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Charlotte Cordes, Jana Friedrichsen, Simeon Schudy

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Motivated Procrastination

Abstract

Procrastination is often attributed to time-inconsistent preferences but may also arise when individuals derive anticipatory utility from holding optimistic beliefs about their future effort costs. This study provides a rigorous empirical test for this notion of 'motivated procrastination'. In a longitudinal experiment over four weeks, individuals must complete a cumbersome task of unknown length. We find that exogenous variation in scope for motivated reasoning results in optimistic beliefs among workers, which causally increase the deferral of work to the future. The roots for biased beliefs stem from motivated memory, such that procrastination may persist even if uncertainty is eventually resolved.

JEL-Codes: C910, D830, D840, D900, D910.

Keywords: anticipatory utility, beliefs, memory, motivated cognition, procrastination, real effort, task allocation.

Charlotte Cordes
Department of Economics
LMU Munich / Germany
charlotte.cordes@econ.lmu.de

Jana Friedrichsen
Institute of Economics
Kiel University / Germany
friedrichsen@economics.uni-kiel.de

Simeon Schudy
Institute of Economics
Ulm University / Germany
simeon.schudy@uni-ulm.de

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1 Introduction

For a long time, it has been argued that procrastination can engender substantial detrimental consequences for individuals and society (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 effort across time (Ariely and Wertenbroch, 2002). 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), that is, as a costly mistake.[1] More recently, belief-based explanations for procrastination have been proposed (Brunnermeier et al., 2017; Breig et al., 2023). Agents may wish to hold optimistic beliefs about their effort costs to enjoy anticipatory utility (as long as total required effort is uncertain) and then fall prey to the planning fallacy (Kahneman and Tversky, 1982). That is, they systematically reduce effort in the present solely based on optimistic beliefs and consequently complete tasks only shortly before important deadlines at high marginal costs (or even miss them). While belief-based explanations for procrastination are appealing and important for welfare considerations, empirical evidence on whether and how agents can sustain optimistic beliefs about their effort costs is missing. In particular, it appears crucial to better understand how agents can form and sustain optimistic beliefs in work environments, when they receive informative signals about their effort costs, and whether optimistic beliefs indeed result in the 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, optimistic beliefs about their effort costs. We term this belief-based delay 'motivated procrastination' as it results only from motivated beliefs about the total effort agents expect to exert and convex effort costs. Motivated procrastination does not require any suboptimal allocation decision conditional on the agents' (wrong) beliefs. Based on wishful thinking (i.e. optimistic beliefs) about the total effort required to complete a task, agents exert less effort in the present and also plan to do so in the future. While this effort allocation appears rational conditional on the agent's belief, the agents decision to reduce effort provision in the present eventually results in higher than expected effort to be exerted in the future, shortly before the deadline (because the agent's beliefs about the total effort required are 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 optimistic beliefs. Previous work on motivated beliefs documents an asymmetry regarding the processing of informative signals which may allow agents to sustain such

[1]For example, present biased individuals may react less to delayed incentives and later regret their effort choice (O'Donoghue and Rabin, 1999; Laibson, 1997; Frederick et al., 2002; DellaVigna and Pope, 2018).

optimistic beliefs. For example, in an environment where agents may form optimistic 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 also sustain optimistic beliefs about effort costs in work environments. Intuitively, agents enjoy greater anticipatory utility the lower their effort costs are. Hence, they may form optimistic beliefs about these costs even when they are exposed to informative signals about the total effort required to complete a task. At the same time, it has also been shown that distortions in updating of ego-relevant beliefs disappear, when uncertainty is known to be quickly resolved (Drobner, 2022). It is thus an open question whether beliefs can cause motivated procrastination in working environments in which uncertainty about the total effort required is eventually naturally resolved.

The current study aims at substantially advancing the understanding of motivated procrastination (i.e., the systematic delay of work based on optimistic beliefs). In a longitudinal experiment, we document the dynamics of motivated beliefs about the total effort required to complete an onerous work-task over a time span of four weeks and study the causal impact of the 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 how much total effort is required to complete a cumbersome transcription task by the end of the third session to receive any payment from the experiment. This provides room for participants to enjoy anticipatory utility by holding motivated, optimistic beliefs about the total effort required to complete the unpleasant task.[2] Second, at the end of the first session, we exogenously vary participants' expectations, henceforth called their beliefs, by sending participants noisy but informative signals about the randomly assigned total effort required to complete the task. Third, two weeks later but before we elicit posterior beliefs (in Session 2), we introduce exogenous variation in the scope that participants have to hold motivated beliefs. While participants across treatments hold the same information and face the same incentives, those assigned to our LowScope condition are reminded of the signal they received two weeks before (in Session 1) while those in the HighScope condition are not reminded. In consequence, HighScope participants face lower costs of suppressing negative news from the past than LowScope

[2]The task consists of an unknown number of sequences of numbers that the participants are required to transcribe into letters according to a coding scheme, which changes for every single sequence (see, e.g., Benndorf et al., 2019). We chose this task as it is unpleasant and causes real effort costs. To render effort costs salient, participants are required to complete ten trial sequences of this cumbersome transcription task at the beginning of the experiment. A majority of our participants (66 percent) consider the task very (34.6 percent) or somewhat (31.5) unpleasant.

participants.[3] Hence, we exogenously introduce scope for motivated memory. Fourth, 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 providing all required effort two weeks later in Session 3. This surprise effort allocation decision allows us to study the action-relevance of optimistic beliefs 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 in a working environment. We document that, independent of potential present bias, negative news suppression can thus indeed cause procrastination. 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. In HighScope, they consider it on average 10 percentage points (24 percent) more likely that the total effort required to complete the task is low (i.e., in the bottom half of possible number of transcription sequences that can be assigned) than in LowScope, 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 the systematic delay of work. We establish a causal relationship between beliefs and effort provided 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 probability of low total required effort leads to completing 7 percent fewer sequences in Session 2 and reduces the likelihood of completing the maximum possible number of sequences in Session 2 by 18 percent.[5] Hence, even though participants receive informative signals and know that uncertainty about the total required effort will be resolved in Session 3, motivated memory allows them to uphold optimistic beliefs about the total effort required to complete the task. Based on these beliefs, they exert less effort in Session 2 and, presumably, also expect to exert less effort in Session 3. However, because participants' beliefs are systematically biased, they eventually have to exert more effort than

[3]Our novel approach was inspired by work that has documented the suppression of negative news through 'motivated memory' in non-work-related decision environments (see, e.g., Zimmermann, 2020; Gödker et al., 2021).

[4]As participants do not know that part of the work 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 bias their beliefs upwards as a consequence of the opportunity to delay work to the future (see also Bénabou and Tirole, 2016; Brunnermeier et al., 2017; Bönisch et al., 2024).

[5]We obtain qualitatively similar results when using propensity score matching to study whether participants in HighScope complete fewer tasks in Session 2 than participants in LowScope given the same prior and signals (see Section 4).

expected in Session 3. That is, they systematically delay work as compared to participants holding more accurate beliefs, providing clean evidence for motivated procrastination.

Our work contributes to several strands of the literature and provides important implications for theory and policy (see also Section 7). In particular, our work establishes a direct link between the literature on potential sources of procrastination and the literature on motivated beliefs. First, we contribute to a novel literature that studies how alternative (contextual) factors may lead to procrastinatory behavior and thereby complements the traditional view of procrastination as a result of time-inconsistent preferences (for a detailed review, see Ericson and Laibson, 2019). Acknowledging these alternative factors provides possible explanations for repeated procrastination and can challenge the revealed preference approach of measuring intertemporal preferences. For example, the presence of an excuse is a contextual factor that can induce procrastinatory behavior by fostering present bias or by reducing the emotional costs of postponing work (Drucker and Kaufmann, 2022; Lepper, 2022). Factors that complicate the identification of time preferences from procrastinatory behavior can arise through time-varying costs (Heidhues and Strack, 2021) and through complex environments, in which choice behavior may arise that can be confused with time-inconsistent behavior (Enke et al., 2023).

Our approach differs from work that posits procrastination as preference-driven. Instead, we contribute to the nascent strand of this literature that studies belief-based procrastination. Theoretical work on belief-based procrastination provides a rationale for procrastination rooted in the anticipatory utility derived from expecting a low workload in the future. Brunnermeier et al. (2017) develop a model in which agents may rationally hold optimistic beliefs about their effort costs 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 regarding the sources of such motivated beliefs and the causal link between them and procrastination behavior is missing.

A rare and important contribution that indirectly sheds light on the role of beliefs for procrastination is the work by Breig et al. (2023). Using a clever experimental design, the authors study 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) in that we exogenously vary 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 the total effort required to complete an onerous task, we shed new light on the emergence of optimistic beliefs. Further, our exogenous variation in the effort required to complete the task and signals received by our participants allows us to establish a direct link between beliefs and procrastination without the need to rely on information about the participants' past procrastination behavior.[6]

Importantly, our study also provides direct evidence that procrastination can persist when agents receive informative signals, as long as they have scope to forget or suppress information they received.[7] 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. The workload participants face in their study, however, was deterministic. Uncertainty about the actual effort required to complete a task – which is a realistic assumption in most real-life settings – and the resulting motivated beliefs about the latter may explain why we still often observe procrastination despite potential room for such learning processes.

Second, we contribute to the literature on motivated belief formation. Specifically, our longitudinal study of motivated reasoning in a work context complements the literature on the dynamics of motivated reasoning and memory errors in other environments. Zimmermann (2020) finds that people form motivated beliefs by suppressing negative news about their performance in an IQ related test over the course of roughly a month.[8] 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 that increasing scope for memory failures increases the suppression of negative news in a work environment. When workers worry about high effort costs (as they do in our unpleasant transcription task), they wish to ignore negative

[6]Note that our heterogeneity analyses (see Section 5) suggest that the link between beliefs and procrastination does neither depend on a participant's general tendency to procrastinate, nor on emotion regulation strategies, which have been stressed as a potential origin of procrastination by psychologists (see Pychyl and Sirois, 2016).

[7]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).

[8]In Zimmermann (2020), this tendency to suppress negative news could be mitigated by increasing incentives, suggesting that these news are not completely deleted from the memory.

news about the total effort required to complete the task. Additionally, we document the action relevance of such motivated beliefs, by identifying a causal relationship between beliefs and procrastination.

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 earlier. 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, 2024), the resolution of uncertainty (Drobner, 2022), responsibility (Bosch-Rosa et al., 2023), anxiety motives (Engelmann et al., 2024), or incentives (Zimmermann, 2020; Gödker et al., 2021), we vary the scope for holding motivated beliefs by varying whether or not participants are reminded of signals they received two weeks earlier. This approach holds information, risk, anxiety, and incentives constant but still affects the extent of motivated reasoning.

From a policy perspective, our results provide an important input into the debate about the welfare costs of procrastination. In preference-based models, policy interventions that reduce procrastination and impose time consistency are viewed as welfare-improving from the individual perspective. In contrast, if procrastination is driven by wishful thinking, as we demonstrate, procrastination may be optimal from an individual welfare point of view. Specifically, agents may rationally trade off the increased aggregate effort costs of backloading work tasks with the increased savoring and better psychological well-being of being hopeful about future workload. This perspective also offers additional positive predictions. For example, an agent whose procrastination and apparent naiveté are an optimizing response that trades off psychological and material incentives, may be reluctant to commit their future self to a more frontloaded work schedule. Thus, motivated procrastination can explain low uptake of commitment, but it may also change how we think about its welfare effects. Most importantly, while policies involving commitment may constitute Pareto improvements when procrastination causes negative externalities and is preference-driven (e.g. in case of present bias), these may not be feasible when procrastination results from motivated reasoning because such policies would harm individual's belief-based utility.

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 additional exploratory findings relating to heterogeneous treatment effects as well as methodological aspects of our approach. In Section 6, we discuss the broader implications of our findings. Section 7 concludes.

2 Experimental design

2.1 Overview

To study the dynamics of motivated beliefs about effort costs 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 effort required to complete a cumbersome task that is independent of participants' innate tendencies to procrastinate. Second, we exogenously vary participants' expectations about the effort they are required to exert thereby inducing variation in participants expected effort costs. Third, we manipulate participants' scope to hold motivated beliefs about how much work needs to be completed by reminding or not reminding them about an informative signal they have received two weeks before.[9] 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 time-line of the experiment and the main contents of the three consecutive sessions that the participants must complete to receive payment.[10]

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 six numbers to be transcribed to letters with the help of a coding key (see Appendix Figure A.5). Once a participant has entered the six 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. To familiarize participants with the task, they need to complete 10 practice sequences in Session 1. We thereby ensure that participants are aware of the fact that the task is unpleasant and involves real effort costs. The total number of sequences is randomly assigned and *ex-ante* unknown to participants. Specifically, 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 actual realization (and

[9] Apart from the reminder in Session 2, the LowScope and HighScope condition were identical. Hence, there was no upfront announcement of the reminder which could have potentially led to different attention or different use of memory enhancing tools across treatments.

[10] 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 data was collected in two waves (to achieve our preregistered sample size). The data collection included an additional experiment run in parallel (with different participants) on the dynamics of motivated reasoning in an ego-relevant environment (akin to the work by Zimmermann, 2020) which we discuss in a companion paper. We obtained IRB approval from the ethics committee at LMU Munich (Project 2022-05) and the project has been preregistered at `https://aspredicted.org/SHS_XD6`.

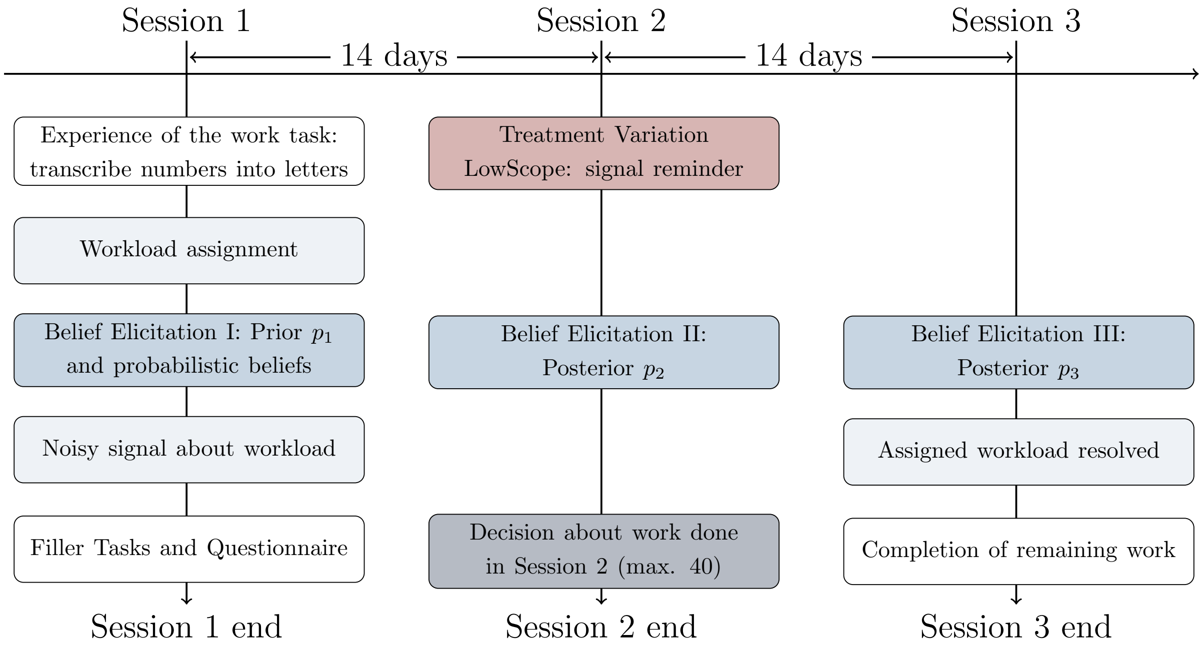

Figure 1: Timeline of the experiment

thus the total number of sequences to be completed) remains unknown to participants until Session 3. Therefore, rational priors allocate a fifty percent probability to facing a high workload ($x_i > 40$) and equal chance to any particular workload x_i. Using this rational prior belief, we can derive the corresponding rational Bayesian posterior for each participants using the individual signal they received. In order to address concerns that differing individual priors may influence the analysis, we explicitly elicit participants' prior beliefs regarding the additional workload in Session 1 in two steps. First, participants have to state their subjective probability of having to solve at most 40 additional tasks ($p_1 = Pr(x_i \leq 40)$). Second, we ask participants how likely they consider each additional workload out of the 10 possible workloads (8, 16, ... 80).[11] We incentivize the belief elicitation using the binarized scoring rule with a prize of €6 paid for one randomly chosen belief elicitation (Hossain and Okui, 2013).[12] 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 possible workloads that have not been assigned to them are higher (lower) than their as-

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

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

signed workload.[13] Finally, participants complete a series of additional (filler) tasks which obscure the purpose of the experiment and provide additional insights for our analyses.[14]

In Session 2, we elicit participants' subjective posterior probability of having 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.[15] 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 that participants received affect their procrastination behavior (see subsection 2.2). 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., optimism in reported beliefs cannot result from a participant's decision not to work in Session 2), both of which would make identification fail. After participants have completed the number of sequences they committed to, Session 2 ends.

At the beginning of Session 3, we again elicit participants' posterior subjective probability of having to solve at most 40 additional tasks ($p_3 = Pr(x_i \leq 40)$). Then, participants complete several psychological questionnaires.[16] Finally, participants learn how many additional sequences were assigned to them and complete the remaining number of sequences, taking into account the number of sequences completed already in Session 2. After they have completed their total workload, participants are informed about their payments. Recall that no payment is made if a participant does not complete this last session. That is, participants forfeit any payment even if they have completed all ques-

[13] 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.

[14] 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.

[15] 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. Further, in both the HighScope and the LowScope condition, less than 3% of our participants complete zero tasks in Session 2, suggesting that potential set-up costs play only a minor role in our setting.

[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).

tionnaires but failed to solve all sequences they had been assigned (which is common knowledge).

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 about their assigned workload (LowScope) or not (HighScope) at the beginning of Session 2 before they report their posterior. Those participants in LowScope who receive a reminder are shown exactly the same signal as in Session 1.[17] 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 biased updating. Hence, differences across treatments identify the causal effect of motivated memory on beliefs in an environment in which agents receive informative signals about their potential workload.

In addition to the variation in scope for motivated reasoning, our design creates exogenous variation in the beliefs that participants hold through the signal they receive. 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 are higher than the workload assigned to the participant – to very negative – all of the three non-assigned workloads are lower than the workload assigned to the participant. As the workloads are assigned randomly, prior beliefs are uniform and the signals are exogenous. Thus, we can use the exogenous variation in beliefs (through signals) to study whether participants react asymmetrically to positive and negative news.

2.3 Procedures

The longitudinal online experiment was programmed in oTREE (Chen et al., 2016). To ensure that we would be able to achieve our pre-registered sample size, we recruited participants via ORSEE (Greiner, 2015) in parallel from the student subject pools of the Munich Experimental Laboratory of Economic and Social Sciences (MELESSA) and the TU-WZB-laboratory in Berlin in two waves (Wave 1: June-July 2022, Wave 2: October-November 2022). In the morning of the session day, each participant received an individual

[17] Following Zimmermann (2020) and to ensure that signals were seen by participants, participants had to manually re-enter the signal shown to them on their screen, both in Session 1 and, if applicable, after the reminder.

link to the online interface of the experiment. All tasks that had to be completed within a given session were explained there. To remain in the study and qualify for final payment, participants had to complete a session by 10pm of the day when they received the link. They were informed about this requirement and the exact dates of all three sessions at the recruitment stage. Following our preregistration, our final sample excludes 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 then chooses how many tasks to solve on the first date. All remaining tasks will have to be solved on the second date; the work decision cannot be revised.

3.1 Belief formation

In line with our experiment, we focus on the belief that the assigned workload is low, where 'low' refers to all workloads in the set $\mathcal{L} = \{x : x \leq \frac{1}{2}\max(x)\}$. Suppose the agent starts out with a prior belief p_1, which is her subjective probability of being required to exert relatively little effort, e.g., having to complete $x \in \mathcal{L}$. For a rational agent, this prior belief corresponds to $p_1 = 0.5$. The agent then receives a signal which can be negative or positive, $s \in \{-1, 1\}$. A negative signal tells the agent that it is more likely than chance

[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. Importantly, we did not observe selective attrition based on negative news. In the Appendix, we provide a comprehensive overview of these exclusion criteria and attrition (see Appendix Figure A.13). 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 allowed to complete the online sessions until 10pm of the session day so that this time includes all breaks and interruptions.

that she has been assigned a relatively high workload ($x \notin \mathcal{L}$), whereas a positive signal suggests a relatively low workload of $x \in \mathcal{L}$ is more likely.

A Bayesian agent with perfect memory will incorporate the signal into her assessment and form a posterior belief about the assigned workload being low according to her prior belief and Bayes' rule. This Bayesian posterior $p_{\mathcal{B}}$ is uniquely defined for any prior and signal received. However, an agent's belief may differ from this rational Bayesian benchmark for two reasons.[20] 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.

When forming beliefs, 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 not only from the cognitive effort of maintaining incorrect beliefs but also from the incentivization because participants are incentivized to report accurate beliefs in the sense of matching the objective Bayesian posterior. More generally, distorting beliefs may also reduce utility due to a suboptimal work decision that the agent takes based on her incorrect beliefs. This cost component, however, is muted in our experiment because agents do not know that they will be able to distribute their workload across Sessions 2 and 3 but believe that they will complete everything in Session 3.[21] We model the agent's problem as follows:

$$\max_{\hat{p}} \quad v(\hat{p}) - f(p_{\mathcal{B}}, \hat{p}, \gamma), \tag{1}$$

where $v(\hat{p})$ describes the anticipatory utility from holding belief $\hat{p}$, which is the chosen subjective posterior probability assigned to facing a low effort in the future, and $f(\cdot)$ describes the costs associated with choosing this possibly distorted posterior which depend on the Bayesian posterior $p_{\mathcal{B}}$ and a parameter γ that captures individual and situational differences in the ease of distorting beliefs. We assume that anticipatory utility is twice differentiable, weakly concave and increasing in the belief that workload is low, i.e. $\partial v(\hat{p})/\partial \hat{p} > 0$, $\partial^2 v(\hat{p})/\partial \hat{p}^2 \leq 0$. The costs $f(\cdot)$ associated with holding the belief $\hat{p}$ are increasing and convex in the distance of the chosen belief from the Bayesian posterior and equal zero if the subjective belief $\hat{p}$ and the Bayesian posterior coincide. Further, we assume that it is less costly for individuals to distort their beliefs if they have more scope γ to do so.[22] In our context, scope is varied via the presence of a reminder with

[20] We focus on reasons related to utility from beliefs. For a comprehensive overview of alternatives to Bayesian updating, see Ortoleva (2022).

[21] 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.

[22] Specifically, we assume a twice-differentiable cost function with $f(p_{\mathcal{B}}, p_{\mathcal{B}}, \gamma) = 0, \partial f(p_{\mathcal{B}}, \hat{p}, \gamma)/\partial \hat{p} > 0$ and $\partial^2 f(p_{\mathcal{B}}, \hat{p}, \gamma)/\partial \hat{p}^2 > 0$ and $\partial^2 f(p_{\mathcal{B}}, \hat{p}, \gamma)/\partial \hat{p}\partial\gamma < 0$ for $\hat{p} \geq p_{\mathcal{B}}$. For $\hat{p} < p_{\mathcal{B}}$, $\partial f(p_{\mathcal{B}}, \hat{p}, \gamma)/\partial \hat{p} < 0$, $\partial^2 f(p_{\mathcal{B}}, \hat{p}, \gamma)/\partial \hat{p}^2 > 0$ and $\partial^2 f(p_{\mathcal{B}}, \hat{p}, \gamma)/\partial \hat{p}\partial\gamma > 0$. As such, our general formulation includes a more

$\gamma_{HS} > \gamma_{LS}$. Intuitively, memory imperfections that arise more strongly in the absence of the reminder increase the dispersion of the agent's belief about her future work load, which reduces her cognitive costs of biasing beliefs.

Solving the agent's maximization problem yields the following first order condition

$$\frac{\partial v(\hat{p})}{\partial \hat{p}} = \frac{\partial f(p_{\mathcal{B}}, \hat{p}, \gamma)}{\partial \hat{p}} \tag{2}$$

which implicitly defines the optimal subjective belief $\hat{p} \geq p_{\mathcal{B}}$. Implicit differentiation yields the observation that the individual belief distortion, which optimally trades off the anticipatory utility from expecting a rather low effort with the costs of deviating from the rational update, increases in the scope parameter γ.[23] This observation gives rise to 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 more optimistic.*

Using data from our experiment, we can explicitly test Hypothesis 1 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.

Our prediction and analyses focus on beliefs as the outcome. As we manipulate beliefs through the channel of memory, our approach is also consistent with empirical findings in the context of ego-relevant environments, in which negative news are remembered with a lower probability than positive news (see Chew et al., 2020). Note also, that our modeling approach is agnostic about the exact stage at which memory eases the belief distortion: Beliefs may become distorted at the encoding stage akin to the model by Hagenbach and Koessler (2022), who model how agents with psychological utility (of which anticipatory utility is one example) may want to distort their memory by manipulating the probability to recall negative signals. But the belief distortion may also occur only at the recall stage, when information is retrieved from memory in a possibly biased way.

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specific (and often used) formulation in which utility $v(\hat{p})$ is linear in $\hat{p}$ and psychological costs from holding inaccurate beliefs are modeled with a quadratic loss function, such as: $v(\hat{p}) - \frac{1}{\gamma}\frac{1}{2}(|p_{\mathcal{B}} - \hat{p}|)^2$.

[23] For a given value of γ, the first-order condition (2) implicitly defines $\hat{p}(\gamma)$. Totally differentiating this implicit function and rearranging, we obtain

$$\frac{d\hat{p}}{d\gamma} = \frac{\partial^2 f(p_{\mathcal{B}}, \hat{p}, \gamma)/\partial \hat{p} \partial \gamma}{\partial^2 v(\hat{p})/\partial \hat{p}^2 - \partial^2 f(p_{\mathcal{B}}, \hat{p}, \gamma)/\partial \hat{p}^2}.$$

By the weak concavity of $v(\cdot)$ and convexity of $f(\cdot)$ in $\hat{p}$, the denominator is negative. As we have assumed that the marginal cost of distorting the belief decreases in γ, the numerator is negative so that the total expression is positive.

3.2 Work decision

Akin to our experimental setting, we assume that after learning the signal and forming her (potentially motivated) belief the agent learns that she can split her work between two dates (today and a later date). Taking the agent's subjective posterior $\hat{p}$ as given, the agent now maximizes her expected utility by allocating tasks of the expected total workload across the two possible working dates. We denote work allocated to the first date by w_1 (in the experiment w_1 equals the number of transcription sequences completed in Session 2). The remaining work (w_2) needs to be completed by the agent on the second date (in the experiment w_2 is completed two weeks later in Session 3). We assume that the agent discounts the future with a discount factor $\delta \in (0, 1]$ and incurs convex effort costs $c(w)$ from working. Thus, when deciding how much to work at the first date, an agent with the subjective belief $\hat{p}$ (regarding the probability that her workload is low) solves the following cost minimization problem:

$$\min_{w_1} \quad c(w_1) + \delta \mathbb{E}_{\hat{p}}[c(w_2)] \qquad s.t. \quad w_1 + \mathbb{E}_{\hat{p}}[w_2] = b + \mathbb{E}_{\hat{p}}[x], \tag{3}$$

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 tasks to be solved for completion of the total workload. 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 $\mathbb{E}_{\hat{p}}[c(b + x - w_1)] = \hat{p}c(b + w_L - w_1) + (1 - \hat{p})c(b + w_H - w_1)$. The optimal allocation of tasks to the first date w_1 as the solution to the minimization problem (3) is characterized by the following first order condition:

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

From the assumption that effort costs are convex in the number of tasks w, it follows that the right-hand side (the discounted expected marginal cost of future effort) decreases 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 marginal effort cost today (left-hand side), that is in a decrease of the workload allocated to the first date, w_1. Intuitively, an agent who does not discount the future (i.e., $\delta = 1$), wants to evenly spread her expected total work between the two work dates (due to convex effort costs). Therefore, if the agent expects total effort to be lower, she expects to work less on both dates, and ends up working less on the first date (but likely needs to work more than expected on the second date if her belief $\hat{p}$ is optimistic). Thus, the expectation about the assigned workload x affects the decision how much work to defer to the future in a very intuitive way:

Hypothesis 2. *Agents who hold more optimistic beliefs regarding the realization of x (and thus their total workload) will complete fewer tasks immediately.*

To test Hypothesis 2 we will exploit the exogenous variation in signals and scope for motivated reasoning to instrument beliefs and study the role of instrumented beliefs on the number of tasks completed in Session 2.

4 Main results

Following our simple theoretical framework and the empirical approach of Zimmermann (2020), we code news into a binary variable *Neg. News* that only distinguishes between negative and positive news. *Neg. News* is an indicator variable which takes the value one 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*, *Neg. News* is zero and indicates positive news, when at least two of the drawn non-assigned possible realizations of x are bigger than the assigned x_i.[24]

4.1 Motivated beliefs, updating and imperfect recall

First, we analyze participants' subjective posterior probabilities p_2 of having to solve at most 40 additional tasks, i.e. $Pr[x_i \leq 40]$, which we elicited in Session 2, two weeks after the signal was initially received. Figure 2 shows the average of these posterior beliefs split up by negative versus positive news across our two treatment conditions. The horizontal dashed line marks the rational prior probability of 50 percent, which corresponds to 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, when they are reminded of the signal before stating their belief inLowScope, their posterior belief of facing low workload is substantially lower (p-value = 0.003, t–test).[25] With positive news, instead, we observe very similar posteriors in HighScope and LowScope (p-value = 0.669, t–test).[26]

[24]In Appendix A.1, we present qualitatively similar results from analyses using non-simplified feedback.

[25]Note that this result is not caused by a general "optimism bias" in participants' memory, i.e. by some general tendency to memorize signals that convey a low likelihood for the event of interest differently, as we do not observe such an asymmetry in a task that involves no wishful thinking (the dot-spot task, see also our additional analyses in Appendix A.5.

[26]Additional analyses in Appendix A.1 reveal that the difference between LowScope and HighScope after negative news is entirely driven by 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).

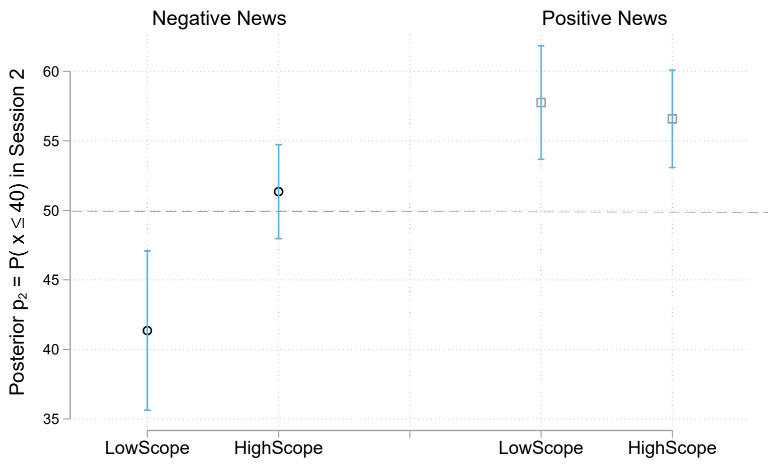

Figure 2: Posterior beliefs

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.

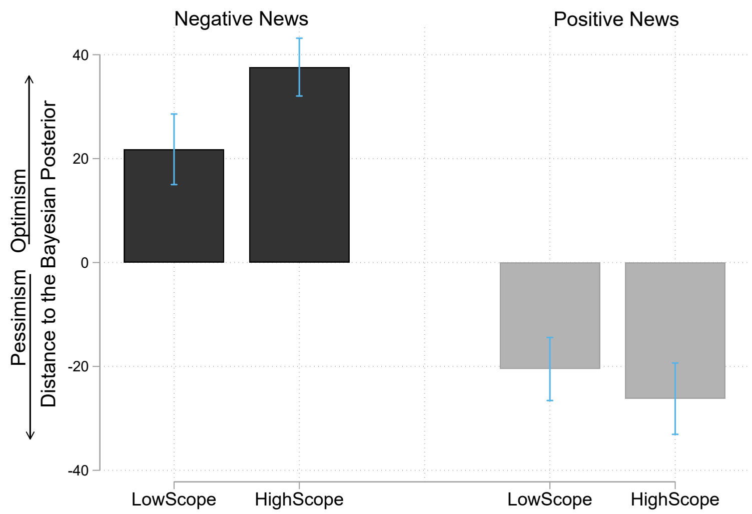

Figure 3: Optimism

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

Next, we compute the distance between participants' elicited posterior p_2 and their Bayesian Posterior based on each participant's elicited prior and signal received ($p_2 - p_{\mathcal{B}}$). Doing so allows us to judge whether beliefs are optimistic (or pessimistic).[27] Figure 3 shows that, on average, participants update conservatively, i.e., compared to the Bayesian benchmark, they are optimistic after negative news and pessimistic after positive news.[28] Comparing participants' optimism by news across treatments mirrors our previous find-

[27] In Appendix A.3.1, we present an analysis based on rational instead of elicited priors which underlines the robustness of our results.

[28] 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

ing. Participants who received negative news (left panel) are substantially more optimistic when assigned to the HighScope condition than when assigned to the LowScope condition (p-value < 0.001, t–test). In HighScope, the distance between an individual's stated posteriors and the individualized Bayesian posterior is on average 19 percentage points larger than in LowScope. For positive news in contrast, the scope for motivated reasoning does not substantially alter participants' pessimism (right panel, p-value $= 0.215$, t–test).

These findings are also confirmed by the regression analyses presented in Table 1. In Panel A, Column (1), we regress a participant's posterior belief p_2 on our treatment indicator HighScope, a dummy for negative news (*Neg. News*) and their interaction. We subsequently add standardized control variables in Columns (2) to (7) showing that 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.

Specifically, 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 a measure of preferences for information using the scale by Ho et al. (2021) (Column (6)). The specification in Column (7) includes all these control variables. In Panel B, we repeat this approach focusing on participants' optimism given by $p_2 - p_{\mathcal{B}}$ as the dependent variable. As can be seen, our results are robust and indicate strong reactions to HighScope when participants received negative news. That is, the effect of our exogenous scope variation on beliefs does not change when controlling for variation in participants' time preferences, emotion regulation strategies, and information preferences. In line with Hypothesis 1, we summarize these findings in Result 1.

Result 1. *Scope for motivated reasoning results in negative news suppression and thus in substantial optimism about total effort costs.*

(Augenblick et al., 2023). Akin to the findings in Thaler (2020) and Barron (2021), in LowScope, we do not observe motivated reasoning in updating towards good news.

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		0.821					0.824
		(1.01)					(1.02)
Procrastination scale			-0.256				0.086
			(1.19)				(1.23)
Suppression factor				-1.050			-1.177
				(1.08)			(1.09)
Reappraisal factor					0.463		0.481
					(1.12)		(1.13)
Pref. for Information						-0.329	-0.554
						(1.12)	(1.13)
Constant	57.758***	57.735***	57.749***	57.830***	57.827***	57.773***	57.914***
	(2.05)	(2.06)	(2.06)	(2.06)	(2.07)	(2.06)	(2.09)
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		-1.691					-1.923
		(1.65)					(1.68)
Procrastination scale			-2.232				-1.944
			(1.55)				(1.65)
Suppression factor				0.187			0.245
				(1.57)			(1.62)
Reappraisal factor					2.119		2.084
					(2.13)		(2.16)
Pref. for Information						-1.248	-0.835
						(1.77)	(1.79)
Constant	-20.501***	-20.452***	-20.584***	-20.513***	-20.188***	-20.445***	-20.190***
	(3.05)	(3.06)	(3.05)	(3.07)	(3.08)	(3.06)	(3.13)
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 standardized 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.

4.2 The causal effect of beliefs on the allocation of work

Finally, we provide evidence on the action relevance of beliefs and show that more optimistic beliefs eventually result in the systematic delay of work. To do so, we study how exogoneous variation in beliefs affects the number of tasks participants complete already in Session 2. In line with the idea that participants may be heterogeneous in their time-preferences and opportunity costs of time in Session 2 and 3, there is substantial heterogeneity in how many tasks participants complete in Session 2 (mean = 29.67, sd = 11.49).[29] To identify the causal role of beliefs for procrastination, we exploit the exogenous variation in beliefs induced by the news and scope condition a participant was randomly assigned to. This variation is by design orthogonal to participants' opportunity costs of time and time-preferences and thus suitable for an instrumental variables (IV) approach.[30] The first stage of our IV approach coincides with Column (1) of Panel A in Table 1, in which we regress participants' posterior beliefs p_2 on treatment assignment (indicator for HighScope), the news received (indicator *Neg. News*), and the interaction of these two variables. Table 2 then shows the second stage of the IV approach, in which we explain the participants' work decisions with the instrumented beliefs. The exclusion restriction of this approach thus relies on the assumption that signals and HighScope do not directly affect work decisions but beliefs do, which appears plausible given our setting.

We first analyze the work decision based on the the number of sequences completed in Session 2 and then the likelihood of a participant completing the maximally possible number of 40 tasks in Session 2. Our results in Table 2 reveal that a 10 percentage point increase in (instrumented) posteriors, and thus 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 standardized control variables related to time preferences, the tendency to procrastinate, emotion regulation and information preferences. Again, we find that our point estimates are robust to including these control variables.

We complement the results from the IV approach with additional analyses using a propensity matching approach. The idea of the latter is to analyze whether participants

[29]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. We imposed this maximum of 40 tasks to ensure that participants would have to complete at least some tasks in Session 3 and to prevent participants from working on more tasks than they were assigned to.

[30]While we observe meaningful adjustments in the raw data, i.e., in the number of tasks due to news and scope (see Appendix A.2), the IV approach allows us to abstract from any variation of work allocation that is caused by other factors than beliefs (e.g. by differences in participants' opportunity costs of time or similar endogenous variables).

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		-0.056					-0.126
		(0.65)					(0.65)
Procrastination scale			-0.247				-0.150
			(0.71)				(0.74)
Suppression factor				-0.023			-0.036
				(0.64)			(0.65)
Reappraisal factor					0.654		0.652
					(0.71)		(0.72)
Pref. for Information						-0.109	-0.094
						(0.70)	(0.71)
Constant	41.650***	41.534***	41.677***	41.643***	41.301***	41.623***	41.139***
	(5.36)	(5.45)	(5.36)	(5.34)	(5.32)	(5.31)	(5.32)
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		-0.002					-0.003
		(0.03)					(0.03)
Procrastination scale			-0.007				-0.002
			(0.03)				(0.03)
Suppression factor				-0.034			-0.034
				(0.03)			(0.03)
Reappraisal factor					0.008		0.009
					(0.03)		(0.03)
Pref. for Information						0.009	0.005
						(0.03)	(0.03)
Constant	0.836***	0.836***	0.837***	0.829***	0.831***	0.844***	0.826***
	(0.21)	(0.22)	(0.21)	(0.21)	(0.21)	(0.21)	(0.21)
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 measured on a scale from 0 - 100 (p_2) about facing low workload is instrumented with the treatment dummy for HighScope, a dummy for negative news and the interaction of both. The dependent variable in Panel A is the number of tasks participants complete in Session 2. The dependent variable in Panel B is the probability to solve the maximum number of tasks (40) in Session 2. The control variables are standardized continuous measures resulting from the respective questionnaires. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

in HighScope complete fewer tasks in Session 2 than participants in LowScope given the same prior and signals using a non-parametric matching strategy. Specifically, we match subjects on their priors (p_1) and on whether they received positive or negative news. Thus, we compare individuals in HighScope and LowScope with the same prior and signal. Table 3 presents the results of this analysis and reports the average treatment effects of HighScope on the number of sequences completed in Session 2 in Panel A. In columns (1) and (2), propensity scores are estimated using a Logit model. In Columns (3) and (4), we present the results obtained when estimating propensity scores with a Probit model. Independent of the exact modeling approach, the findings indicate that being in the HighScope condition lowers the number of tasks completed in Session 2 by around 2.3-2.4. Panel B shows the effect of HighScope on the probability to complete the maximum number of tasks. HighScope reduces the latter by 11.9-12.5 percentage points. In summary, the above results are in line with Hypothesis 2. We thus conclude with Result 2.

Result 2. *More optimistic beliefs reduce the number of tasks completed immediately, resulting in a systematic delay of work.*

Table 3: ATE of HighScope on the work decision

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

	Logit		Probit	
	1 Neighbor	2 Neighbors	1 Neighbor	2 Neighbors
	(1)	(2)	(3)	(4)
ATE				
HighScope	-2.351**	-2.317**	-2.351**	-2.317**
	(1.20)	(1.16)	(1.20)	(1.16)
N	367	367	367	367

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

	Logit		Probit	
	1 Neighbor	2 Neighbors	1 Neighbor	2 Neighbors
	(1)	(2)	(3)	(4)
ATE				
HighScope	-0.119**	-0.125**	-0.119**	-0.125**
	(0.05)	(0.05)	(0.05)	(0.05)
N	367	367	367	367

Notes: The table shows results from propensity score matching. The analysis matches individuals on priors (p_1) and signals (neg. news) using nearest neighbor matching, with replacement when >1 neighbor. The dependent variable in Panel A is the number of tasks participants complete in Session 2. The dependent variable in Panel B is the probability to solve the maximum number of tasks (40) in Session 2. Columns (1) and (2) use 1 neighbor, while Column (2) uses 2 neighbors. Columns (1) and (2) use logit while Columns (3) and (4) use probit to estimate propensity scores. Abadie-Imbens robust standard errors clustered at the day level reported in parentheses, and *** p<0.01, ** p<0.05, * p<0.1.

5 Additional results

5.1 Dynamics of belief distortions

One may expect that a wishful thinker states optimistic beliefs in Session 2 to enjoy belief-based utility from being optimistic about the effort level required to complete the task in Session 3. At the same time one may also expect her to revise these beliefs downwards in Session 3, when she is incentivized to report an accurate belief, because there is little additional anticipatory utility to be enjoyed in Session 3 before the realized task number is resolved. This reasoning would suggest that posteriors become more realistic from Session 2 to Session 3. However, it appears also plausible that forming and stating an optimistic belief increases a decision-maker's adjustment costs (see also Falk and Zimmermann, 2018), particularly when she based her effort decision on this explicitly stated belief in Session 2. Consequently, we may see little adjustment in optimism from Session 2 to 3 even though the relevance of anticipatory utility likely vanishes close to the end of the experiment.

Figure 4 illustrates optimism in Sessions 2 and 3 across treatment and news condition and highlights that, indeed, participants' optimism ($p_2 - p_{\mathcal{B}}$) hardly changes from Session 2 to 3 (mean in Session 2: 4.95, mean in Session 3: 5.94, $p = 0.444$, t–test).[31] Furthermore,

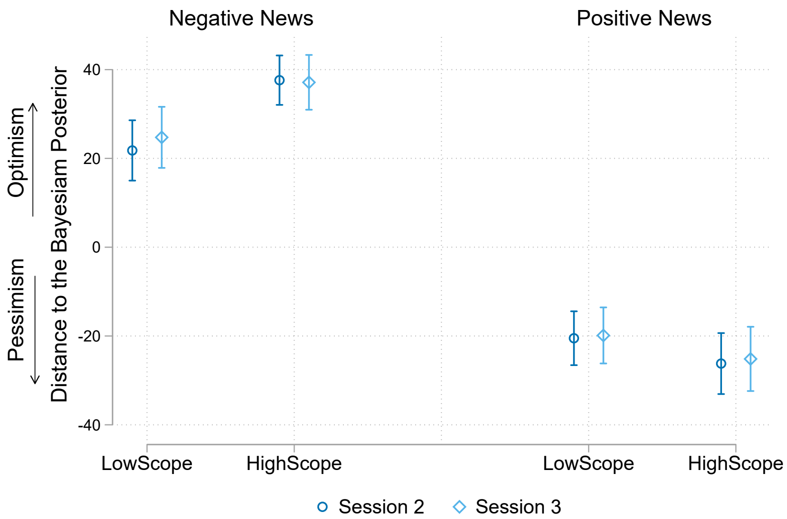

Figure 4: Belief dynamics

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

[31] This result is caused by the minor change in mean posteriors (mean in Session 2: 52.58, mean in Session 2: 51.57). This hold also for participants who received negative news in LowScope (for which the difference in optimism appears visually slightly larger, t-test, $p = 0.342$), as well as for participants who received negative news in HighScope (t-test, $p = 0.856$), or positive news ($p = 0.613$ in HighScope, $p = 0.797$ in LowScope).

we find that 42.5% of our participants exhibit “sticky beliefs”; they state exactly the same expectations in Session 2 and Session 3 ($p_2 = p_3$). This stickiness may have different explanations. First, our design potentially facilitates consistency in the sense of stating identical beliefs across sessions because the decision screen for the belief elicitation looked very similar in Sessions 2 and 3. However, a general concerns for consistency *per se* cannot explain why we observe sticky beliefs significantly more often in the HighScope than in the LowScope treatment.[32] About two thirds of those participants with “sticky beliefs” hold a posterior belief of 50% and thereby seem to completely ignore the signal in both sessions. The remaining third of participants with “sticky beliefs” who hold beliefs different from 50% have invested cognitive resources to form and express their belief in Session 2, which may have increased their adjustment costs in Session 3 akin to the findings by Falk and Zimmermann (2018). Pushing this line of thought further, this result may hint to a belief formation process in which beliefs are internalized once they are formed. In a subsequent belief elicitation, the internalized belief will simply be retrieved instead of the participant forming a new one. Indeed, rational individuals have no reason to adjust their beliefs if there is no new information.

Another explanation for why beliefs do not become more realistic in Session 3 is that participants may use their actions to *ex post* 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 participants who chose a low workload (which might have been a suboptimal result based on an optimistic belief in Session 2) because these are needed to rationalize this choice.[33] .

Among those who do adjust their beliefs in Session 3 (67%), we observe beliefs moving upwards (31%) and downwards (26%) to approximately the same extent. This finding is consistent with the presence of two opposing mechanisms: On the one hand, the importance of anticipatory utility decreases as the revelation of actual workloads approaches and this will make beliefs more realistic.[34] On the other hand, the upcoming revelation of workloads may increase the salience of the possibly extreme outcomes and, thereby, heighten anxiety or worries about the uncertain outcome. Affected participants may feel a stronger need to adopt “wishful thinking” (Engelmann et al., 2024) to deal with their

[32] 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: $p < 0.001$).

[33] In the theory by Heidhues et al. (2023), 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. That is, optimistic beliefs in Session 3 may simply appear consistent to participants who chose to work rather little in Session 2. Importantly, the latter intuition cannot explain the observed asymmetry in posteriors due to negative news in HighScope in Session 2

[34] 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 (Drobner, 2022).

emotions. Alternatively, the minor changes in beliefs from Session 2 to 3 may also emerge from noise due to imperfect memory.

5.2 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 with respect to these variables. We focus on optimism ($p_2 - p_{\mathcal{B}}$) as the outcome variable for these analyses, as it avoids potential biases due to imbalances in priors and signals across different subgroups of smaller size.

5.2.1 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 differences 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 Table 4 and show that neither time preferences nor the tendency to procrastinate are strong mediators of overoptimism resulting from motivated memory. 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

Table 4: 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 ($p_2 - p_{\mathcal{B}}$). 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

are very similar to the one from the full sample in Column (1) for both subgroups (Chow-test, $p = 0.911$). Hence, our data suggests time preferences are not a relevant mediator of belief adjustments based on scope for negative news suppression. Columns (4) and (5) in Table 4 show that the effect is also robust when splitting the sample by the participants' tendency to procrastinate. The coefficients on the interaction term for both subgroups and the full sample are almost identical (Chow-test, $p = 0.998$), suggesting that procrastinatory tendencies do not mediate the effect either.

5.2.2 Emotion regulation

The inclination to distort beliefs about the likelihood of unpleasant events may depend on an individual's ability and strategies to cope with negative emotions (see, e.g. Engelmann et al., 2024) and psychologists have proposed emotion regulation as a potential cause for the systematic delay of work (see Pychyl and Sirois, 2016). 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 5 reports the results of these additional exploratory analyses, again including the benchmark specification in Column (1). We find that individuals with an above

Table 5: Regression results: Heterogeneity in optimism with respect to emotion regulation

	(1)	(2)	(3)	(4)	(5)
	Full sample	High suppr.	Low suppr.	High reappr.	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 ($p_2 - p_{\mathcal{B}}$). 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

median tendency to inhibit their emotion-expressive behavior (suppression) 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, the difference between the two coefficients fails to be statistically significant at conventional levels (Chow-test, $p = 0.1085$). Notably, as shown in Column (3), participants who are less likely to suppress emotion-expressive behavior are also more likely to be too pessimistic when not being reminded of positive news in HighScope.[35] Thus, they seem to forget signals in general and are more likely to hold posterior beliefs close to their priors. In contrast, we find no apparent heterogeneity when we split the sample based on participants' reappraisal strategies (see Columns (4) and (5), Wald-test: $p = 0.8725$).

5.2.3 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

[35]The HighScope coefficient for positive news differs across the two sub-samples (Chow-test, $p = 0.0508$).

Table 6: 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 ($p_2 - p_{\mathcal{B}}$). 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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

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 6 reports the results of this analysis, again including the benchmark specification in Column (1) and for a median split with respect to the strength of information preferences in Column (2) and (3). We find that information preferences appear indeed relevant for motivated memory. For participants with a strong preference for information revelation (Column (2)), there is a weak and statistically insignificant interaction effect of HighScope and negative news (11.823). Instead, participants with weaker preferences for information revelation are substantially (27 percentage points) more likely to be more optimistic in HighScope than in LowScope after receiving negative news (Column (3)). These findings suggest that participants who generally tend to avoid information are also more likely to form optimistic beliefs based on negative news suppression, although we fail to statistically reject the equality of the estimated coefficients across the two sub-samples (Chow-test: $p = 0.197$).

6 Discussion

Our findings in Section 4 reveal a causal link from scope for motivated reasoning (through motivated memory) to optimistic 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 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 optimistic beliefs. In this section, we discuss an important design element that allowed us to empirically identify motivated procrastination. We further comment on the dynamics of motivated beliefs in our setting and discuss the importance of studying motivated reasoning in more complex, multidimensional work environments in future research.

Our 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, only after eliciting their posterior beliefs in Session 2, we announced the possibility to complete some of the assigned work immediately at the end of Session 2. Doing so, we prevent two potential biases in the elicited beliefs that would hinder the clean identification of motivated beliefs: pessimistic beliefs that serve as a commitment[36] and optimistic beliefs that serve as a justification for procrastinating.[37] This important element of our experimental design thus ensures that we can 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 in the sense of the systematic delay of work based on optimistic beliefs because 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 particular workload and have been randomly reminded (or not) about the signal regarding their total workload. Hence, if beliefs of individuals

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

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

who have been randomly assigned to the LowScope condition are systematically more optimistic after negative news than those of individuals assigned to the HighScope condition, and exogenous variation in beliefs about the total workload causally affects the number of tasks completed in Session 2, then 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.

In addition to studying the relationship of motivated memory, optimistic beliefs, and the systematic delay of work, we also shed 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 individuals do not form motivated beliefs when the resolution of uncertainty is immediate. The underlying idea is that the costs of distorting beliefs may outweigh the short-lived utility benefits derived from overoptimism if anticipatory utility from optimistic beliefs can only be enjoyed over a short period of time. Our study advances this literature by showing that uncertainty resolution does not generally preclude motivated beliefs. In our experiment, individuals formed optimistic beliefs although they knew that uncertainty would be resolved in the future. Apparently, the benefits from holding optimistic beliefs for two weeks in our experiment were sufficient to trigger a motivated belief distortion.

Finally, we intentionally use a parsimonious experimental environment that focusses on motivated reasoning in a single dimension (namely in beliefs about the total workload a decision maker expects to encounter). While this approach 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 forming motivated beliefs about the total workload, decision makers may for example form motivated beliefs about their ability, potential future distractions, or other factors that matter for the allocation of work across time. We explicitly abstract from such additional factors and purposefully designed our experiment to limit motivated reasoning in additional dimensions. For example, the trial period for the unpleasant task ensures that experimental participants understand the limited role of ability for task completion and allows them to form reasonable expectations about the time needed to complete a single sequence. As an extension of our work, future research may study motivated procrastination in multidimensional settings, for example by exogenously varying both the scope for motivated memory and another relevant dimension such as the perception of the difficulty of the task. Such a study could reveal whether the systematic delay of work becomes even more prevalent in more complex decision environments and thereby deepen our understanding of motivated procrastination.

7 Conclusion

For individuals and society at large, procrastination may have negative consequences including poor savings, neglected exercise plans, and mismanaged workload. While often attributed to inconsistent time preferences or present bias, more recent theories proposed that procrastination may be caused by motivated, optimistic beliefs. This study provides the first direct evidence of the underlying cause of such beliefs. It shows that motivated memory allows decision makers to form biased beliefs which ultimately result in a systematic delay of work. Such belief-based procrastination can exist independently of preference-based tendencies to procrastinate and appears similarly important for more and less patient participants.

Our results advance the understanding of belief-based procrastination and provide important insights for individuals, organizations and policy makers. First, we offer 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 a key source of optimistic beliefs. These optimistic beliefs in turn result in the systematic delay of work. 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 to curb procrastination (for a discussion see also Ericson, 2017; Altmann et al., 2022).[38] Third, our exploratory analyses show that the causal chain from scope for motivated memory to procrastination is particularly prevalent among individuals who are generally hesitant to acquire possible unpleasant information. Hence, we identify a group of participants who appear particularly susceptible to motivated procrastination and thus highly relevant to be considered when design policies or considering welfare effects. 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, delay costly actions (e.g. healthier lifestyles) that may prevent more severe future health outcomes.[39] 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). More-

[38] Although such reminders may in principle also have additional demotivating effects leading to fewer tasks being completed in the present, our experimental results indicate that such potentially countervailing effects are dominated by the disciplining effect on the formation of optimistic beliefs.

[39] Relatedly, Roth et al. (2024) show that misperceptions in the form of pessimism about the effectiveness of therapy cause low therapy take-up.

over, 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 limiting the scope for motivated cognition by 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 observed 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 appears important to better understand how quickly individuals can suppress negative news after they have perceived them. Effective interventions supposed to mitigate motivated cognition and its potentially 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 'memory errors' in working environments 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 for ego-relevant environments 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 (for online publication)

A Additional analyses

A.1 Main results on beliefs based on exact feedback

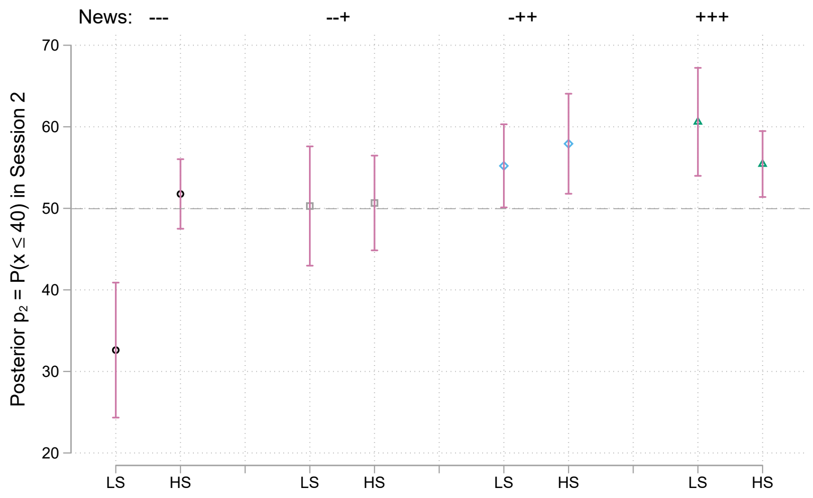

Figure A.1: Posterior beliefs

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.

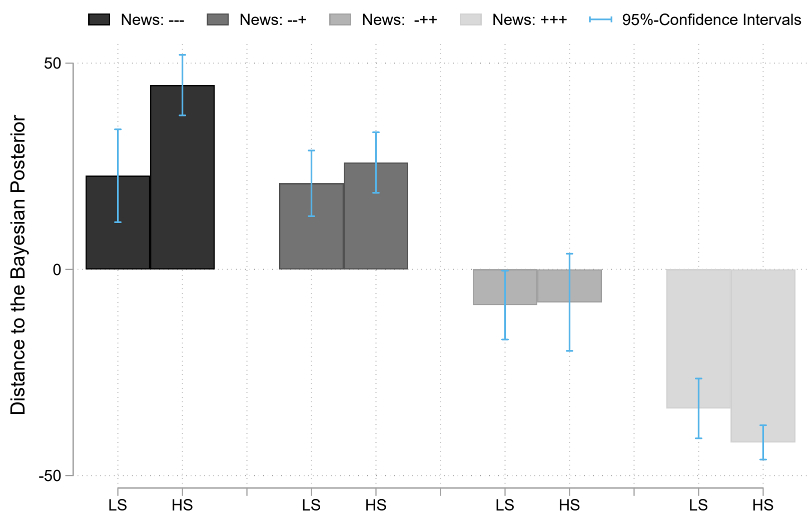

Figure A.2: Distance to the Bayesian posterior

Notes: The figure shows participants' optimism in Session 2 ($p_2 - p_{\mathcal{B}}$) 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		0.884					0.940
		(1.05)					(1.06)
Procrastination scale			-0.471				-0.240
			(1.16)				(1.20)
Suppression factor				-0.630			-0.693
				(1.01)			(1.02)
Reappraisal factor					-0.067		-0.111
					(1.13)		(1.13)
Pref. for Information						-0.599	-0.686
						(1.12)	(1.14)
Constant	60.605***	60.755***	60.658***	60.619***	60.584***	60.721***	60.907***
	(3.28)	(3.30)	(3.32)	(3.29)	(3.33)	(3.31)	(3.42)
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		-1.319					-1.526
		(1.50)					(1.53)
Procrastination scale			-1.867				-1.715
			(1.44)				(1.53)
Suppression factor				-0.705			-0.500
				(1.38)			(1.46)
Reappraisal factor					1.587		1.553
					(2.04)		(2.07)
Pref. for Information						0.033	0.298
						(1.67)	(1.70)
Constant	-33.717***	-33.941***	-33.506***	-33.701***	-33.239***	-33.723***	-33.363***
	(3.59)	(3.60)	(3.67)	(3.60)	(3.67)	(3.64)	(3.80)
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 ($p_2 - p_{\mathcal{B}}$). 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 standardized 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

In this section, we briefly discuss the raw data of the number of tasks solved in Session 2. First, we find that participants choose to work on more tasks after receiving negative news than after receiving positive news. Positive as compared to negative news lead participants to complete on average 2.72 tasks less in Session 2 (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 (28.92 vs. 30.41, t–test $p = 0.215$), which is in line with our finding that the HighScope treatment induced some individuals to hold optimistic 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 exert effort in Session 2 because these beliefs suggest a lower total required effort work to complete the task.

A.3 Robustness analyses

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 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 Neg. News (and the constant) are significantly different across the two specifications.[40]

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
	(4.6)	(3.4)
Neg. News	42.295***	55.206***
	(4.45)	(3.29)
HighScope*Neg. News	21.530***	15.339***
	(6.33)	(4.68)
Constant	-20.501***	-27.302***
	(3.17)	(2.34)
N	367	367

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

[40]Constant: $p = 0.006$, Neg. News: $p < 0.001$, HighScope: $p = 0.339$, Interaction: $p = 0.160$; Chow-test.

A.3.2 Experience with the task

In the first wave, due to a technical problem participants from the Berlin-based participant pool did not have to complete the ten trial sequences of the transcription task in Session 1. Nevertheless, these participants read the instructions for the transcription task (see Figure A.5). To test whether the lack of task experience alters our results, we employ two complementary approaches. First, we include a dummy variable (*No trial*) indicating whether participants did not experience the trial phase. Second, we run the same regression specifications as in the main text but exclude those participants who had no trial experience. Table A.3 presents the results of these additional analyses. Column (1) shows that not participation in the trial (*No trial*) 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 participated in the trial (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, see Column (6)) .

Table A.3: Regression Results: 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 ($p_2 - p_{\mathcal{B}}$) 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 number of sequences 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)
No trial		0.263			-0.075	
		(1.34)			(0.06)	
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)
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 5.2. 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		0.858				
		(1.08)				
Procrastination scale			0.624			
			(1.21)			
Suppression factor				-0.913		
				(1.10)		
Reappraisal factor					-1.437	
					(1.37)	
Pref. for Information						-0.911
						(1.17)
Constant	48.951***	48.951***	48.951***	48.951***	48.951***	48.951***
	(1.13)	(1.13)	(1.13)	(1.13)	(1.13)	(1.13)
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 standardized continuous measures resulting from the respective questionnaires. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

A.5 The dotspot task

Besides the main experimental design, our setup also included a belief formation and memory task regarding a neutral topic: The number of red/blue dots shown to participants in a graph (see Figure A.3). In the first session, we showed participants a graphic consisting of a total of 400 dots for 8 seconds. We randomized i) whether the majority of the dots was red or blue (35% and 65%, respectively) and ii) whether we asked subjects to estimate the percentage of red or blue dots. In the second session, we again asked the people for the percentage of red (or blue) dots. Before doing so, in LowScope, the graph appeared again on participants' screen while in HighScope the graph was not shown again. Participants' guesses regarding the number of dots of a particular color was incentivized using the binarized scoring rule (in both sessions). Figure A.3 shows an example of participants' screen with a graph including 35% red dots.

This task allows us to examine whether, when being asked about the probability of an event to occur, participants tend to forget or misremember signals indicating a low likelihood of that event more easily than signals suggesting a high likelihood (here: many of the dots were of the color of interest / in the main experiment: signal about a high likelihood of being assigned a low number of sequences).

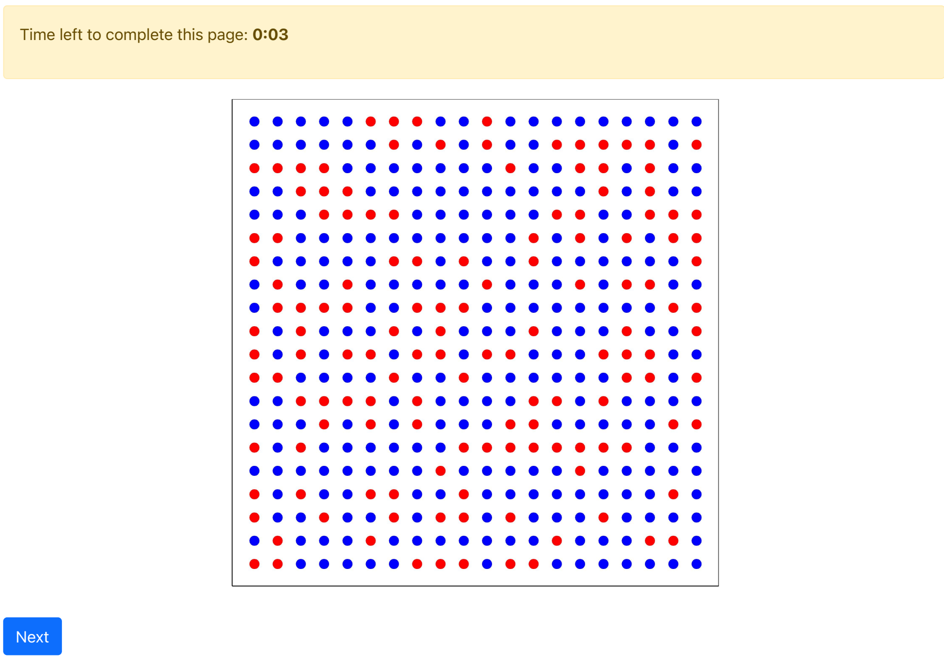

Figure A.3: Screen with the dotspot task

Table A.6: Regression results: Absolute difference between guessed percentage of dots in Session 1 and 2 in the dotspot task

	(1)
HighScope	3.637***
	(1.40)
ManyDots	0.660
	(1.39)
HighScope× ManyDots	2.226
	(1.97)
Constant	6.096***
	(0.97)
N	367

Notes: The table shows results from OLS regressions. The dependent variable is the absolute difference between the percentage entered between Session 1 and 2. The independent variables include the HighScope dummy, a dummy that indicates whether the majority of the dots was in the color that was asked for (ManyDots) as well as the interaction of these two. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A.6 shows the results of a regression analysis, where we regress the absolute difference between the percentage entered between Session 1 and 2 on the HighScope dummy, a dummy variable that indicated whether the majority of the dots was in the color that was asked for (ManyDots) as well as the interaction of these two. Naturally, the absolute difference is bigger in HighScope, where individuals need to rely on their memory. However, we see that there is no differential effect depending on ManyDots. In other words, we observe no different memory effects for news that convey a high likelihood of the event of interest occurring (as compared to a low likelihood) in a neutral task. Consequently, the asymmetry in optimism after negative news (a signal for a low likelihood of the event considered) in HighScope in the main experiment does not stem from a general memory effect but results from participants' wishful thinking about low total workload.

For completeness, we further shed light on the dynamics of beliefs in the dot-spot task by focusing on the distributions of the difference between the percentage entered in Session 1 and 2. Figure A.4 plots these differences separately by scope and fraction of dots in the color that was asked for. While in LowScope this difference is approximately normally distributed around 0, in HighScope beliefs dynamics are in line with regression to the mean in a symmetric manner: Participants who saw a dotspot with 35% dots of the color asked for similarly moved towards stating 50% as did participants who saw a dotspot with 65%. The latter findings underline our previous conclusion.

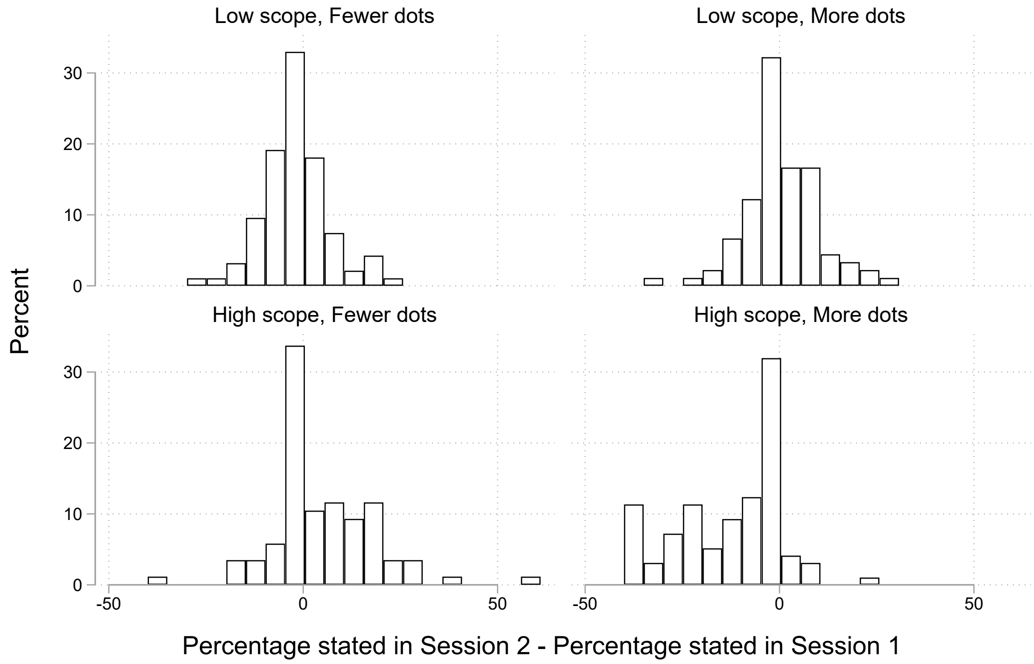

Figure A.4: Histogram of belief differences in the dotspot task

B Experimental material and exclusion

B.1 Screenshots

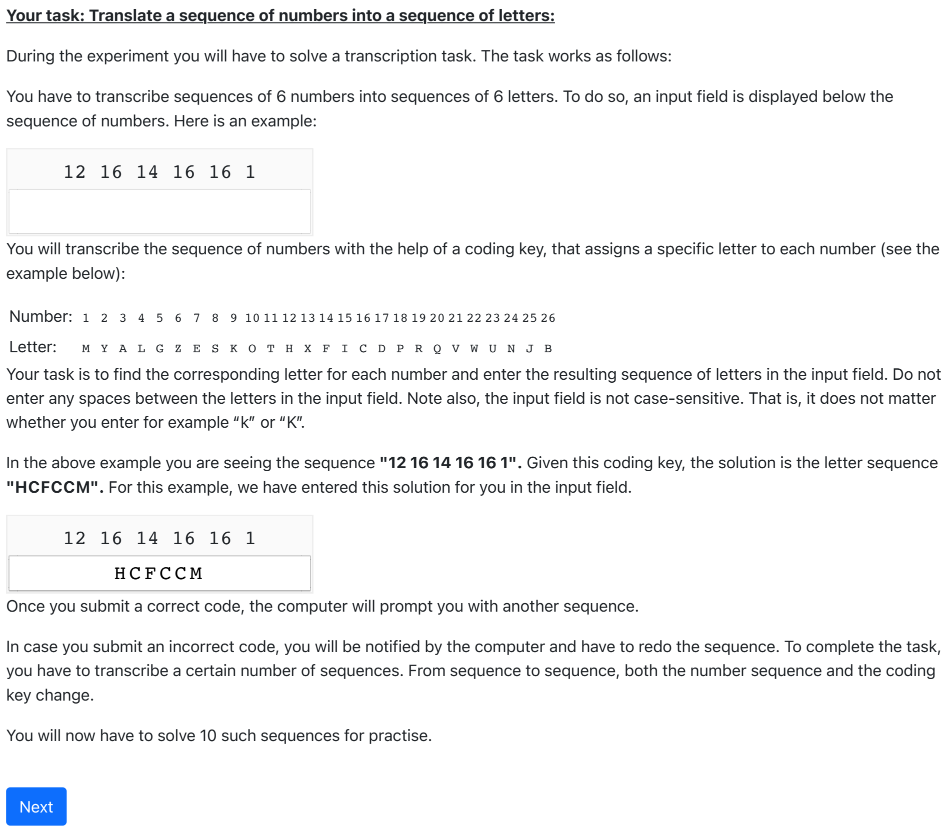

Figure A.5: Explanation of the transcription task

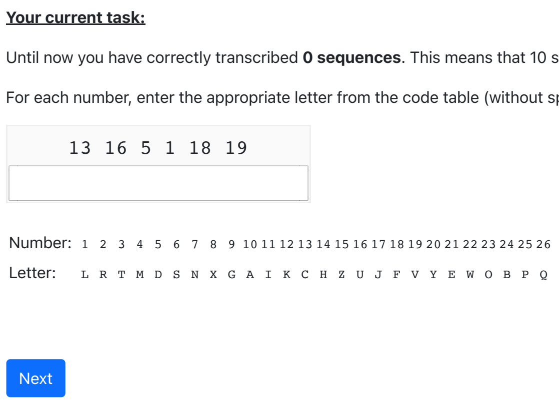

Figure A.6: Example of the transcription task

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.7: 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.8: 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.9: 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.10: Feedback provision

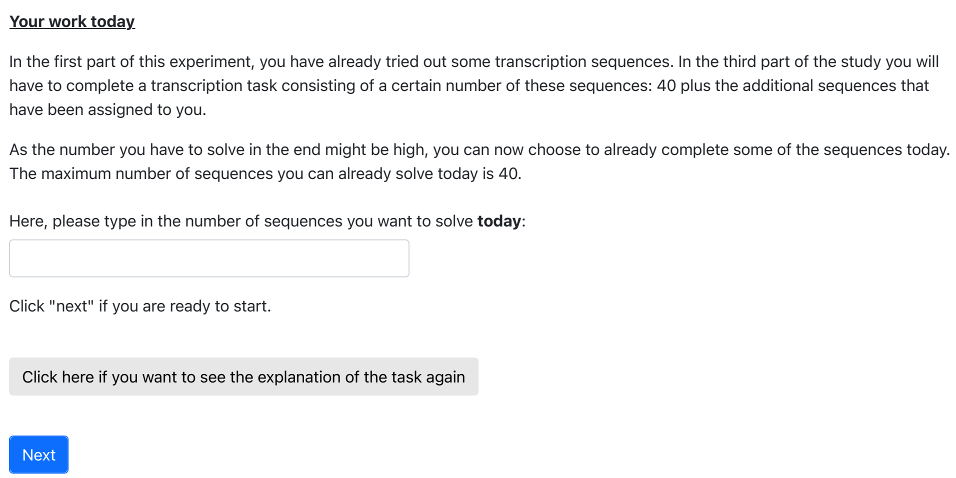

Figure A.11: Work choice

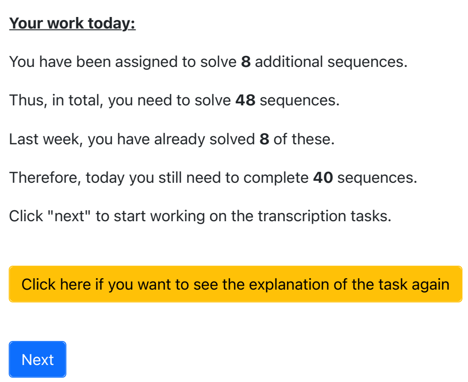

Figure A.12: Work load resolution

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 (spent 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.13 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 there is some attrition at all stages of the experiment, attrition is overall relatively low for a longitudinal experiment lasting for four weeks 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).

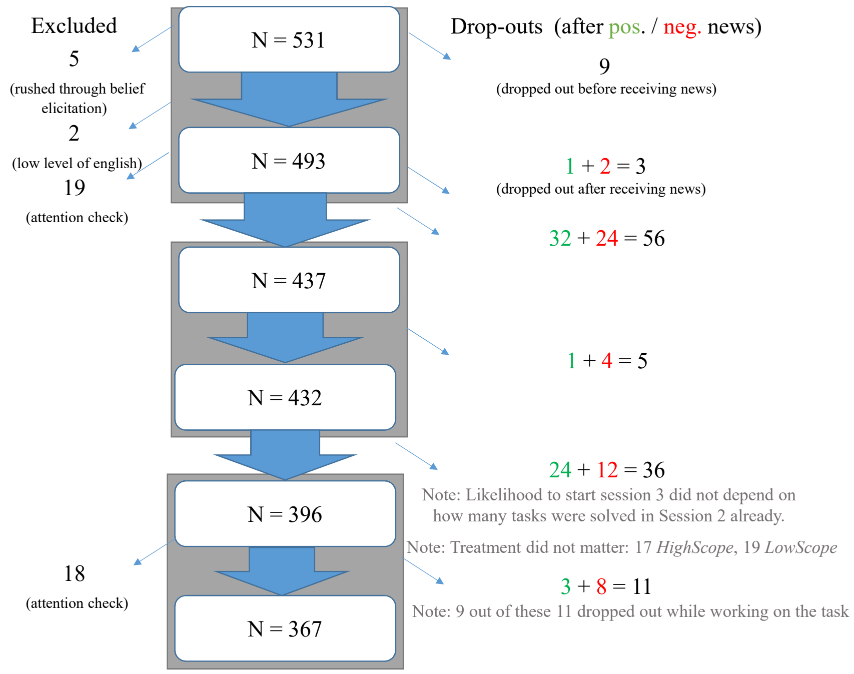

Figure A.13: Exclusion and attrition

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.