategies to cope with negative news, their tendency to acquire or avoid information that may contain negative news could affect how they react to variation in the scope for negative news suppression. On the one hand, decision makers who tend to avoid information may also be more willing or able to suppress or forget negative news, as both information avoidance and motivated memory require a willingness to ignore information that is in principle accessible. On the other hand, decision makers who tend to avoid information may exactly do so because they have a hard time suppressing negative news once they received them. If so, 'information avoiders' may react less to scope for negative news suppression. To study whether preferences for information shape the impact of scope for motivated reasoning, we use participants' preferences for the revelation of (unpleasant) information, which we elicited following Ho et al. (2021).

Table 5 reports the results of this additional exploratory analysis, again including the benchmark specification in Column (1) and using a median split as before. We find that information preferences are indeed relevant for motivated memory. The interaction effect of HighScope and negative news (11.823) is substantially smaller and statistically insignificant among participants with a strong preference for information revelation (Column (2)) whereas participants with weaker preferences for information revelation are 27 pp more optimistic in HighScope after receiving negative news (Column (3)). These findings suggest that participants who generally tend to avoid information are also much more likely to form overly optimistic beliefs based on negative news suppression.

5 Discussion

The findings above reveal a causal link from scope for motivated reasoning (through motivated memory) to overoptimistic beliefs about workload and document the action-relevance of such beliefs for the allocation of work across time. Given the opportunity to smooth their assigned workload over two dates, individuals with overly optimistic beliefs about their total workload decide to work less in the present than individuals with less optimistic beliefs. As overoptimism about total workload in our experiment results from an exogenous change in the scope for motivated memory about negative news, we provide direct evidence for the systematic delay of work based on overly optimistic beliefs. In this section, we discuss an important design element that allowed us to empirically identify such 'motivated procrastination', comment on the dynamics of motivated beliefs in our setting, and discuss how future research could deal with motivated reasoning in more complex, multidimensional work environments.

First, our novel experimental approach includes a crucial design feature which allows for the clean identification of the causal relationships of interest but merits some further discussion. In the experiment, individuals were not informed upfront about the possibility to allocate their expected workload across the two work dates. Instead, after eliciting their posterior beliefs in Session 2, we surprised them with the possibility to complete some of their work immediately (i.e. at the end of Session 2). This feature avoids both the use of overly pessimistic beliefs as commitment[28] as well as a biased elicitation of overly optimistic beliefs.[29] As such, the feature ensures that we can cleanly identify the direct effect of scope for motivated reasoning on motivated beliefs and document the relevance of the exogenous variation in beliefs for actions. However, technically speaking, this feature forces us to derive insights on "motivated procrastination" based on a decision to complete work 'earlier than expected' (as participants were upfront only informed that they had to have completed the task by the end of Session 3) rather than 'later than planned' (the typical way in which preference-based procrastination decisions have been studied in the past). Importantly, our conclusions still speak to 'motivated procrastination', i.e. the systematic delay of work based on overly optimistic beliefs as we compare participants' work allocations across treatment conditions. Specifically, we compare work allocations across virtually identical groups of individuals who have been randomly assigned a par-

[28] Overly pessimistic beliefs would have allowed a sophisticated preference-based procrastinator to complete more tasks early on than *ceteris paribus* otherwise. This two-way dependency between beliefs and actions is clearly spelled out in Brunnermeier et al. (2017).

[29] If participants had known about the work allocation decision, preference-based procrastinators may had reported overly optimistic beliefs to justify the systematic delay of work.

ticular workload and have been randomly reminded (or not) about the signal regarding their workload. Hence, if beliefs of individuals who have been randomly assigned to the low scope condition are systematically more optimistic after negative news than those of individuals assigned to the high scope condition, and exogenous variation in beliefs about the total workload causally affects the number of tasks completed in Session 2, motivated memory is the underlying reason for the systematic delay of work. In other words, it is the comparison of work allocation decisions for different, exogenously manipulated beliefs that allows us to learn about motivated procrastination.

Second, in addition to studying the relationship of motivated memory, overoptimistic beliefs and the systematic delay of work, our experimental design also sheds light on the dynamics of motivated beliefs in environments in which uncertainty is known to be resolved. Previous evidence on ego-relevant motivated beliefs by Drobner (2022) has shown that motivated beliefs are not formed when resolution is immediate, in line with the idea that the costs of distorting beliefs may outweigh the short-lived utility benefits derived from overoptimism if anticipatory utility from overly optimistic beliefs can only be enjoyed over a short period of time. Our study advances this literature by showing that individuals may form overly optimistic beliefs even when it is known that uncertainty will be resolved in the future, as long as the benefits from holding those beliefs can be enjoyed for some time (two weeks in our experiment). Interestingly, we also find that beliefs are not adjusted shortly before uncertainty resolution. This finding may appear surprising because participants were clearly incentivized to form accurate beliefs at the beginning of Session 3 and had little time to enjoy anticipatory utility (within Session 3) before the resolution of uncertainty. However, this result may be reconciled by general preferences for consistency or an increase in adjustment costs of beliefs once these have been explicitly formed and expressed (see also Falk and Zimmermann, 2018). While our design potentially facilitates consistency in beliefs as the screen for the belief elicitation looked very similar in Sessions 2 and 3, general concerns for consistency cannot explain why we observe sticky beliefs significantly more often in the HighScope treatment because general preferences for consistency should not differ across the experimental treatment conditions.[30] Interestingly, about two thirds of participants with sticky beliefs hold a posterior belief of 50%, that is, they seem to completely ignore the signal in both sessions. For participants holding beliefs different from 50%, we suspect that having explicitly formed and expressed their belief in Session 2 may have increased their adjustment costs akin to the findings by Falk and Zimmermann (2018). An alternative explanation for why beliefs do not become more realistic in Session 3 is that participants may *ex post* use their actions to impute the beliefs they must have held when allocating work in Session 2 as in Heidhues et al. (2023). Such behavior will yield optimistic beliefs exactly for those who chose a

[30]We find that 54% of participants in HighScope report the same belief in Session 2 and 3, while only 32% do so in the LowScope condition (χ^2–test: $p < 0.001$).

low workload (which might have been a suboptimal result based on an overly optimistic belief in Session 2) because these are needed to rationalize this choice. Future research may thus investigate whether beliefs are more accurate if they are elicited only once, shortly before the resolution of uncertainty about total workload (speaking to increased adjustment costs) or whether accuracy of beliefs depends on the presence and timing of an action based on them (speaking to the ex-post imputation of beliefs based on actions).

Third, we want to emphasize that our study intentionally uses a parsimonious experimental environment that focuses on motivated reasoning in a single dimension (namely in beliefs about the total workload a decision maker expects to encounter). While this design feature allows us to cleanly identify the role of motivated memory for the belief-based systematic delay of work, many work environments may allow decision makers to form motivated beliefs in multiple dimensions. Apart from the workload itself, people may for example form beliefs about their ability or other aspects that matter for the allocation of work across time. We explicitly abstract from such additional factors. Our unpleasant task is designed such that participants immediately understand the limited role of ability and the trial periods ensure that all participants have a reasonable estimate of the time needed to complete a sequence, to limit motivated reasoning about these additional dimensions. As an extension of our work, future research may study 'motivated procrastination' also in multidimensional settings. This could be done, for example, by exogenously varying both, the scope for motivated memory and the perception of the difficulty of the task (which was kept constant in our setting) or the perceived ability of workers. This could reveal whether 'motivated procrastination' becomes even more prevalent in more complex decision environments, complementing important recent work on the role of complexity for the elicitation of time preferences (Enke et al., 2023).

6 Conclusion

Procrastination can have serious negative effects, including poor savings, neglected exercise plans, and mismanaged workload. While often attributed to inconsistent time preferences or present bias, recent theories suggest that belief-based procrastination also plays a significant role. This study provides first causal evidence on the underlying causes of biased beliefs that may result in a systematic delay of work. In a longitudinal experiment in which participants have to complete a real-effort task of *ex-ante* unknown length, we exogenously vary the signals participants receive regarding their individual workload as well as the scope they have to forget these signals. We document both motivated belief formation through the suppression of negative signals (motivated memory) and a causal link between beliefs about workloads and the systematic delay of work.

Our results advance the understanding of belief-based procrastination and provide important implications for individuals, organizations and policy makers. First, we provide an empirical foundation for how motivated cognition may result in procrastination independent of and potentially in addition to the effect of present bias. We find that motivated memory, i.e, the suppression of negative news when given the scope to forget, appears as key source of overly optimistic beliefs. These overly optimistic beliefs in turn result in a systematic delay of work. As such, motivated memory turns out to be a likely channel that allows decision-makers to delay their work even in environments in which they receive informative signals about how much work they may encounter. Second, these novel insights on the source of biased beliefs in work contexts provide a basis for targeted interventions and an additional rationale for the efficacy of reminders in the context of procrastination (for a discussion see also Ericson, 2017; Altmann et al., 2022).[31] Third, our exploratory analyses show that this causal channel from scope for motivated memory to procrastination is particularly prevalent in individuals who are generally hesitant to acquire information that may be unpleasant. Hence, we identify a group of participants that is particularly 'vulnerable' in environments that allow for negative news suppression and motivated memory. Fourth, our research has broader implications beyond the specific context of work. The action relevance of motivated memory may extend to other areas, such as procrastination in preventive health-care. For example, individuals who are reluctant to learn about negative news regarding their future health may also be more likely to ignore past negative signals regarding their health and, thus, procrastinate costly actions (e.g. healthier lifestyles) that may prevent more severe future health outcomes. Similarly, our findings could apply to insurance or savings contexts, where people may suppress negative past news about potential negative future outcomes which may in turn prevent them from taking action early on (i.e., buying disability insurance or starting saving earlier). Moreover, 'motivated procrastination' may also be at play when it comes to the adoption of energy-saving technology, for instance in the context of residential heating and insulation.

While a straightforward implication of our results is that providing (unavoidable) reminders could lead to strong behavioral changes, the welfare implications of the latter are ambiguous, as the value of belief-based anticipatory utility is difficult to estimate. Future research may seek to address this and intriguing additional research questions. For example, we observe that beliefs are rather sticky once posteriors have been formed. Consequently, it appears crucial to study further to what extent people choose *when* they form their beliefs and to what extent these chosen beliefs react to later changes in the environment or the available information. Further, it is relevant to better understand

[31] Note that although such reminders about signals may cause additional demotivating effects that could result in fewer tasks completed in the present, our experimental results indicate that such potentially countervailing effects are dominated by the disciplining effect on the formation of overly optimistic beliefs.

how quickly individuals can suppress negative news after they have perceived the signals. Effective interventions that mitigate motivated cognition and its adverse consequences need to reach individuals after having received negative news but before they engage in its cognitive suppression. Possible interventions may also benefit from a better understanding of whether the 'memory errors' we observe result solely from negative news suppression in the sense of *positive amnesia* (forgetting a past negative event), or additionally stem from *positive delusion* (fabricating a positive event that did not actually happen), or *positive confabulation* (morphing the memory of a past negative event into a positive memory) as discussed in Chew et al., 2020. Exploring these and related questions will help to develop a comprehensive understanding of the role of motivated memory for the systematic delay of effort, which is pertinent to analyzing the ensuing welfare consequences.

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Appendix

A Additional analyses

A.1 Main results on beliefs based on exact feedback

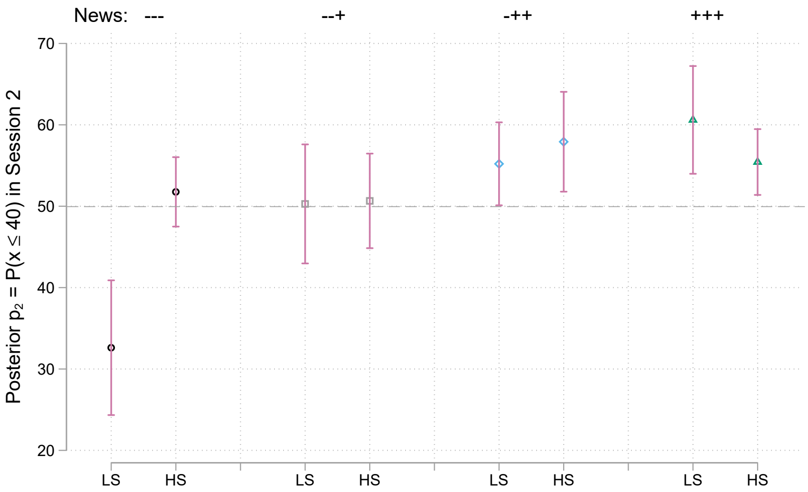


Figure A.1: Posterior beliefs

Notes: The figure shows participants' posterior beliefs in Session 2 (p_2) across treatment conditions and news (very negative: $---$, negative: $--+$, positive: $-++$, very positive: $+++$) received. The pink bars indicate 95%-confidence intervals.

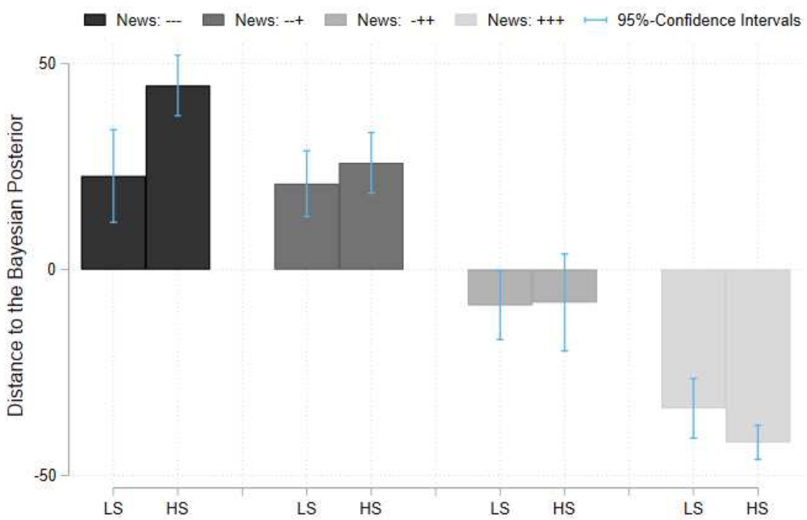


Figure A.2: Distance to the Bayesian posterior

Notes: The figure shows participants' optimism in Session 2 ($p_2 - p_{bay}$) across treatment conditions and news (very negative: $---$, negative: $--+$, positive: $-++$, very positive: $+++$) received. The blue bars indicate 95%-confidence intervals.

Table A.1: Regression results: Effects on posterior beliefs and optimism

Panel A: Posterior beliefs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HighScope	-5.173	-5.523	-5.219	-5.258	-5.155	-5.093	-5.542
	(3.84)	(3.91)	(3.87)	(3.85)	(3.86)	(3.87)	(3.98)
1 neg. signal	-5.396	-5.729	-5.530	-5.342	-5.377	-5.567	-5.921
	(4.14)	(4.19)	(4.21)	(4.14)	(4.19)	(4.17)	(4.30)
2 neg. signals	-10.322**	-10.478**	-10.330**	-10.468**	-10.279**	-10.446**	-10.723**
	(4.89)	(4.93)	(4.91)	(4.90)	(5.02)	(4.91)	(5.09)
3 neg. signals	-27.988***	-28.021***	-28.040***	-27.881***	-27.981***	-28.228***	-28.198***
	(5.26)	(5.24)	(5.28)	(5.27)	(5.27)	(5.20)	(5.22)
HS X 1 neg. signal	7.886	8.333	8.081	7.992	7.869	7.867	8.529
	(5.50)	(5.60)	(5.56)	(5.50)	(5.52)	(5.52)	(5.68)
HS X 2 neg. signals	5.548	5.660	5.557	5.660	5.510	5.485	5.660
	(6.01)	(6.03)	(6.03)	(6.00)	(6.08)	(6.03)	(6.11)
HS X 3 neg. signals	24.318***	24.844***	24.305***	24.288***	24.322***	24.272***	24.792***
	(6.02)	(5.99)	(6.02)	(6.03)	(6.03)	(6.05)	(6.04)
Patience		4.398					4.677
		(5.23)					(5.27)
Procrastination scale			-0.862				-0.439
			(2.13)				(2.20)
Suppression factor				-0.511			-0.563
				(0.82)			(0.83)
Reappraisal factor					-0.062		-0.104
					(1.05)		(1.05)
Information preferences						-1.361	-1.557
						(2.54)	(2.60)
Constant	60.605***	57.608***	63.501***	62.456***	60.867***	64.838***	66.213***
	(3.28)	(4.65)	(8.27)	(4.65)	(5.31)	(8.57)	(11.74)
N	367	367	367	367	367	367	367

** table continues on next page **

Panel B: Optimism

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
HighScope	-8.233**	-7.710*	-8.417**	-8.329**	-8.657**	-8.238**	-8.319*
	(4.14)	(4.20)	(4.20)	(4.15)	(4.16)	(4.14)	(4.28)
1 neg. signal	25.055***	25.552***	24.524***	25.117***	24.595***	25.065***	24.819***
	(5.49)	(5.53)	(5.60)	(5.47)	(5.54)	(5.55)	(5.71)
2 neg. signals	54.577***	54.810***	54.546***	54.414***	53.568***	54.583***	53.777***
	(5.34)	(5.35)	(5.37)	(5.36)	(5.57)	(5.38)	(5.63)
3 neg. signals	56.426***	56.477***	56.217***	56.545***	56.280***	56.439***	56.352***
	(6.65)	(6.68)	(6.65)	(6.61)	(6.68)	(6.55)	(6.60)
HS X 1 neg. signal	8.911	8.243	9.687	9.030	9.301	8.912	9.328
	(8.25)	(8.23)	(8.35)	(8.22)	(8.28)	(8.25)	(8.32)
HS X 2 neg. signals	13.282*	13.115*	13.317*	13.407**	14.174**	13.285*	14.115**
	(6.77)	(6.78)	(6.80)	(6.79)	(6.84)	(6.77)	(6.89)
HS X 3 neg. signals	30.201***	29.415***	30.149***	30.167***	30.107***	30.203***	29.152***
	(7.87)	(7.93)	(7.86)	(7.87)	(7.87)	(7.91)	(7.95)
Patience		-6.564					-7.592
		(7.46)					(7.62)
Procrastination scale			-3.419				-3.142
			(2.64)				(2.81)
Suppression factor				-0.572			-0.405
				(1.12)			(1.18)
Reappraisal factor					1.478		1.447
					(1.90)		(1.93)
Information preferences						0.075	0.676
						(3.80)	(3.87)
Constant	-33.717***	-29.244***	-22.232**	-31.645***	-39.940***	-33.949***	-24.715
	(3.59)	(6.20)	(10.03)	(5.66)	(8.64)	(12.63)	(16.82)
N	367	367	367	367	367	367	367

Notes: The table shows results from OLS regressions. The dependent variable in Panel A is participants' posterior belief about the probability to face low workload (p_2). The dependent variable in Panel B is participants' optimism ($p_2 - p_{\text{bay}}$). The main explanatory variables are the treatment dummy HighScope, dummies for the number of negative signals received and the interaction between the number of negative signals and HighScope. The omitted category are 0 neg. signals (= 3 pos. signals). The control variables are continuous measures resulting from the respective questionnaires. Robust standard errors clustered at the day level reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

A.2 Number of tasks solved in Session 2 (by signals and treatments)

Figure A.3 shows the number of tasks participants choose to complete already in Session 2 by news and scope variation. First, we see that participants choose to work on more tasks after receiving negative news than after receiving positive news. In quantitative terms, positive news lead participants to complete on average 2.72 tasks less in Session 2 than what they would have completed with negative news and this difference is significant (30.95 average tasks after negative news, 28.23 after positive news, t–test $p = 0.023$). Second, participants tend to work less in HighScope (t–test $p = 0.215$), which is in line with our finding that the HighScope treatment induced some individuals to hold highly overoptimistic beliefs.

These findings are in line with the idea that more optimistic beliefs (through positive news and the scope to manipulate beliefs) provide lower incentives to work in Session 2 because these beliefs suggest a lower total work load which, for an individual who wants to smooth consumption as in our theoretical framework, requires fewer tasks to be completed in either session.

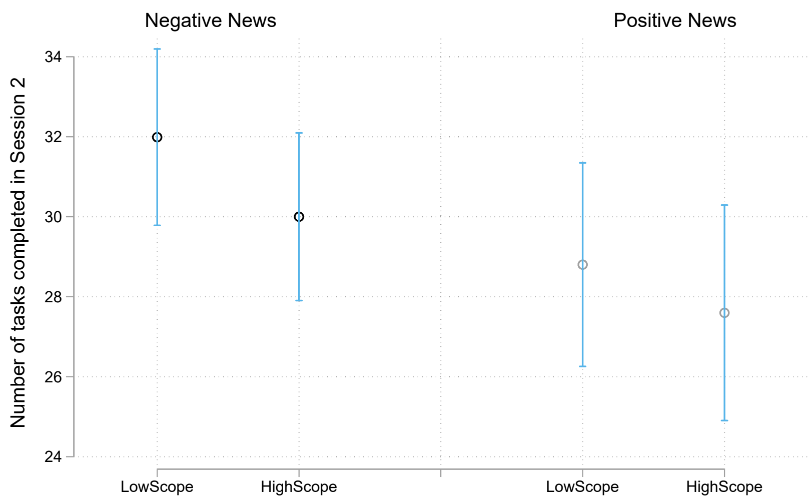


Figure A.3: Work decision in Session 2

Notes: The figure shows participants' work decision in Session 2 across treatment and news. The blue bars indicate 95%-confidence intervals.

A.3 Robustness checks

A.3.1 Alternative priors

Our treatment effect is robust to using rational priors that assign 10% probability to each possible workload instead of using the experimentally elicited subjective priors. In Table

Table A.2: Regression results: Comparison of the treatment effect on optimism with subjective and objective priors

	(1) Benchmark based on subjective priors	(2) Benchmark based on objective priors
HighScope	-5.709	-2.698
	(4.61)	(3.21)
Neg. News	42.295***	55.206***
	(4.58)	(3.55)
HighScope*Neg. News	21.530***	15.339***
	(6.39)	(4.69)
Constant	-20.501***	-27.302***
	(3.05)	(2.25)
N	367	367

Notes: The table shows results from OLS regressions. The dependent variable is participants' optimism ($p_2 - p_{\text{bay}}$). In Column (1), p_{bay} is calculated using the subjective priors that we elicited in Session 1. Column (2) uses the objective prior where every number of tasks is equally likely to calculate p_{bay}. The explanatory variables are the treatment dummy HighScope, a negative news dummy and the interaction. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.2, we show the specification with subjective priors as a benchmark in Column (1) and specification using objective priors in Column (2). The effects are qualitatively similar and only the coefficient for Negative News is significantly different across the two specifications (Wald-test, $p < 0.001$).

A.3.2 Experience with the task

Due to a technical problem a subset of participants did not experience the task in the first session. Therefore, they had to state their beliefs without knowing the exact nature of the task. Specifically, this was the case for all participants from the Berlin-based participant pool in the first wave of data collection. In order to test whether this had an effect on the results, we employ two complementary approaches. First, we include a dummy variable (*No trial*) indicating whether participants experienced this problem (1) or not (0). Second, we run the same regression specifications as in the main text but excluding those participants who were affected. Table A.3 shows the results. Column (1) shows that not being able to gain experience with the task did not significantly affect prior beliefs. The same holds true for posterior beliefs and optimism (see Column (3) and (6)). Further, including the *No trial* dummy does not change the point estimates of our relevant explanatory variables (compare Column (2) and (3) for posterior beliefs and Column (5) and (6) for optimism). The point estimates also remain largely unchanged when we run the regression on the subsample of individuals who did not suffer from the technical error (compare Column (2) and (4) for posterior beliefs and Columns (5) and (7) for optimism) even though this exclusion reduces the sample size substantially (from N=367 to N=277).

Table A.3: RegressionrResults: Controlling for experience with the task

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No trial	1.110		-1.075			-1.667	
	(2.48)		(2.34)			(2.90)	
HighScope		-1.173	-1.295	-0.495	-5.709	-5.899	-3.629
		(2.71)	(2.75)	(3.21)	(4.61)	(4.65)	(5.76)
Neg. News		-16.403***	-16.503***	-15.769***	42.295***	42.141***	40.696***
		(3.54)	(3.56)	(4.19)	(4.58)	(4.62)	(5.83)
HighScope*Neg. News		11.165***	11.376***	10.985**	21.530***	21.857***	21.203***
		(4.31)	(4.38)	(5.11)	(6.39)	(6.46)	(7.98)
Constant	48.679***	57.758***	58.077***	57.391***	-20.501***	-20.006***	-20.214***
	(1.34)	(2.05)	(2.24)	(2.59)	(3.05)	(3.41)	(4.14)
N	367	367	367	277	367	367	277

Notes: The table shows results from OLS regressions. The dependent variable in Panel A is participants' posterior belief about the probability to face low workload (p_2). The dependent variable is participants' prior (p_1) in Column (1), posteriors (p_2) in Columns (2)-(4) and optimism ($p_2 - p_{\text{bay}}$) in Columns (5) - (7). The explanatory variables are a dummy that is 1 if participants did not experience the task prior to belief elicitation (*No trial*), the treatment dummy *HighScope*, a negative news dummy and the interaction. Columns (4) and (7) are based on the restricted sample excluding those participants that did not experience the task prior to belief elicitation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4 repeats the above analyses with respect to the work decision: We find that neither controlling for the technical error (compare Column (2) to (1) and (5) to (4), respectively) nor excluding participants who could not participate in the trial task (compare Columns (3) to (1) and (6) to (4), respectively) has an impact on the relevant point estimates. However, due to the loss in power, the effect of beliefs on the probability to complete the maximally possible 40 tasks is no longer significant, as the exclusion reduces the sample size substantially (from N=367 to N=277).

Table A.4: Regression results: Controlling for experience with the task

	(1)	(2)	(3)	(4)	(5)	(6)
Posterior $\hat{p}_2$	-0.232**	-0.232**	-0.220*	-0.008*	-0.007*	-0.007
	(0.10)	(0.10)	(0.12)	(0.00)	(0.00)	(0.00)
No trial		0.263			-0.075	
		(1.34)			(0.06)	
Constant	41.650***	41.568***	41.001***	0.836***	0.844***	0.835***
	(5.36)	(5.38)	(6.45)	(0.21)	(0.21)	(0.25)
N	367	367	277	367	367	277

Notes: The table shows results from IV regressions using the general method of moments estimator. The posterior belief (p_2) about facing low workload is instrumented with the treatment dummy for HighScope, a dummy for negative news and the interaction of both (for the first stage, see Table 1). The dependent variable in Columns (1) to (3) is the number of tasks participants complete in Session 2. The dependent variable in Columns (4) to (6) is the probability to solve the maximum number of tasks (40) in Session 2. Columns (4) and (7) are based on the restricted sample excluding those participants that did not experience the task prior to belief elicitation. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.4 Participants' characteristics and prior beliefs

Heterogeneity in characteristics may systematically shape participants priors and thus bias the findings reported in Section 4.4. In Table A.5, we regress priors on participants characteristics. We do not find statistically significant relationships between participants characteristics and their priors.

Table A.5: Regression results: Balance in priors

	(1)	(2)	(3)	(4)	(5)	(6)
Patience		4.269				
		(5.39)				
Procrastination			1.144			
			(2.22)			
Suppressionfactor				-0.741		
				(0.89)		
Reappraisalfactor					-1.339	
					(1.27)	
Preferences for information						-2.069
						(2.65)
Constant	48.951***	45.896***	45.180***	51.614***	55.019***	55.209***
	(1.13)	(4.15)	(7.26)	(3.44)	(5.87)	(8.20)
N	367	367	367	367	367	367

Notes: The table shows results from OLS regressions. The dependent variable is participants' prior belief about the probability to face low workload (p_1). The control variables are continuous measures resulting from the respective questionnaires. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

B Experimental material and exclusion

B.1 Screenshots

<u>Your task: Translate a sequence of numbers into a sequence of letters:</u>

During the experiment you will have to solve a transcription task. The task works as follows:

You have to transcribe sequences of 6 numbers into sequences of 6 letters. To do so, an input field is displayed below the sequence of numbers. Here is an example:

12 16 14 16 16 1

You will transcribe the sequence of numbers with the help of a coding key, that assigns a specific letter to each number (see the example below):

Number: 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26

Letter: M Y A L G Z E S K O T H X F I C D P R Q V W U N J B

Your task is to find the corresponding letter for each number and enter the resulting sequence of letters in the input field. Do not enter any spaces between the letters in the input field. Note also, the input field is not case-sensitive. That is, it does not matter whether you enter for example “k” or “K”.

In the above example you are seeing the sequence **"12 16 14 16 16 1".** Given this coding key, the solution is the letter sequence **"HCFCCM".** For this example, we have entered this solution for you in the input field.

12 16 14 16 16 1

HCFCCM

Once you submit a correct code, the computer will prompt you with another sequence.

In case you submit an incorrect code, you will be notified by the computer and have to redo the sequence. To complete the task, you have to transcribe a certain number of sequences. From sequence to sequence, both the number sequence and the coding key change.

You will now have to solve 10 such sequences for practise.

Next

Figure A.4: Explanation of the transcription task

<u>Your current task:</u>

Until now you have correctly transcribed **0 sequences**. This means that 10 sequences are still outstanding.

For each number, enter the appropriate letter from the code table (without spaces).

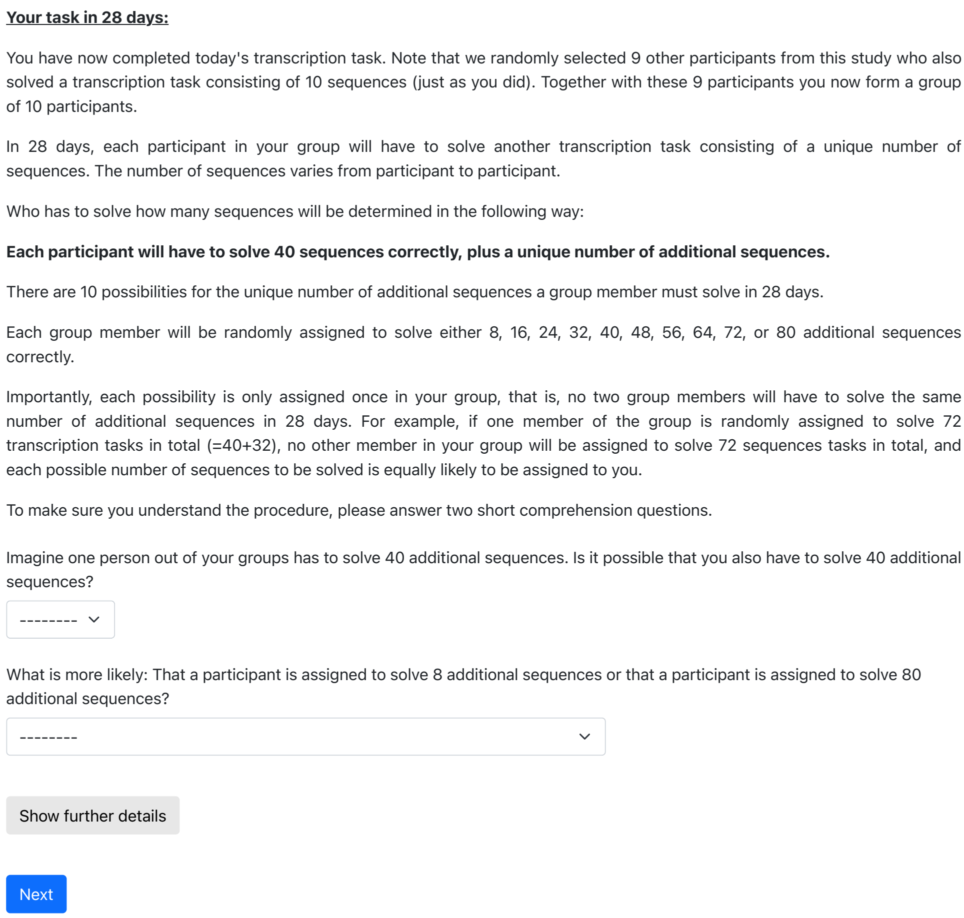
13 16 5 1 18 19

Number: 1 2 3 4 5 6 7 8 9 10 11 12 13 14 15 16 17 18 19 20 21 22 23 24 25 26

Letter: L R T M D S N X G A I K C H Z U J F V Y E W O B P Q

Next

Figure A.5: Example of the transcription task



Your task in 28 days:

You have now completed today's transcription task. Note that we randomly selected 9 other participants from this study who also solved a transcription task consisting of 10 sequences (just as you did). Together with these 9 participants you now form a group of 10 participants.

In 28 days, each participant in your group will have to solve another transcription task consisting of a unique number of sequences. The number of sequences varies from participant to participant.

Who has to solve how many sequences will be determined in the following way:

Each participant will have to solve 40 sequences correctly, plus a unique number of additional sequences.

There are 10 possibilities for the unique number of additional sequences a group member must solve in 28 days.

Each group member will be randomly assigned to solve either 8, 16, 24, 32, 40, 48, 56, 64, 72, or 80 additional sequences correctly.

Importantly, each possibility is only assigned once in your group, that is, no two group members will have to solve the same number of additional sequences in 28 days. For example, if one member of the group is randomly assigned to solve 72 transcription tasks in total (=40+32), no other member in your group will be assigned to solve 72 sequences tasks in total, and each possible number of sequences to be solved is equally likely to be assigned to you.

To make sure you understand the procedure, please answer two short comprehension questions.

Imagine one person out of your groups has to solve 40 additional sequences. Is it possible that you also have to solve 40 additional sequences?

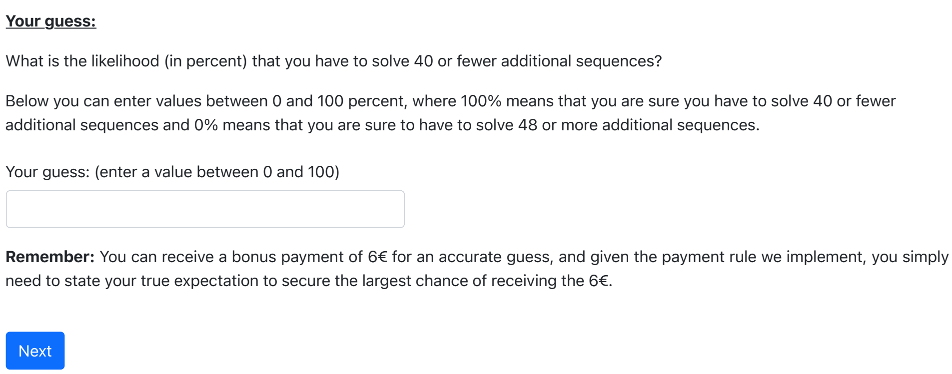
What is more likely: That a participant is assigned to solve 8 additional sequences or that a participant is assigned to solve 80 additional sequences?

Show further details

Next

Figure A.6: Explanations of the work load assignment

Notes: By clicking the button "Show further details" participants could see a paragraph that explained the random draw using a pictorial description.



Your guess:

What is the likelihood (in percent) that you have to solve 40 or fewer additional sequences?

Below you can enter values between 0 and 100 percent, where 100% means that you are sure you have to solve 40 or fewer additional sequences and 0% means that you are sure to have to solve 48 or more additional sequences.

Your guess: (enter a value between 0 and 100)

Remember: You can receive a bonus payment of 6€ for an accurate guess, and given the payment rule we implement, you simply need to state your true expectation to secure the largest chance of receiving the 6€.

Next

Figure A.7: Belief elicitation of p

Guesses about the exact number of additional sequences (40 or fewer):

You indicated that you expect that you have to solve 40 or fewer additional sequences in 28 days with probability 83 percent. Now we would like to know, how you would estimate the likelihood of having to solve a specific number of additional sequences in 28 days.

Please indicate below your estimates

What do you think is the likelihood that you have to solve 8 additional sequences?

What do you think is the likelihood that you have to solve 16 additional sequences?

What do you think is the likelihood that you have to solve 24 additional sequences?

What do you think is the likelihood that you have to solve 32 additional sequences?

What do you think is the likelihood that you have to solve 40 additional sequences?

Important: The sum of your 5 estimates must be equal to your stated probability of having to solve 40 or fewer additional sequences (which you stated as 83 percent)!

Remember: You can receive a bonus payment of 6€ for an accurate guess, and given the payment rule we implement, you simply need to state your true expectation to secure the largest chance of receiving the 6€.

Next

Figure A.8: Belief elicitation of probabilistic beliefs

Remember: Each group member was randomly assigned to solve either 8, 16, 24, 32, 40, 48, 56, 64, 72 or 80 additional tasks in 28 days and no group member will have to solve the same number of additional tasks.

We will now provide you with some information that could be helpful for you in order to better estimate whether you have to solve many or few additional tasks.

We randomly selected 3 out of the 9 other participants from your group. We will now inform you, whether each of these 3 participants must solve more or fewer additional tasks than you in 28 days.

Of the 3 randomly selected participants from your group...

Number of participants that need to solve **fewer** additional tasks: 0

Number of participants that need to solve **more** additional tasks: 3

Next

Figure A.9: Feedback provision

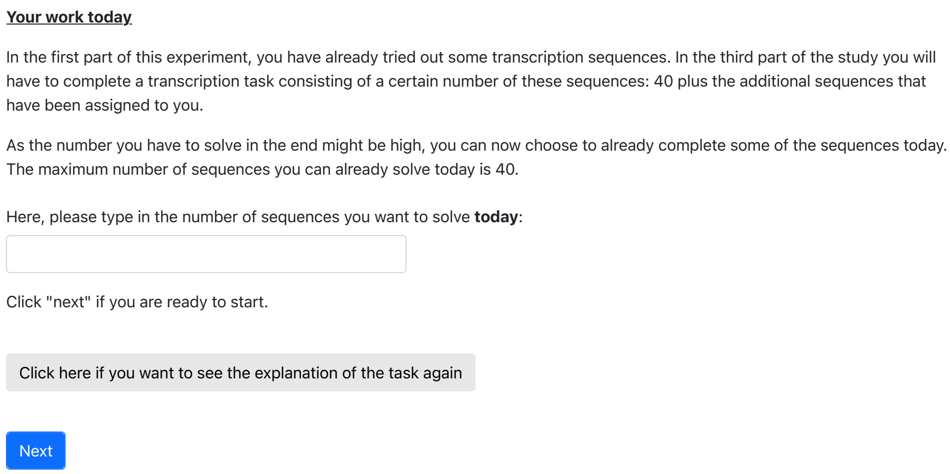


Figure A.10: Work choice

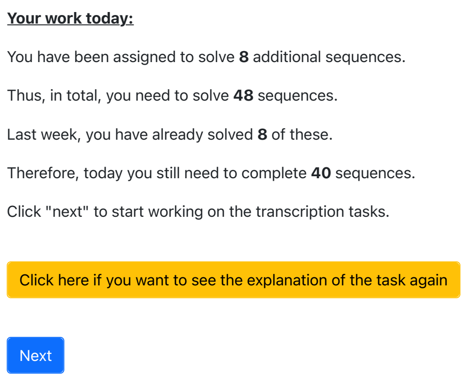


Figure A.11: Work load resolution

B.2 Exclusion and attrition

As specified in our preanalysis plan, we excluded participants who:

- have a low level of English (below 30% on a self-assessment scale from 0 to 100%).
- rushed through the belief elicitation (spend less than 1 minute in total on the three pages related to the explanation of the belief elicitation and incentivization, point-belief elicitation and probabilistic belief elicitation).
- did not pass one of our two attention checks in the first and third session (questions where we asked participants to select one specific value on a Likert-scale)

Figure A.12 provides an overview of when and how participants were excluded (left side) or dropped out (right side). As can be seen, on the left side, very few participants were excluded due to the first two exclusion restrictions. However, in total 41 participants did not pass at least one of the attention checks and were excluded. For the main analysis – as preregistered – we restrict the analysis to those participants that completed all three sessions. Although we do find attrition in all stages of the experiment, attrition is overall relatively low for a longitudinal experiment and importantly, attrition is not selective based on negative news (drop-outs after positive news are indicated by green numbers, drop-outs after negative news by red numbers).

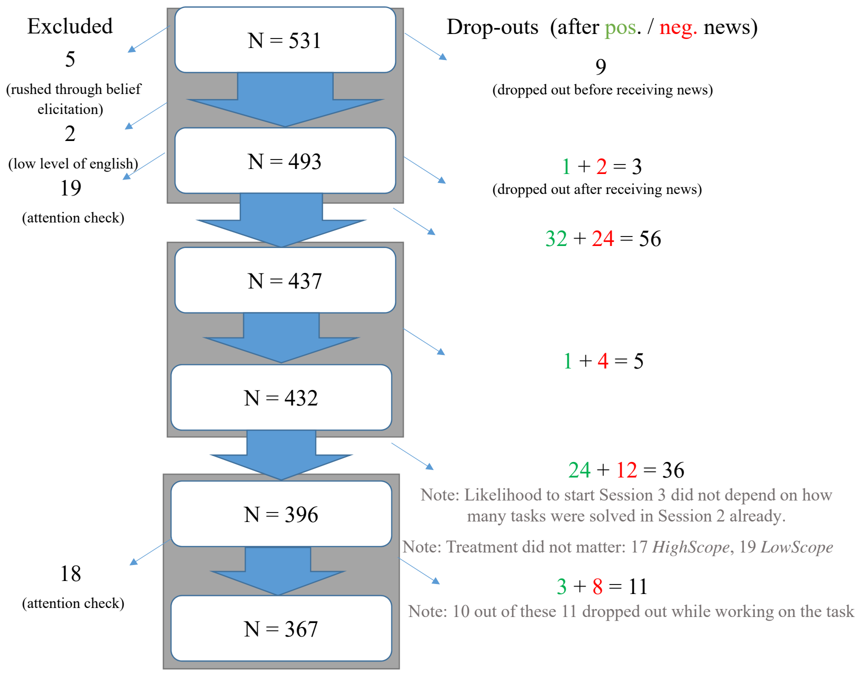


Figure A.12: Exclusion and attrition

Notes: The figure shows participants that were excluded (left side) or dropped out (right side). Gray squares in the Figure represent the three experimental sessions. The number ($N =$) on the top of each gray square indicates how many participants started the respective session while the bottom number indicates how many participants completed the respective session.