Why Do We Procrastinate? Present Bias and Optimism

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Abstract

Time inconsistency is typically modeled as resulting from present bias, a form of preference non-stationarity. In this paper, we study an alternative: excessively optimistic beliefs about future costs or demands on an individual's time. The models can be distinguished by how individuals respond to information. Experimental results refute the hypothesis that non-stationarity is the sole source of dynamic inconsistency, but they are consistent with biased beliefs about shocks. These findings offer an explanation for low takeup of commitment and suggest that personalized information can mitigate procrastination.

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1 Introduction

Procrastination is an important feature of everyday life. It is a common topic of conversation at work and at home, and economists have documented it in consequential settings including retirement saving, exercise, and education (DellaVigna, 2009). Procrastination is commonly modeled as originating from present biased discounting that favors the present at the expense of the future (Strotz, 1955, Laibson, 1997, O'Donoghue and Rabin, 1999, Barro, 1999, Ashraf et al., 2006, Heidhues and Kőszegi, 2010, Augenblick et al., 2015).[1] We study an alternative model in which dynamic inconsistency arises from excessive optimism about future demands on an individual's time. While both models predict dynamically inconsistent choices, they predict different responses to information that changes beliefs. We test these predictions experimentally and reject the hypothesis that present bias is the sole source of dynamic inconsistency. Instead, we find evidence that biased beliefs about demands on time matter. Our results suggest that the typical policy prescription—committing to decisions in advance—is incomplete and that personalized historical information is an important additional tool for people making decisions over time.

Biased beliefs about one's cost or time shocks can cause choices made ahead of time to differ from choices made in the moment. Consider an agent who does not accurately anticipate the arrival of a time-consuming task. Colloquially, we say that such an agent is optimistic about her time shocks. Once the task arrives, the agent will need to reallocate her planned time use to accommodate the unanticipated shock.[2] If the agent has systematically biased beliefs over future time shocks, then such procrastination can occur even with neoclassical discounting. We refer to this source of dynamic inconsistency as *biased beliefs about shocks*.[3]

Dynamic inconsistency can also arise from preference non-stationarity, which does not require that preferences between options remain unchanged if the timing of all options is shifted by the same amount (Koopmans, 1960, Halevy, 2015). This leads the

[1] In the quasi-hyperbolic model of Laibson (1997), the agent discounts at rate δ between future periods, but between the current period and the next period at rate $\beta\delta$ with $\beta < 1$. This heavier discounting leads to "present biased" allocative choices. Chakraborty (2021) axiomatizes a model of present bias and shows that a variety of models of time preferences satisfy the axioms.

[2] The model shares features with Kahneman and Tversky's (1982) "planning fallacy." Our theory links biased beliefs and dynamic inconsistency.

[3] In contrast to Halevy (2008) and Andreoni and Sprenger (2012), this inconsistency is a result of the decision maker having incorrect beliefs rather than a utility function that does not take the expected utility form.

agent to exhibit dynamic inconsistency because choices made far enough in advance will be governed by one set of preferences, while choices made about the immediate future will be governed by a different set. If an agent is naïve about her own non-stationarity, she believes that she will behave more consistently than she actually does, so we refer to this source of dynamic inconsistency as *naïve non-stationarity*.[4]

Because these two models lead to similar dynamically inconsistent choices, a research design seeking to distinguish naive non-stationarity from biased beliefs about shocks cannot rely solely on revealed procrastination. However, with assumptions about how subjects update their beliefs, making past behavior salient to decision makers resolves this identification problem. Specifically, we assume that when confronted with past procrastination, a decision maker with biased beliefs about shocks updates those beliefs to be more pessimistic. In the same situation, we assume that a decision maker with naive nonstationarity would become "more sophisticated," increasing the likelihood of procrastination according to the decision maker's beliefs. Thus, both models rely on agents not incorporating information about their prior choices when forming beliefs, but the two models make different predictions about how agents will respond to information that changes beliefs.

First, the two models give different predictions for how effort allocation will change in response to reminders about past dynamic inconsistency. Naïve non-stationary agents have a clear idea of the shocks that they face, but have trouble committing to time use choices. Such agents will not change effort allocations in response to information. In contrast, for agents with biased beliefs about shocks, correcting these beliefs will cause them to change their effort allocations to better conform to the true state of the world.

Second, information can cause naïve non-stationary agents to learn about their own non-stationarity. For instance, an agent might learn that her discounting is more present-biased than she previously thought. This will increase commitment demand for time-use choices made far enough in advance. If agents have biased beliefs over time shocks, however, this prediction need not hold. Information on past dynamically inconsistent decisions should help belief-based dynamically inconsistent agents bring their beliefs in line with the true state, but this does not necessarily lead them to demand costly commitment (Laibson, 2015).[5]

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[4] In the quasi-hyperbolic model, partially naïve agents have true discounting parameters β and δ but believe their present-bias parameter is $\hat{\beta}$ where $\beta < \hat{\beta} \leq 1$ (O'Donoghue and Rabin, 2001).

[5] Optimistic agents with an underlying neoclassical utility function would generally like flexibility

We tested these predictions in two experiments. Both lasted for two weeks, with the first week allowing us to measure baseline dynamically inconsistent behavior for each subject. In both weeks, subjects chose how many real-effort tasks to complete at a fixed date. In the first experiment, each task done at the beginning of the week reduced the number of tasks that needed to be completed later in the week, while in the second experiment, subjects were paid on a future date for each task completed.[6] Subjects made each choice twice: once well in advance of task completion, and once immediately before. Subjects could reveal a preference for costly commitment by choosing whether the earlier or later choice would be more likely to be implemented using a price list denominated in additional tasks.

At the beginning of week 2, before engaging in task decisions like those in week 1, treated subjects were presented with information. In the first experiment, they were presented with a reminder about their task choices in the first week.[7] In the second experiment, one set of subjects was treated with reminders about their task choices in the first week, while the other set of subjects was given information relevant to how difficult the tasks would be in week 2. Because we only treated some subjects with this information, the experiments allow us to identify the effect of changing beliefs while controlling for other determinants of procrastination and commitment demand.

Our experimental results indicate that both biased beliefs about shocks and non-stationary preferences are important determinants of time inconsistency. The strongest evidence is on biased beliefs about shocks and comes from testing the effect of treatment on task allocation. In both experiments we observed reallocation, on average, in response to each of the treatments. Among subjects who reallocated work in week 1, the treatment with information caused a practically large and statistically significant change in reallocation of tasks in week 2. This is inconsistent with preference non-stationarity and consistent with biased beliefs over cost shocks. Evidence on naive non-stationarity comes from testing the effect of treatment on commitment demand in week 2 for individuals who reallocated in week 1. In the first experiment, treatment increased week 2 commitment demand among reallocators, while in the second experiment it did not.[8]

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and making beliefs less optimistic has an ambiguous effect on commitment demand.

[6] A discussion of the motivation for the differences between the two experiments is in Section 2.3.4.

[7] In the first experiment, subjects were also given information about how well they were able to forecast their own bedtimes, a real-world procrastination behavior.

[8] Section 3.2.3 discuses potential explanations for this difference in results.

Our first experiment shares design features with Augenblick et al. (2015), which is part of an extensive literature evaluating the prevalence of dynamic inconsistency across a variety of domains. Augenblick et al. (2015) shows that in their experiment, present bias is more common in real-effort choices than in choices over time-dated money. It also demonstrates that observed present bias is correlated with the take up of commitment. In contrast, our study is focused on identifying the sources of dynamic inconsistency rather than measuring it. Because of this, our paper is more closely related to Halevy (2015), which identifies how non-stationarity and time-varying preferences contribute to time inconsistency using a classroom-based experiment with time-dated monetary rewards. We add to this literature by showing that biased beliefs contribute to time inconsistency and that targeted information can reduce that time inconsistency.

This study provides empirical evidence that a model of dynamically inconsistent behavior based purely on naive non-stationarity is incomplete. The presence of biased beliefs about shocks matters for policy aimed at dynamically inconsistent behavior. The prescription from the time inconsistency literature has primarily been to encourage commitment by sophisticated present-biased agents. Our results suggest that such tools are inappropriate for some people. If procrastination stems from overestimation of future earnings or underestimation of how difficult it will be to quit smoking, then organizations and individuals seeking to correct dynamic inconsistency should provide personalized, salient information. This hypothesis is consistent with the widespread sale of goods—like fitness trackers and planners—that help consumers reflect on execution of their own plans.[9]

In addition, our study makes two contributions to research on demand for costly commitment. First, our findings help explain the widely observed low take-up of such commitment. Subjects whose dynamic inconsistency originates solely from optimism will not demand costly commitment. Schilbach (2019) observes that in the majority of past experiments, subjects were either unwilling to pay for commitment or were willing to pay only very small amounts. Second, our experimental design makes a methodological contribution in its elicitation of commitment demand. In contrast to most previous work eliciting commitment demand, our commitment price is denominated in tasks rather than money.[10] By keeping all choices in the task domain, we

[9]Paul Krugman has made this point when reflecting on his own fitness tracker use, writing that "what fitness devices do, at least for me, is make it harder to lie to myself" (Krugman, 2015).

[10]To the best of our knowledge Toussaert (2018) is the only other experiment that elicits commit-

reduce the tendency of commitment demand to spike sharply at a zero price. We find that about one-quarter of the subjects in the lab experiment and over one-half of the subjects in the online experiment were willing to commit to their time use choices at positive task-denominated prices.

Finally, our results contribute to a growing body of research demonstrating the importance of a decision maker's beliefs for how they make choices involving time. There is evidence that decision makers are subject to the "planning fallacy" when forming beliefs about future events (Kahneman and Tversky, 1982, Roy et al., 2005). DellaVigna and Malmendier (2006) and Acland and Levy (2015) both study gym membership and attendance, showing that consumers systematically overestimate how often they will go to the gym in the future even when this choice entails monetary costs. Börsch-Supan et al. (2018) demonstrate that a much larger portion of regret about not having saved more earlier in life is explained by positive and negative financial shocks than present bias. Allcott et al. (2021) show that some payday loan borrowers exhibit evidence of both time inconsistency and overoptimism about repayment. Consistent with the common lack of commitment demand in experimental subjects, Augenblick and Rabin (2019) find that individuals' *predictions* about the choices they will make in the future suggest that they do not understand their own present bias. Furthermore, subjects who make choices for the future immediately after completing tasks volunteer for less work in the future than those asked just before completing tasks. While the authors interpret this as evidence of projection bias, it is also consistent with decision makers who are optimistic about their desire to complete future tasks but who update after getting information. Using a design similar to that of Augenblick and Rabin (2019), Fedyk (2022) confirms that individuals are not able to predict their own dynamic consistency but do predict present bias in others. Furthermore, subjects with this asymmetry tend to exhibit optimism about the future demands on their time. Our paper experimentally tests the link between time inconsistency and a planning fallacy that occurs due to biased beliefs about shocks.

The paper proceeds as follows. Section 2 describes the motivating theory, lays out testable hypotheses, and gives the experimental design. Section 3 presents empirical tests of our hypotheses. Section 4 concludes.

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ment demand with prices denominated in tasks.

2 Experimental Design

In this section, we begin by describing our conceptual framework and providing experimental hypotheses. We then describe two longitudinal experiments that we use to study the sources of procrastination.

The first experiment (henceforth the "lab experiment") was run with a student sample at a large university in the United States. The first session of the experiment, in which experimental procedures were explained, was run in person in a laboratory. All follow-up experimental responses were completed online using the survey software Qualtrics. As discussed in Section 2.3.4, some of the surprising results of the lab experiment motivated us to conduct a follow-up experiment with a larger sample, an additional treatment, and a slightly altered design.

The second experiment (henceforth the "online experiment") was conducted with an online sample using the recruitment website Prolific.[11] After volunteering for the study on Prolific, subjects completed all experimental responses on Qualtrics.

2.1 Conceptual Framework and Hypotheses

Consider a decision maker who must choose how many tasks to complete at some date t. Completion of these tasks has some benefit at a later date, with more tasks completed bringing higher benefits. However, tasks are costly in terms of time and effort, and costs may be unknown before date t. We study two choices the decision maker might make: how she chooses well in advance of the tasks needing to be completed (henceforth "committed decisions"), and immediately before they are completed ("uncommitted decisions"). This paper focuses on why these choices might differ systematically. We call any difference in the chosen number of tasks a *reallocation*. Positive values for reallocation mean the number of tasks that the decision maker chose well in advance of completion is higher than the number she chose just before completion; negative values mean the opposite.

There are many reasons why committed decisions might be different from uncommitted decisions. The economics literature has placed significant emphasis on procrastination (a positive average value of reallocation). The most common explanation for these reallocations is non-stationary preferences, the leading model of which is β-δ (Laibson, 1997). In this model, the difference between committed and

[11] Comparisons of responses by participants recruited from Prolific, other online platforms, and undergraduate subject pools show that Prolific subjects are typically more diverse and their participation is of higher quality (Eyal et al., 2021).

uncommitted decisions arises as a result of differences in the way the decision maker discounts. When the committed decision is made, both costs and benefits are in the future. When the uncommitted decision is made, costs are in the present and benefits are in the future, leading to benefits being systematically down-weighted and fewer tasks being completed. Thus, choices exhibit present-bias. Furthermore, research has shown that to explain behavior, decision makers must be at least partially naive about their present bias: they act as if they believe they will behave in a less present-biased way than they do (Acland and Levy, 2015, Augenblick et al., 2015, Augenblick and Rabin, 2019, Le Yaouanq and Schwardmann, 2022).

In this paper, we propose an alternative explanation for systematic task reallocations: biased beliefs about costs.[12] When the decision maker makes the committed choice, she might not know exactly the costs that she will face when she completes the tasks. When she makes the uncommitted choice, she would likely be aware of any time shocks that are relevant to her costs. Biased beliefs about these shocks could then lead to systematically reallocating tasks.[13] For instance, if the decision maker is optimistic about the costs she will face, then she would tend to be surprised by high realizations of costs, leading to systematic task reallocation.

Because both naive non-stationarity and biased beliefs about shocks lead to systematic task reallocation, observing these task reallocations cannot differentiate the models. But because both models rely on incorrect beliefs (either over the decision maker's own preferences or the shocks that they will face) to generate patterns seen in the data, one way to differentiate between the two models is to observe the effects of provision of information about the distribution of costs on subsequent allocative and commitment behavior.[14]

To illustrate the effect of information within the context of the two models, we

[12]We do not model the source of incorrect beliefs, instead taking them as given and studying their implications. However, a number of existing models could lead to these optimistic beliefs. Kahneman and Tversky (1982) coined the term "planning fallacy" and provided an intuitive model in which decision makers neglect distributional information, leading to optimistic beliefs about outcomes like task duration. Beliefs and updating rules have also been modeled as a choice variable from the point of view of the decision maker (Bénabou and Tirole, 2002, Brunnermeier and Parker, 2005, Brunnermeier et al., 2016). Agents in these models trade off between the distortions caused by incorrect beliefs and their benefits, such as improved self-esteem or higher motivation.

[13]Despite previous work using task reallocations as evidence of bias, the expected value of reallocations can be nonzero even for a decision maker with standard preferences (Strack and Taubinsky, 2021). In general, the expected value depends on the decision maker's cost function.

[14]A formal model consistent with this framework is available upon request and was included in earlier versions of this paper.

consider a decision maker who consistently exhibits positive task reallocations and analyze the effect of treating this subject with information (such as the information on past dynamically inconsistent behavior that we treated some subjects with in our experiment) that affects her beliefs.

For a naive non-stationary decision maker, we assume that this information causes the decision maker with positive task reallocations to believe they are more present biased. In the notation of O'Donoghue and Rabin (2001), this would imply that $\hat{\beta}$ falls. Within the context of the two-period decision problem described above, this change in beliefs only affects the decision maker's willingness to commit—not the allocative choices that she makes when either committed or uncommitted. The effect on commitment demand is straightforward: the decision maker expects that (from the perspective of their current self) her future self will make worse decisions, thus increasing the value of the present self's choices being implemented. The allocations chosen by the decision maker are not expected to change because the treatment only affects the decision maker's beliefs, not her preferences: the present self prefers a particular allocation, and changing beliefs about what the future self will do does not change that preference.

Hypothesis NNS. For naive non-stationary agents, information provision that increases the perceived level of present bias will increase commitment demand but will have no effect on work allocations.

For the decision maker with biased beliefs about shocks, we assume that information provision makes them more pessimistic about their cost shocks.[15] Such a decision maker modifies her choices so that her earlier decisions are more consistent with the decisions she makes later. Updating beliefs in this way has an ambiguous effect on commitment demand.

Hypothesis BBS. For agents with biased beliefs about shocks, information provision that makes beliefs less optimistic will decrease procrastination.

We emphasize that while much of the above discussion is framed in terms of treating a decision maker with systematically positive reallocations (i.e. one who procrastinates), our empirical identification and tests do not rely on the sign of the subject's

[15]More formally, we assume the updated belief distribution about cost shocks first-order stochastically dominates the prior.

reallocation. Subjects who reallocation positively or negatively in this framework are dynamically inconsistent. Informing naïvely nonstationary subjects of either positive or negative reallocation plausibly makes them more likely to believe their preferences are non-stationary. Similarly, for agents with biased beliefs about shocks, information that makes beliefs more optimistic will increase procrastination, while information that makes beliefs more pessimistic will decrease it. Changes in both directions are useful for identifying the source of behavior, and the empirical approach described in Section 3 reflects this.

In what follows, we describe two experiments designed to test these hypotheses. Information in these experiments takes one of two forms. In both experiments, some subjects are provided with reminders about their own past allocative choices. We expect that for naive non-stationary decision makers, reminders about positive (negative) reallocations cause the decision maker to believe that they are more (less) present-biased, increasing (decreasing) commitment demand but leaving allocations unchanged. For decision makers with biased beliefs about shocks, we expect that reminders about positive (negative) reallocations cause the decision maker to be less (more) optimistic about time shocks, decreasing (increasing) reallocations.

In the second experiment, we introduce an experimentally administered cost shock, about which we expect subjects to be optimistic. Subjects compete in a contest, and winning reduces the difficulty of the tasks that they have to complete. Overconfidence in the contest acts similarly to optimism about cost shocks. We provide some subjects with experimentally administered signals relevant to the likelihood that they will win the contest. We hypothesize that negative (positive) signals make decision makers less (more) confident about their likelihood of winning the contest, decreasing (increasing) reallocations.

Because of differences in design between our two experiments, we defer precise discussion of our identification arguments to Sections 3.1.1 and 3.2.1.

2.2 Lab-Based Experiment

The hypotheses laid out above were tested using two experiments. The first, lab-based experiment is described here. The experimental instructions and surveys for the lab experiment appear in Appendix I.

To begin, subjects completed an introductory session in the lab.[16] The remainder

[16]Subjects were given an overview of the timeline and requirements of the study, completed a survey of basic demographic information as well as a present bias elicitation, did five sample tasks,

of the experiment took place during twelve sessions split over two weeks. Table 1 shows a timeline of these sessions. The two weeks of the experiment were identical except for a randomly assigned treatment given during week 2.

The core of the experiment involved subjects making plans for work (real effort tasks) to be carried out Monday and Wednesday of each week, choosing whether to commit to those plans (at a cost in terms of extra tasks), then choosing whether to alter their plans when it came time to actually begin the work on Monday evening. Procrastination was measured by the amount of work that subjects reallocated from Monday to Wednesday relative to their plans.[17] The first week of the experiment was used to gather information on baseline dynamic inconsistency for all subjects. During week 2, a randomly chosen half of the subjects were treated with information on their behavior during week 1. The treatment occurred prior to the subjects making their week 2 work plans. This allows us to study the effect of such information on choices made by the treatment group, testing the hypotheses laid out in Section 2.1, while using the control group to account for anything else that might have changed between weeks 1 and 2.

Table 1: Timeline for Lab Experiment

Monday	Tuesday	Wednesday	Thursday
Morning (6 a.m. – 2 p.m.)			
• Bedtime elicitation • **(Week 2 only)** Randomized treatment • Commitment demand: choose mandatory tasks Mon&Wed • Committed choice: allocate 10 additional tasks across Mon/Wed	• Bedtime elicitation	• Bedtime elicitation	• Bedtime elicitation
Evening (9 p.m. – 4 a.m.)			
• Bedtime plan • Randomized commitment probability & mandatory tasks revealed • *Un*committed choice: allocate 10 additional tasks across Mon/Wed • Complete Mon tasks		• Bedtime plan • Complete Wed tasks	

learned about how the allocation and commitment decisions would be made, and were required to complete a comprehension quiz before advancing.

[17] We also measured dynamic inconsistency around choices of when to go to bed (see Section 2.2.2) in order to test effects on a consequential real-world behavior (Gibson and Shrader, 2018).

Given that the experiment involved dynamic choices, subjects were required to complete surveys and tasks at particular times. A link to each morning survey was sent out at 6 a.m., and subjects were instructed to complete the survey before noon that day. At noon, subjects who had not completed the task were sent a reminder and had two hours to complete the survey. If they did not complete the survey by 2 p.m., they were dropped from the study.[18] A link to the evening surveys was sent out at 9 p.m. and the tasks that were part of those surveys had to be completed before 4 a.m. the next morning.

Subjects received $40 total for completing the full study. An initial payment of $10 was made to all subjects on Thursday or Friday of the first week. The second payment of $30 was made to the subjects on Thursday or Friday of the second week, conditional on all portions of the experiment being completed on time.

2.2.1 Allocations, Tasks, and Commitment

Subjects made two allocation decisions each week. Each allocation decision consisted of dividing 10 tasks between Monday and Wednesday evenings. On each of these evenings, subjects had to complete the tasks allocated to that evening in addition to a number of mandatory tasks, which are described below. The first allocation was made when completing a survey on Monday morning, imposing at least a seven-hour delay between when the allocative decision was made and when the tasks were actually carried out. The second allocation was made immediately before completing the tasks on Monday evening.

In addition to allocating tasks across evenings, subjects were also offered the chance to commit to their Monday morning choice, increasing the probability that the morning allocation would be the one implemented. If the subjects did not commit they had a one-in-five chance of the morning allocation being implemented. If the subjects did commit this probability rose to four out of five. The commitment was probabilistic rather than deterministic to preserve the incentive compatibility of the evening choices.

To elicit subjects' demand for commitment, they were given the choice of whether or not to commit at a variety of prices, both positive and negative. Due to previous work, including Augenblick et al. (2015), suggesting that many subjects' money-denominated willingness to pay for commitment is near 0, the prices were denomi-

[18] We analyze attrition in Table A1 and find no evidence of selection on observables.

nated in terms of mandatory tasks that would have to be done each night in addition to the tasks that were allocated to that night. Mandatory tasks could potentially vary between 4 and 16, depending on a subject's choices and which choice was implemented.

The tasks that subjects were required to complete consisted of moving sliders to match particular, predetermined levels. Slider tasks have proved useful in experimental settings as tasks that require real effort and focus from subjects (Gill and Prowse, 2012). [19] A single task consisted of moving nineteen sliders. Each page included no more than 10 tasks. Because each task would fill a computer screen, subjects needed to scroll downward to complete additional tasks. Subjects were unable to proceed to the next page if the current page was incomplete or if there were any errors. If subjects tried to proceed in these cases, they were informed that the task had a problem but were not told which slider was incorrect. The tasks were designed so that each would take about one minute to complete.

2.2.2 Bedtime Plans and Actions

Both planned and actual bedtimes were elicited from subjects. In each morning survey, subjects were asked when they went to sleep the night before. Additionally, in both the morning and evening surveys subjects were asked at what time they expected to go to sleep that night. These predictions were deliberately not incentivized because an incentivized prediction could have functioned as a commitment device.[20] The bedtime information allows for tests of changes in real-world time allocation behavior, reported in previous versions of this paper.

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[19] The required level of each slider was varied to increase difficulty. Each slider was initialized at the number one but had to be clicked before it became active. To avoid subjects becoming confused by their tasks not being accepted due to an inactive slider, the number one was omitted from the potential target levels.

[20] In addition to these self-reported measures, in the first week subjects wore Fitbit wristbands to independently measure their sleep. To obtain the information from the Fitbits, they had to be returned to the experimenter and synced. Due to the time required to sync and recharge each Fitbit, it was infeasible to then immediately return them to the subjects, so the subjects did not have them in week 2 and the experimental treatment focused on survey measures of actual and planned bedtime.

2.2.3 Treatment

Within each study wave, randomization was uniform at the subject level.[21] In the second week of the study, treated subjects were given information about their own past choices. The treatment—a real example of which can be seen in Figure 1—consisted of three main parts. The first described the allocation choices that the individual made the week before. Subjects were told whether or not any tasks were reallocated on Monday evening. The second part reported the subject's average actual and predicted bedtimes and gave the difference between them in minutes. Finally, treated subjects were asked why someone's choices and predictions might change throughout the day. Subjects were given a blank space in which they had to type something to proceed.

The treatment information was presented neutrally to avoid experimenter demand effects. The message was presented within the survey without an experimenter present, ruling out any physical or vocal suggestions (de Quidt et al., 2019). We provided subjects with information that they could have recorded for themselves had they chosen to do so. Finally, we did not mention commitment.

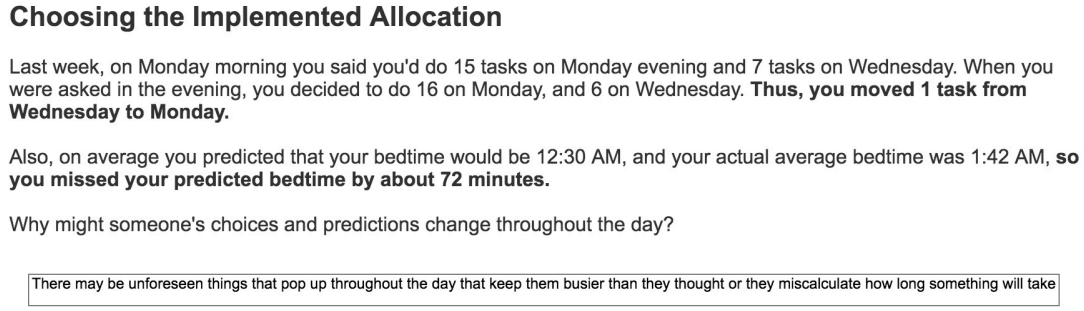
This information was given to treated subjects (and only treated subjects) on Monday morning of the second week. They were shown the information after they reported their bedtime for the previous night and made a prediction for Monday night but before they made the commitment and allocation decisions.

2.2.4 Sample and Summary Statistics

Undergraduate subjects were recruited to four different sessions of the lab experiment across the second semester of the 2016-2017 academic year. A total of 274 subjects completed the introductory session. Twenty-six of these subjects did not complete some surveys and left the experiment having received only the initial payment of $10. The vast majority of those who dropped out of the experiment did so in the first week of their participation. Another 39 subjects missed the completion deadlines for at least one survey, though they eventually did answer all surveys. These subjects are excluded from the primary sample, leaving a final baseline sample of 209 subjects in the lab experiment. Table A1 shows that observable baseline characteristics do not predict attrition. Summary statistics for the final estimation sample are shown in Table A2, and distributions of week 1 and week 2 task reallocation and commitment

[21] Treatment-control balance is assessed in Table A2. The largest standardized difference between the two groups was that more female subjects were in the treatment group. Results are robust to including or excluding controls for this and other demographic variables.

Figure 1: Treatment



Notes: An example of an actual message that one of the treated subjects received at the beginning of week 2 of the lab experiment, along with the response they entered. The information was provided to subjects just before they made commitment and allocation decisions. The text given in the box is an example of a response that a subject gave to the open-ended question about why someone's choices and predictions might change. The box was empty when subjects were presented with the message.

demand are shown in Figures A1 and A2. Of particular relevance for the estimation results below, the summary statistics show that 35 out of 100 control group subjects and 28 out of 109 treatment group subjects were dynamically inconsistent in week 1 (equally split between those who reallocated positively and negatively). [22]

2.3 Online Experiment

The online experiment was pre-registered (AEARCTR-0011140) and tested hypotheses similar to those in the lab experiment. It contained one treatment arm that used over-confidence to study biased beliefs about shocks and another that replicated the lab experiment. Full surveys appear in Online Appendix II.

Subjects had to complete five surveys in total. The first survey introduced the experiment.[23] Table 2 provides the timeline for the other surveys, which occurred over two weeks. Like the lab experiment, the online experiment first gathers information on subjects' behavior in week 1 then uses randomized treatments with information

[22] While most (but not all) papers find that evidence of present bias on average, there is often substantial heterogeneity between subjects (Imai et al., 2021). Furthermore, the presence of present bias may depend on how allocations are made: Freeman and Laughren (2024) finds that subjects appear *future*-biased when required to make complete tasks once-and-for-all. One helpful feature of our identification strategy is that it relies on dynamic inconsistency but does not require that subjects are present-biased on average.

[23] In this survey, which always occurred on a Thursday, subjects were informed of the full experimental schedule, completed four example tasks (two easy and two hard, described below), and completed a comprehension quiz about the experiment.

in week 2 to study effects on reallocation and commitment demand.

Table 2: Timeline for Online Experiment

Monday/Tuesday	Thursday/Friday
• Contest • **(Week 2 only)** Randomized contest information treatment • Contest belief elicitation • **(Week 2 only)** Randomized task information treatment • Choose committed allocations • Complete tasks to pay for commitment	• Choose *un*committed allocation *un*conditional on contest • Learn contest result • Choose *un*committed conditional allocation (easy or hard tasks) • Complete tasks

The most important difference between the lab and online experiments is a new treatment based on provision of information about the distribution of costs. In week 1, subjects made allocation decisions (described further below) both over time and over tasks with different levels of difficulty (more or fewer sliders). Subjects were able to do the easy tasks if they won a contest–which consisted of IQ test questions–during the first session of the week. Overconfident subjects would have believed they would be more likely to face the easy tasks, and so would have made more optimistic work plans. During session two of week 1, subjects learned whether they won the contest from the first session. Overconfident subjects would have been more likely than they expected to learn they were facing hard tasks, leading to higher perceived costs and thus reallocation. In week 2, a randomized treatment group was provided with additional information about their contest performance in week 1 prior to making any allocation choices. Specifically, this treated group is told that we matched their performance with two additional randomly drawn contestants, and we tell them the number of times (out of two) that they would have won these matches. We expect this additional information would have led to more accurate beliefs about task difficulty (costs) and less reallocation.[24]

Timing of the surveys was again important to capture dynamic behavior. The second survey was completed on Monday or Tuesday during each study wave. The third survey was completed on Thursday or Friday. Surveys four and five were completed

[24]Another treatment group received the same type of reallocation behavior information as the treatment group in the lab experiment, allowing for a replication.

on the following Monday-Tuesday and Thursday-Friday, respectively.[25] Subjects that did not complete a survey by the required time were dropped from the study.[26]

All payments for the online experiment were made the day after the fifth survey of the relevant experiment (on a Saturday). Subjects received a baseline payment of \$9 for completing all five surveys.[27] Given their implemented allocation and piece rate (both described below) they received their payments for completing tasks as a "bonus" on Prolific.

2.3.1 Contests

The contest in the second and fourth surveys was between pairs of participants and involved completing ten questions drawn from the matrix reasoning item bank (MaRs-IB, Chierchia et al. (2019)). Each question contained an incomplete matrix of abstract shapes, with four potential options to complete the matrix. These questions appear in Online Appendix II. Subjects were told that they would complete an "IQ quiz" and that the winner of the contest would receive easier tasks in a future session while the loser would receive more difficult tasks. Subjects were told that in the event of a tie, the winner would be determined randomly.[28] After the contest, we elicited each subject's beliefs about the likelihood that she had won the contest. These beliefs were elicited without incentivization (Armantier and Treich, 2013).

2.3.2 Allocations, Tasks, and Commitment

The allocative decisions that subjects made differed from those in the lab experiment. Rather than splitting a fixed number of tasks between two dates, in surveys two and four subjects were asked to choose how many tasks they would complete in surveys three and five for various piece rates and task difficulty levels. The number of tasks that participants could choose had to be between 0 and 19.[29] The piece rates that

[25] The timing restrictions were based on Eastern Standard Time, so subjects on the west coast had to complete surveys 2 and 4 between 9 p.m. on Sunday and 9 p.m. on Tuesday.

[26] We analyze attrition in Table A4.

[27] This amount was calibrated based on pilot data to attain a median hourly wage of \$12/hr for the experiment *excluding* time spent completing the tasks in surveys three and five (which were incentivized through a separate, piece-rate payment).

[28] In practice, to determine whether a subject won, each subject's score was recorded and compared against the full distribution of scores of those who completed the quiz in that subject's wave.

[29] We set the maximum number of tasks to 19 with the hope of avoiding subjects choosing focal round numbers for their allocation.

subjects faced were \$0.06, \$0.12, and \$0.18 per task.[30] In surveys two and four, subjects made choices conditional on the tasks being easy (winning the contest), being hard (losing the contest), and without knowing the difficulty level (unconditional on contest outcome). In surveys three and five, subjects made choices without knowing the difficulty level (unconditional on contest outcome) and conditional on the *realized* contest outcome (either easy or hard).[31] Thus, subjects made nine allocative choices in surveys two and four and six allocative choices in surveys three and five.

Subjects also made commitment choices in surveys two and four. They chose from a price list that offered an 80% chance of the survey two and four choices being implemented (at the cost of doing one to five easy slider tasks) or an 80% chance of the survey three and five choices being implemented (at the cost of doing three easy slider tasks). Unlike in the lab experiment, the sliders associated with the commitment choice had to be completed at the time commitment was chosen rather than at the same time as those paid by piece rate. This reduced the chances that the randomly drawn commitment price could directly affect the subject's allocation.

The tasks subjects completed in the online experiment were similar to those from the lab experiment except for the following differences. First, there were two types of tasks: *easy* and *hard*. Each easy task consisted of 20 sliders that needed to be matched to a number between 1 and 20, while each hard task consisted of 30 sliders that needed to be matched to a number between 1 and 30. Second, sliders were initialized at the number zero rather than one. Third, each task needed to be completed on its own page (rather than being grouped into a maximum of 10 tasks per page). Thus, for each task, subjects needed to match all sliders to the relevant number before being able to move on to the next task. Easy tasks were designed so that each would take about one minute to complete, while hard tasks were designed to take between 90 and 120 seconds.

We informed subjects which piece rate they were randomized into immediately after making the allocative choices in surveys three and five. Subjects were then informed whether their committed or uncommitted allocations would be implemented as well as whether their choices that were conditional on contest outcome or unconditional on contest outcome would be implemented. The probability that committed

[30] These piece rates were calibrated based on pilot data to increase the proportion of allocations that were interior.

[31] We expected that making the choice after the contest outcome had been realized would make the realized difficulty level more salient.

decisions were implemented was either 20% or 80%, and was based on the choice made in the randomly selected row in the commitment price list. The probability of implementing an allocation unconditional on the contest outcome was 50%.

2.3.3 Treatments

In the online experiment, treatment occurred during the fourth survey. Subjects were randomized into one of three equal-size treatment groups after entering the survey. The information treatments only modified Survey 4. In the *no information* (control) condition, subjects completed the contest and then proceeded to the belief elicitation and the commitment decision.

In the *contest information* treatment (Figure 2), subjects completed the contest, then received information about their performance in the previous contest. We first reminded the subject of whether they won or lost the first contest and required them to confirm that they understood this by selecting the appropriate option from "I won the contest in the previous study" and "I did not win the contest in the previous study." The subject then received information that took the form "We also matched you with two other randomly drawn participants from the previous study, and you (lost against both/won against one/won against both) of them."[32] They confirmed that they understood the message and then completed the belief elicitation and the commitment decision.

In the *task information* treatment (Figure 3), subjects completed the contest and the belief elicitation, then received information of the form "In Session 2, for a payment rate of ($0.06/$0.12/$0.18) per set and not knowing whether the sets would be easy or hard, you agreed to complete (number chosen) sets. In Session 3, in the same setting, you agreed to complete (number chosen) sets." The payment rate that was used for each subject was chosen randomly from the three options with equal probability.[33] Subjects in the task information treatment were then asked, "Why might someone's choices change over time?" They had to type something into a text box as a response to this question, but there were no requirements about what to type other than the box not being empty.

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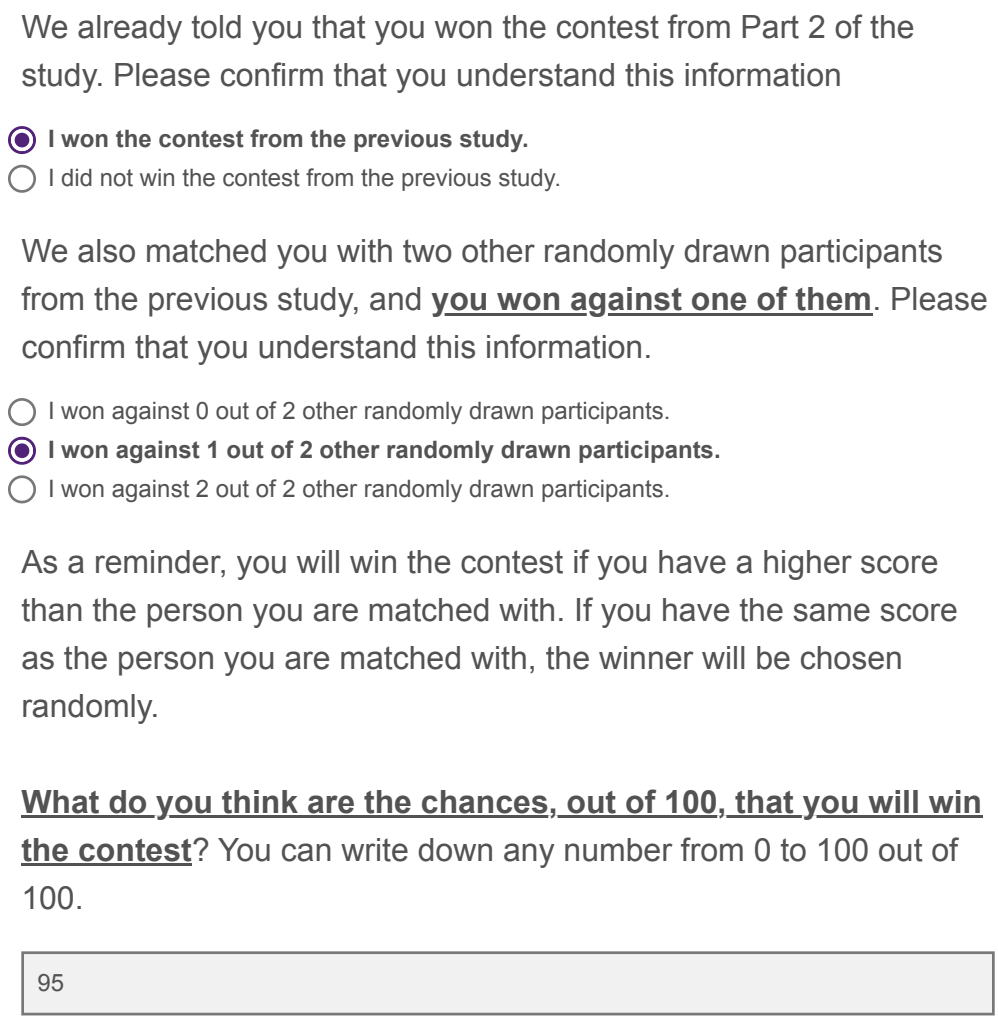
[32]These messages were generated randomly conditional on the subject's score and the full distribution of scores in the same way as the contest outcome.

[33]We chose to report the *unconditional* allocations because the conditional allocations that subjects made in survey 3 were only for the *realized* contest outcome. Reporting only the unconditional allocations allowed us to randomize which pair of allocations was reported in a way that did not depend on the subject's performance in the contest.

Figure 2: Contest Information Treatment from the Online Experiment



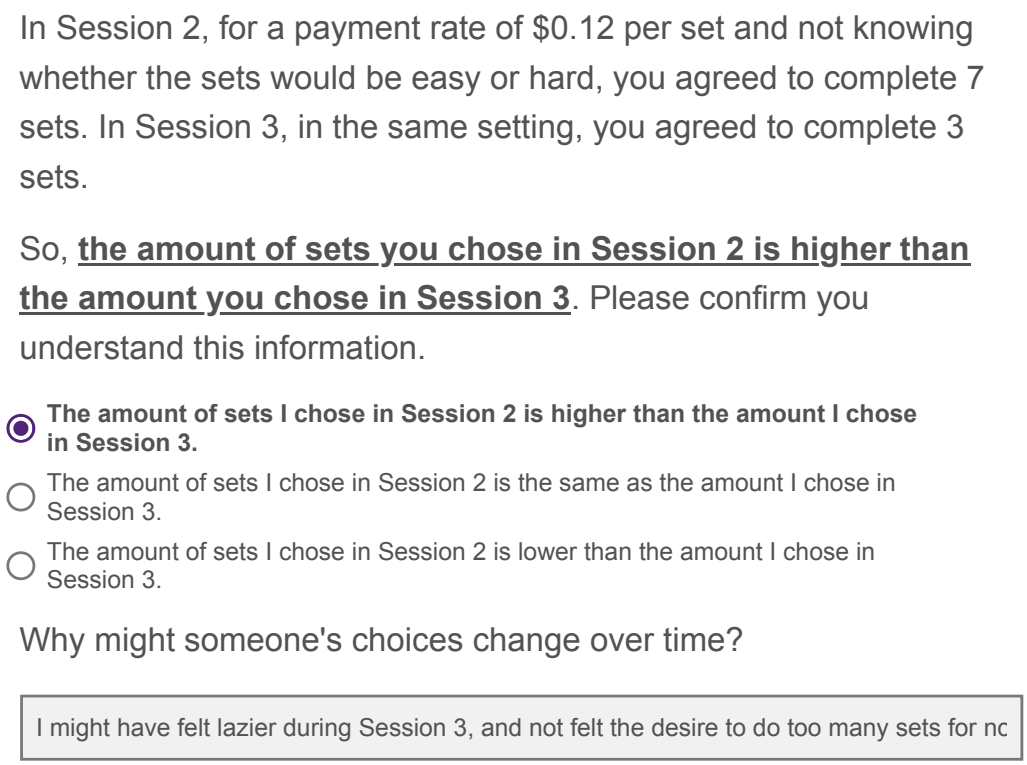
Notes: An example of an actual message that one of the subjects in the contest information treatment received in Survey 4 of the online experiment, along with the response they entered. The information was provided to subjects just before they made commitment and allocation decisions. The multiple-choice questions were unselected and the beliefs elicitation was empty when subjects were presented with the message.

2.3.4 Reasoning behind changes between experiments

In this section, we discuss the changes made from the lab experiment to the subsequent online experiment.

First, as discussed in Section 2.2.4, the level of procrastination observed in our

Figure 3: Task Information Treatment from the Online Experiment



Notes: An example of an actual message that one of the subjects in the task information treatment received in Survey 4 of the online experiment, along with the response they entered. The information was provided to subjects just before they made commitment and allocation decisions. The multiple-choice question was unselected and the response box was empty when subjects were presented with the message.

student sample was lower than expected: 63 of 209 subjects changed their allocation in the first week, 28 in the treatment group and 35 in the control. Our main object of interest is the *heterogeneous* effect of treatment on subjects based on their week-1 task reallocations. So while the lab experiment had more than 100 subjects in both the treatment and control groups, our main effect is identified by subsamples of sizes 28 and 35. The results that we discuss in Section 3.2.2 show statistical significance using standard inferential procedures, but we conducted a follow-up experiment with a larger sample and altered design to eliminate sample-size concerns.

A second worry is the causal interpretation of the estimated treatment effect in the lab experiment. While the treatment itself is randomized, our main focus is the *interaction* of the treatment with the level of task reallocation in week 1. Based

on our conceptual framework, we expect that this effect arises from the treatment altering the subjects' beliefs about either their naïve non-stationarity or the distribution of time shocks that they face. However, we cannot rule other treatment effects beyond the scope of our model. This concern is addressed with the addition of the contest-information treatment. Because the number of wins of two that is reported to the subject is random *conditional on their score*, the coefficient cannot reflect heterogeneous treatment effects of receiving a message but can only capture the effect of the content of the message. Put differently, in the lab experiment we are interested in understanding the differential effect of receiving the messages "you did more tasks than you originally agreed to" vs. "you did fewer tasks than you originally agreed to." But in the design of the lab experiment, the populations of subjects that we can send these two messages to are different. In the online experiment, we send subjects with the same contest score (and any other latent characteristics) messages that make them more or less optimistic.

The information treatments in the online experiment did not involve any information about the subject's bedtime or bedtime predictions. This allowed for the effects of the "task information" treatment to be measured separately. Because bedtime would no longer be used, the surveys did not include questions about time use or bedtimes, and subjects did not complete more than one survey in a day.[34] The reduction in the number of surveys also allowed for a longer lag between committed allocation decisions and task completion, and the tasks required as payment for the commitment demand decisions could be completed without delay.

The form of the task allocation changed between the lab and online experiments. In the lab experiment, an allocation involved splitting 10 tasks between two days, similar to the design used by Augenblick et al. (2015). In the online experiment, an allocation was a number of tasks that could be completed at various piece rates, with payment occurring at a fixed date after the experiment concluded, similar to the design used in Augenblick and Rabin (2019). We suspect that splitting tasks equally between the two dates was a focal option for subjects in the lab experiment. However, this equal split would also be chosen by time-consistent subjects that did not face time shocks. We believed that paying a piece rate would reduce the chances that subjects appeared time consistent simply because they were choosing focal options.

Finally, the task information treatment in the online experiment should only affect

[34]Furthermore, subjects did not receive or use a sleep tracker in the first week.

subjects' beliefs about the costs of completing the tasks rather than the potential benefits in the future. In the lab experiment, one could argue that if the treatment affected subjects' beliefs about the likelihood of costly time shocks on a Monday night, that it should also affect their beliefs about time shocks on Wednesday night. In the online experiment, the benefit of agreeing to do more tasks is not related to expected business at any future data, so the treatment should only affect beliefs about the distribution of likely costs.

2.3.5 Sample and Summary Statistics

The experiment was completed fully online in two waves during April and May of 2023. Subjects were recruited through Prolific, and all subjects were required to be nationals of the United States. A total of 1,479 subjects completed the introductory session. 1,178 subjects completed all sessions. Following our preanalysis plan, observations for piece rates under which the subject's corresponding week-one choice was censored (0 easy tasks or 19 hard tasks) were excluded from the sample to increase statistical power, leaving a final estimation sample of 888 subjects (283 control, 306 treated with task information, 299 treated with contest information).[35] Attrition from the start of the study and after treatment is assessed in Table A4. Observable baseline characteristics do not predict post-treatment attrition. Working as a manager is the variable most strongly associated with overall attrition. Summary statistics for the final estimation sample are shown in Tables A5 and A6, and distributions of week 1 and week 2 task reallocation and commitment demand are shown in Figures A3 and A4. The summary statistics show substantial dynamic inconsistency, with approximately 90% of subjects exhibiting non-zero reallocation for at least one week-1 task choice (committed versus uncommitted).

3 Results

3.1 Contest Information Effect on Cost-Based Reallocations

3.1.1 Estimating Equation and Identification

The estimating equation for task reallocation in response to contest information is

$$R_{CD,ir} = \alpha_0 N_{0,i} + \alpha_1 N_{1,i} + \alpha_2 N_{2,i} + \boldsymbol{Z}_{\boldsymbol{ir}}^{'}\boldsymbol{\theta_1} + \boldsymbol{X}_{\boldsymbol{ir}}^{'}\boldsymbol{\theta_2} + \varepsilon_{ir} \tag{1}$$

[35] Empirical results without this sample restriction appear in Tables A7 and A10.

where the outcome variable ($R_{CD,ir}$) is the week-2 difference between committed unconditional tasks (difficulty unknown) and uncommitted easy/hard tasks by subject i for piece rate r.[36] Whether the uncommitted tasks are easy or hard depends on the outcome of the contest, so the contest provides an observable cost shock. Reallocation R_{CD} depends in part on this shock, so we call it contest-dependent (CD) reallocation. The primary right-hand-side variables are indicators for whether subjects in the contest information treatment group were told they won zero, one, or two out of two contests against randomly selected competitors ($N_{0,i}$, $N_{1,i}$, and $N_{2,i}$ respectively).

The other variables in the equation are controls and the remaining stochastic error term, ε_{ir}.[37] The controls fall into two sets. First are the experimental design controls $\boldsymbol{Z_{ir}}$, which include variables required for conditional exogeneity of contest information and the other, orthogonal treatment that was part of the online experiment (see Section 2.3). The design controls are piece rate fixed effects, indicators for week-one and week-two contest score, indicators for whether the subject won the week-one and week-two contests against her randomly drawn opponent, reported reallocation of tasks in week 1, an indicator for being in the task information treatment group, and the interaction of reported reallocation and the task information treatment group indicator. The second set of controls $\boldsymbol{X_{ir}}$ are baseline (week 1) subject characteristics, selected to maximize precision using double machine learning with LASSO (Belloni et al., 2013, Chernozhukov et al., 2018).[38] Equation (1) differs from the pre-analysis plan in minor respects. For this and all other estimating equations, such differences are discussed in Appendix B and evaluated empirically in several appendix tables.

We use Equation (1) to test Hypothesis BBS. As pre-specified, we do so by testing the difference between the estimates $\hat{\alpha}_2$ and $\hat{\alpha}_0$, which measures the difference in week 2 reallocation behavior between a treated subject who was given a signal that she was a relatively strong competitor (two out of two wins) versus a signal that she was

[36] Intuitively, this dependent variable is meant to capture the type of inconsistency observed in real life. Early (committed) choices are made without knowledge of what the final state will be (so they are *unconditional*). Later (uncommitted) choices are made *conditional* on the state, which is observed immediately before taking action.

[37] In all regressions we employ standard errors clustered at the subject level.

[38] Results without these controls are presented in Table A8 and show similar point estimates. The set of possible variables for the LASSO procedure are the difference between committed unconditional tasks and uncommitted easy/hard tasks in week 1 as well as indicators for experiment wave, education level, employment status, elicited risk tolerance, elicited patience, age, gender, ethnicity, country of birth, languages spoken, student status, and working as a manager.

a relatively weak competitor (zero out of two wins).[39] A negative signal (zero wins out of two) should make a subject less confident about the likelihood of winning the contest in week 2, leading the subject to believe that she has a higher likelihood of performing hard tasks. The reverse is true for a positive signal (two wins out of two). A positive difference in coefficients provides evidence consistent with BBS, because it implies that subjects who become less optimistic (overconfident) decrease their level of procrastination.

The effects of contest information from Equation (1) are not heterogeneous treatment effects.[40] Because indicators for contest score are included in $\boldsymbol{Z_{ir}}$, the estimated treatment coefficients capture the effect of receiving either positive or negative information, conditional on the underlying probability of winning the contest. Because the information was randomized within groups with the same score, these groups were balanced in expectation on both unobservable and observable characteristics.

3.1.2 Overconfidence Online

Before examining contest-dependent task reallocation, we first present evidence on the mechanisms discussed in Section 2.1. Hypothesis BBS asserts that contest information affects beliefs, making earlier decisions more consistent with later decisions. That is, beliefs change, so plans change, and the end result is a change in reallocation. To assess empirically the first two links in this hypothesized causal chain, we estimate contest-information treatment effects on beliefs about winning the contest and earlier decisions (committed allocations).

Effects on beliefs about winning the contest appear in Table 3 Column 1. The results are based on estimating a subject-level version of equation (1) where the dependent variable is the probability that the subjects reported for whether they would win the contest in week 2.[41] Because priors are necessarily on the $[0, 100]$ interval, a zero-win message should weakly decrease win belief and a two-win message should weakly increase it.[42] Estimates are consistent with these predictions. On average, subjects who received a zero-win message decreased their win beliefs by approximately 11 percentage points, while subjects who received a two-win message

[39] The effect of the one-win message (α_1) is ex ante ambiguous and we do not interpret it.

[40] That is, randomized treatment variables do not interact with endogenous subject characteristics.

[41] The regression omits controls for week 2 contest score or the week 2 win indicator because those were not known by the subjects at the time they reported beliefs.

[42] The effect of a one-win message is theoretically ambiguous and our pre-specified test does not involve this coefficient.

Table 3: Effects of contest information on beliefs, weight on easy allocation

	(1) Win belief	(2) w_e
0-win message ($\hat{\alpha}_0$)	-11.3	-0.19
	(2.93)	(0.090)
1-win message ($\hat{\alpha}_1$)	-1.86	-0.032
	(2.58)	(0.072)
2-win message ($\hat{\alpha}_2$)	4.80	0.052
	(2.34)	(0.068)
$\hat{\alpha}_2 - \hat{\alpha}_0$	16.1	0.24
Right-tailed p value	8.0e-07	.012
Subjects	888	565
Observations	888	1301

Notes: Both columns shows results from estimating versions of equation (1) on the online experiment sample. The dependent variable in Column (1) is the elicited probability that a subject would win the contest in week 2. In Column (2) it is the weight w_e on the committed easy allocation. The sample size is reduced, as w_e is undefined for subjects whose committed easy and committed hard allocations are identical. In parentheses are standard errors clustered at the subject level.

increased win beliefs by 4.8 percentage points. In relative terms, the two-win message led to a 16 percentage point increase in reported probability of winning compared to the zero-win message (proportionally, a 30% increase on over average week 1 beliefs).

Table 3 Column 2 evaluates the second link in the chain: whether the contest information that changed beliefs also changed plans. We refer to the dependent variable of this regression as the *weight on the committed easy allocation*, denoted w_e. It is defined as the difference between the committed unconditional and committed hard allocations divided by the difference between the committed easy and committed hard allocations.[43] Intuitively, w_e is the distance between the unconditional and hard allocations, expressed as a proportion of the distance between the easy and hard

[43]In other words, w_e is found by solving $u = w_e e - (1 - w_e)h$ where u is the committed unconditional allocation, e is the committed easy allocation, and h is the committed hard allocation. For subjects who set the easy allocation equal to the hard allocation at a given piece rate, this ratio is undefined, and such observations are excluded from the regression. Table A13 reports similar effects on related dependent variables, $\frac{w_e}{1-w_e}$ and $u - h$ (the numerator of w_e, which is defined for all subjects).

allocations. For example, a high w_e means the unconditional allocation is far from the hard allocation, and therefore close to the easy allocation. The results in Table 3 Column 2 show that the 2-win message brings the unconditional allocation closer to the easy allocation, while the 0-win message brings the unconditional allocation closer to the hard allocation. This is consistent with the logic of Section 2.1: when subjects become less optimistic, their unconditional choices move closer to what they choose when they *know* the bad outcome will occur.

Table 4: Effect of contest information on contest-dependent task reallocation

	R_{CD} reallocation
0-win message ($\hat{\alpha}_0$)	-0.20
	(0.30)
1-win message ($\hat{\alpha}_1$)	0.43
	(0.28)
2-win message ($\hat{\alpha}_2$)	0.88
	(0.32)
$\hat{\alpha}_2 - \hat{\alpha}_0$	1.08
Right-tailed p value	.0036
Subjects	888
Observations	2322

Notes: Results are from estimating equation (1) using the online experiment sample. The dependent variable is contest-dependent reallocation (committed unconditional minus uncommitted easy/hard). In parentheses are standard errors clustered at the subject level.

Having evaluated mechanisms, we proceed to the outcome of primary interest: reallocation behavior. Table 4 shows the results of estimating equation (1) where the dependent variable is contest-dependent reallocation (R_{CD}). Treated individuals who received a zero-win message decreased their contest-dependent task reallocations relative to the control group, while two-win message recipients did the opposite. Comparing the effects on these two groups (taking the difference between $\hat{\alpha}_2$ and $\hat{\alpha}_0$), the gap between planned and actual work was one task greater for subjects who received the two-win message relative to the those who received the zero-win message (right-tailed p value of 0.0036).[44]

[44]The test statistic $\hat{\alpha}_2 - \hat{\alpha}_0$ and the one-tailed alternative hypothesis were pre-specified.

This result is clearly consistent with Hypothesis BBS. Information that makes subjects less optimistic (0-win signals) decreases reallocations, while information that makes them more optimistic (2-win signals) increases reallocations. Because this information has an effect on allocations, the result is not consistent with Hypothesis NNS, showing that reallocations cannot be caused solely by naïve nonstationarity.

3.2 Procrastination Reminder Effect on Time-Based Reallocation and Commitment Demand

3.2.1 Estimating Equation and Identification

In this section, we examine the effects of information about past procrastination on both task reallocation and commitment demand, in both the lab and online experiments. The estimating equations for all results are versions of the following:

$$y_i = \gamma_1 W_i + \gamma_2 R_{R,i1} + \gamma_3 W_i R_{R,i1} + \boldsymbol{Z_i'\theta_3} + \boldsymbol{X_i'\theta_4} + \nu_i. \tag{2}$$

The dependent variable, denoted generically by y_i, depends on the hypothesis being studied. To study commitment demand in both the online and lab-based experiments, the outcome variable is the change in commitment demand for subject i between week 1 and week 2, denoted C_i. To study task reallocation, the outcome variable is the number of tasks reallocated in week 2. For the lab experiment, this is the number of tasks that the subject committed to completing minus the uncommitted allocation that the subject completed. For the online experiment, this is $R_{CI,ir}$, the difference between committed and uncommitted allocations within piece rate and information condition (easy/hard/unconditional). This reallocation is contest-independent (CI), in contrast to the contest-dependent reallocation ($R_{CD,ir}$) analyzed in Section 3.1.

The primary right-hand-side variables are an indicator for being in the reallocation reminder treatment group (W_i), task reallocation in week 1 (which we denote $R_{R,i1}$ because it is the reallocation behavior that was *reported* to treated subjects), and the interaction of these two variables.[45] The vector $\boldsymbol{Z_i}$ contains control variables for features of the experimental design and the vector $\boldsymbol{X_i}$ contains controls for baseline (week 1) characteristics,[46] while ν_i is the stochastic error term.

[45] In the online experiment task reallocation regressions, we randomly selected one of the piece rates and provided information on that to the treated subjects. The outcome variable in that specification is piece-rate specific, and subjects could contribute up to three observations to the regression.

[46] For the lab experiment, design controls are indicators for study wave and receipt of a survey completion reminder in week 1. The set of possible variables for the LASSO procedure are gender

The main parameter of interest is γ_3, which we use to test Hypotheses BBS and NNS. The hypotheses concern the effect on subsequent choices of changes in beliefs, which we assume are affected by information on past choices. For naive non-stationary subjects, we assume that reminders about past choices affect beliefs about the subject's own preferences. For optimistic subjects, we assume that reminders about past choices affect beliefs about future shocks. As we control for week-1 reallocation, γ_3 captures whether information-treated subjects reallocate differently in week 2 than their week-1 behavior would predict.

One important additional assumption must be invoked for the lab experiment: that for optimistic subjects, the treatment affected the subjects' beliefs more about night 1 (Monday) than night 2 (Wednesday). This is a natural assumption because the treatment reminded the subjects specifically about choices made on Monday of the previous week.[47] This assumption is not required for the online experiment.[48]

3.2.2 Optimism and Present Bias in the Lab

Table 5 shows the results from reporting past reallocation behavior. Column 1 shows the effect on week 2 task reallocation, and the additional effect of being treated with non-zero information is shown by coefficient on the interaction. The estimate of -0.44 shows that for subjects who reallocated work in week 1, the treatment caused a statistically significant and practically substantial reduction in the reallocation of tasks in week 2.[49] Column 2 shows the treatment effect on the change in commitment demand, with the estimated coefficient on the interaction term positive and statistically significant. This estimate is consistent with a present-biased individual updating her belief over her present bias and increasing her commitment demand in response.

The estimates from Table 5 Column 1 are inconsistent with Hypothesis NNS, but are consistent with Hypothesis BBS. Intuitively, our test is whether reminding a subject of a past reallocation causes them to update their beliefs either about their own non-stationarity or about the shocks that they face. The fact that reallocations

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and age indicators, GPA and GPA squared, an employment indicator, and week-one time spent on socializing and studying. For the online experiment, controls are identical to those described in Section 3.1.1. Results without LASSO-selected controls appear in Tables A3 and A11.

[47] Formally, this will hold if the distribution of cost shocks depends partly on the day of the week.

[48] All tasks in the online experiment were completed at once at a piece rate. Information about time shocks could only affect beliefs about costs, not about the benefits of completing more tasks.

[49] The other coefficients are not of primary interest. The coefficient on reallocation in week 1 shows that a subject's reallocations are correlated across weeks: Subjects who reallocated more tasks in week 1 also did so in week 2.

Table 5: Lab experiment: Effect of task information on contest-independent task reallocation and commitment demand

	(1) R_{CI} reallocation	(2) ΔCommitment demand
Task message	0.20	0.040
	(0.37)	(0.41)
Reported reallocation	0.31	0.015
	(0.13)	(0.10)
Task message × reported reallocation	-0.44	0.44
	(0.20)	(0.18)
Two-tailed p value, interaction	0.027	0.011
Subjects	209	209
Observations	209	209

Notes: The table shows results from estimating equation (2) on the lab experiment sample. Column (1) shows the effect on task reallocation of being treated with messages about week 1 task reallocation. Column (2) shows the effect of the same treatment on the change in commitment demand. In parentheses are standard errors clustered at the subject level.

change in week 2 as a result of these reminders indicates that the information cannot *solely* be affecting beliefs about nonstationarity.

However, the evidence from Table 5 Column 2 shows that the reminders did have an effect on commitment demand, consistent with Hypothesis NNS. Our interpretation is that naive non-stationarity contributes to the reallocations of subjects in the lab experiment.[50]

3.2.3 Optimism and Present Bias Online

Table 6 reports results from the online replication of the lab-based experiment. The structure of the table follows that of Table 5, with the first column showing the effect of treatment on contest-independent task reallocation behavior and the second column showing the effect on the change in commitment demand.

The effect of the interaction of treatment on individuals with a larger reported reallocation is qualitatively similar to the result from the lab experiment. The treatment caused these individuals to reduce their task-independent reallocation in week 2

[50]While estimates could be consistent with updating the beliefs of an agent with biased beliefs about shocks, the theoretical prediction in that context is ambiguous and we surmise that any such effect would be small.

Table 6: Online experiment: Effect of task information on contest-independent task reallocation and commitment demand

	(1) R_{CI} reallocation	(2) ΔCommitment demand
Task message	0.31	0.075
	(0.14)	(0.11)
Reported reallocation	0.10	-0.0052
	(0.021)	(0.015)
Task message × reported reallocation	-0.062	0.0072
	(0.037)	(0.025)
Left-tailed p value, interaction	0.045	0.39
Subjects	888	888
Observations	4644	888

Notes: Results are from estimating equation (2) on the online experiment sample. Column (1) shows the effect on contest-independent task reallocation of being treated with messages about week 1 reported task reallocation. Column (2) shows the effect of the same treatment on the change in commitment demand. In parentheses are standard errors clustered at the subject level.

by 0.06.[51] As in the lab experiment (Table 5 Column 1), these results are inconsistent with Hypothesis NNS but consistent with Hypothesis BBS.

Table 6 Column 2 shows that, in contrast to the lab experiment, treatment did not have a statistically significant effect on the change in commitment demand for individuals who reallocated more in week 1. There are multiple potential explanations. First, sample populations differed. Students might be more present biased or more likely to update about their bias. While there do appear to be differences in the initial distributions of commitment demand between samples, we do not find substantially different average reallocation behavior. Second, there were slight differences between the experiments in the way commitment demand was elicited. The range of possible "prices" (denominated in tasks) was smaller online, and the tasks that the subjects had to do to pay for commitment were completed at the time of commitment rather than alongside the other tasks. Finally, it is possible that the original result was a false positive.

[51] Comparing the results in the lab and online experiments, we find that among untreated subjects, an additional unit of reallocation in Week 1 is associated with an additional Week 2 reallocation of 0.3 tasks in the lab experiment, but only 0.1 tasks in the online experiment. Thus it is not surprising that the magnitude of the treatment effect also falls.

4 Conclusion

This paper models agents whose dynamic inconsistency potentially arises from two sources: naive non-stationary preferences and biased beliefs over cost shocks. Agents with optimistic beliefs about shocks will exhibit dynamically-inconsistent choices over effort that are observationally equivalent to those driven by naive non-stationarity. An information intervention that tells agents about their past time inconsistency, however, can distinguish these models: optimistic agents will change effort allocations, but naive agents with non-stationary preferences will not.

We test these predictions experimentally and find that biased beliefs about shocks do matter for time inconsistency. The results help explain puzzlingly low take-up of costly commitment. Perhaps more importantly, they offer an alternative policy prescription to help overcome time-inconsistent behavior—providing information on agents' own past execution of their plans just prior to a new decision. One avenue for future research is to identify situations in which subjects demand this information, and how it can be structured to reduce time inconsistency with minimal associated welfare losses.

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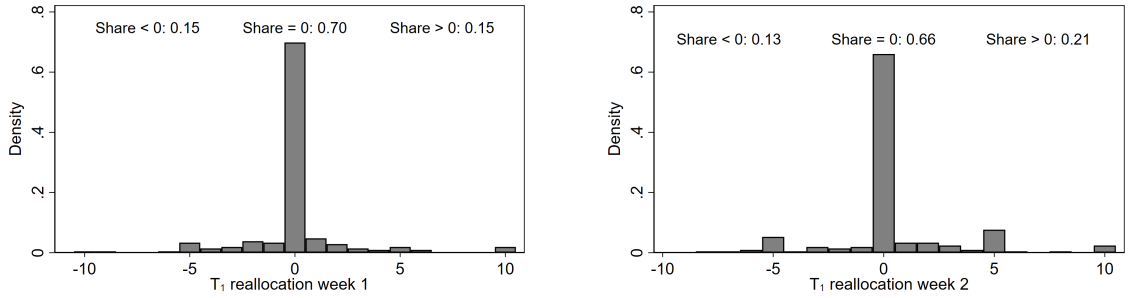
Appendix for Why Do We Procrastinate? Present Bias and Optimism

A Appendix figures and tables

A.1 Appendix figures

A.1.1 Lab experiment

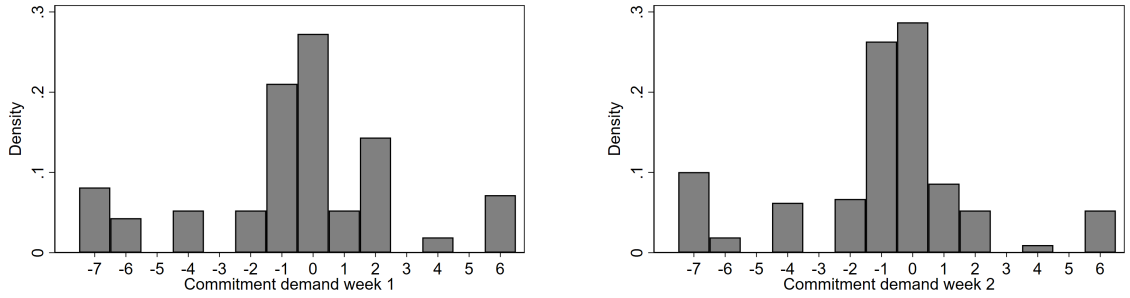
Figure A1: Lab experiment: Distribution of task reallocation



(a) Task reallocation in week 1 (b) Task reallocation in week 2

Notes: The figure shows the distribution of task reallocation in the lab experiment. Panel (a) shows week 1, before treatment. Panel (b) shows week 2, after treatment. The x-axis is the number of tasks put off (committed minus uncommitted).

Figure A2: Lab experiment: Distribution of commitment demand

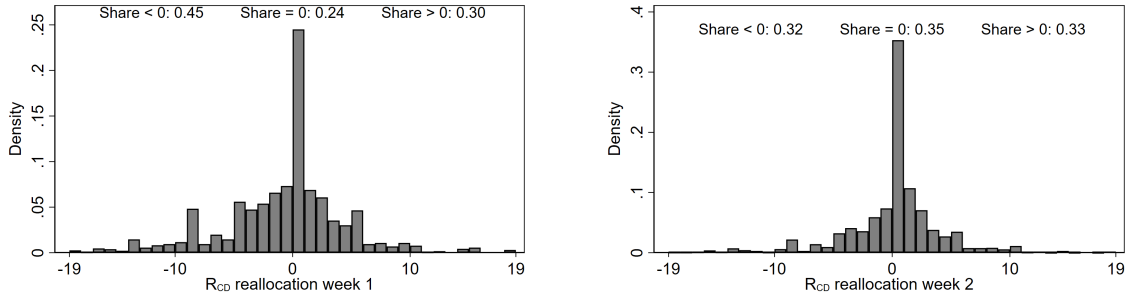


(a) Commitment demand in week 1 (b) Commitment demand in week 2

Notes: The figure shows the distribution of commitment demand in the lab experiment. Panel (a) shows week 1, before treatment. Panel (b) shows week 2, after treatment. The x-axis shows the *maximum* price the subject was willing to pay for commitment in terms of extra tasks. A commitment demand of one indicates that the subject was willing to do one extra task to be committed, but was unwilling to do two. The in-kind price could take on both positive and negative values. Subjects who were unwilling to commit even if it lowered the number of tasks they had to do by six were assigned a commitment demand of negative seven.

A.1.2 Online experiment

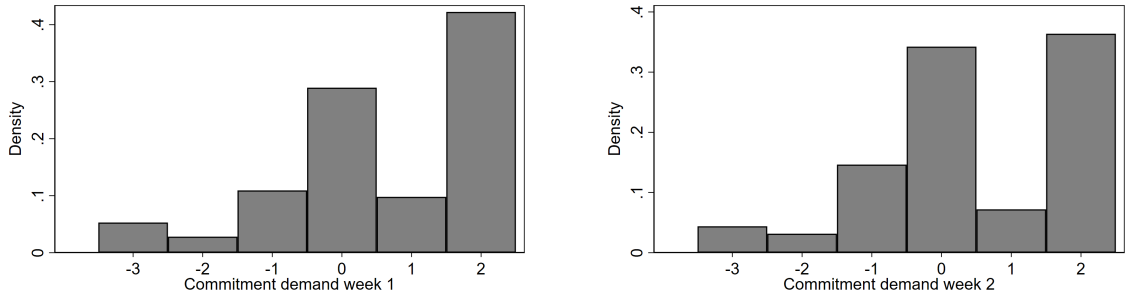
Figure A3: Online experiment: Distribution of task reallocation



(a) R_{CD} task reallocation in week 1 (b) R_{CD} task reallocation in week 2

Notes: The figure shows the distribution of task reallocation (R_{CD}) in the online experiment. Panel (a) shows week 1, before treatment. Panel (b) shows week 2, after treatment. The x-axis shows the number of committed unconditional tasks minus uncommitted realized (easy/hard) tasks.

Figure A4: Online experiment: Distribution of commitment demand



(a) Commitment demand in week 1 (b) Commitment demand in week 2

Notes: The figure shows the distribution of commitment demand in the online experiment. Panel (a) shows week 1, before treatment. Panel (b) shows week 2, after treatment. The x-axis shows the *maximum* price the subject was willing to pay for commitment in terms of extra tasks. A commitment demand of one indicates that the subject was willing to do one extra task to be committed, but was unwilling to do two. The in-kind price could take on both positive and negative values. Subjects who were unwilling to commit even if it lowered the number of tasks they had to do by two were assigned a commitment demand of negative three.

A.2 Appendix tables

A.2.1 Lab experiment

Table A1: Lab experiment: Test of attrition predictors

	Finished study
Treat	0.052
	(0.053)
Age	0.018
	(0.013)
GPA	0.070
	(0.055)
Female (indicator)	0.035
	(0.054)
Study wave	0.024
	(0.023)
F	1.21
p-value	0.30
Observations	273

Notes: The table shows an omnibus F-test on attrition. Sample includes all lab experiment subjects who completed our baseline survey instrument: 64 who did not complete the study and 209 who did. Estimates are from a regression of a study completion dummy on the listed variables. No other variables are included. In parentheses are heteroskedasticity-robust standard errors (White, 1980).

Table A2: Lab experiment: Summary statistics and covariate balance

	(1) Control Mean/(SD)	(2) Treated Mean/(SD)	(3) Diff./(SE)
Commitment demand week 1	-0.92	-0.13	-0.79
	(3.04)	(3.27)	(0.44)
Dyn. inconsistent week 1 (indicator)	0.35	0.26	0.093
	(0.48)	(0.44)	(0.064)
Bedtime difference from plan (minutes)	36.1	39.8	-3.68
	(68.7)	(53.9)	(8.50)
GPA (4 point scale)	3.22	3.31	-0.085
	(0.49)	(0.47)	(0.066)
Female (indicator)	0.54	0.70	-0.16
	(0.50)	(0.46)	(0.067)
Study wave	2.47	2.56	-0.090
	(1.13)	(1.09)	(0.15)
Observations	100	109	

Notes: The table shows pre-treatment summary statistics for the baseline sample from the lab experiment, broken down by treatment status. Columns 1 and 2 show average values with standard deviations in parentheses below for the control and treatment groups respectively. Column 3 shows the difference in means, with standard errors in parentheses below.

Table A3: Lab experiment: Effect of task information on contest-independent task reallocation and commitment demand, design controls only

	(1) R_{CI} reallocation	(2) ΔCommitment demand
Task message	0.32	-0.10
	(0.37)	(0.41)
Reported reallocation	0.33	0.0063
	(0.14)	(0.10)
Task message × reported reallocation	-0.41	0.43
	(0.20)	(0.19)
Two-tailed p value, interaction	0.045	0.027
Subjects	209	209
Observations	209	209

Notes: The table shows results from estimating a variant of equation (2) on the lab experiment sample. Column (1) shows the effect on task reallocation of being treated with messages about week 1 task reallocation. Column (2) shows the effect of the same treatment on the change in commitment demand. All baseline controls are excluded. In parentheses are standard errors clustered at the subject level.

A.2.2 Online experiment

Table A4: Online experiment: Test of attrition predictors

	(1) Finished study	(2) Finished study
Study wave	0.0023	-0.0084
	(0.021)	(0.0042)
Patience (11 point scale)	0.014	0.0018
	(0.0046)	(0.0011)
Risk tolerance (11 point scale)	-0.020	0.00041
	(0.0040)	(0.00052)
Manager (indicator)	-0.046	-0.0080
	(0.029)	(0.0055)
Employed (indicator)	-0.019	-0.0043
	(0.024)	(0.0028)
Dyn. inconsistent (R_{CD}) week 1		-0.0053
		(0.0027)
Subject win prob. week 1		0.000011
		(0.000013)
Sample	All subjects	Week 1 finishers
F	7.11	0.58
p-value	0.0000014	0.77
Observations	1479	1182

Notes: The table shows an omnibus F-test on attrition. Sample includes all online experiment subjects who completed the session 1 survey: 301 who did not complete the study and 1178 who did. Estimates are from a regression of a study completion dummy on the listed variables. No other variables are included. In parentheses are heteroskedasticity-robust standard errors (White, 1980).

Table A5: Online experiment: Descriptive statistics and covariate balance for contest information treatment

	(1) Control Mean/(SD)	(2) Treated Mean/(SD)	(3) Diff./(SE)
Task reallocation (R_{CD}) week 1	-0.90	-0.94	0.041
	(4.6)	(4.2)	(0.36)
Dyn. inconsistent (R_{CD}) wk 1 (indicator)	0.86	0.87	-0.018
	(0.35)	(0.33)	(0.028)
Commitment demand week 1	0.63	0.56	0.071
	(1.45)	(1.44)	(0.12)
Subject win prob. week 1	54.1	54.2	-0.077
	(25.2)	(25.4)	(2.10)
Female (indicator)	0.40	0.41	-0.012
	(0.49)	(0.49)	(0.041)
Study wave	1.40	1.38	0.014
	(0.49)	(0.49)	(0.040)
Born in US (indicator)	0.93	0.93	-0.00044
	(0.26)	(0.26)	(0.021)
English first lang. (indicator)	0.94	0.95	-0.013
	(0.24)	(0.22)	(0.019)
College degree (indicator)	0.54	0.50	0.042
	(0.50)	(0.50)	(0.041)
Age	39.4	38.9	0.54
	(11.5)	(10.9)	(0.93)
Observations	283	299	

Notes: The table shows summary statistics for the baseline sample from the lab experiment, broken down by treatment status for the contest information treatment. Observation counts do not sum to 888 because the task-information group is not included. Columns 1 and 2 show mean values with standard deviations in parentheses below for the control group and the contest treatment group respectively. Column 3 shows the difference between the means with standard errors below. The first variable ("Dyn. inconsistent (R_{CD})") is an indicator for whether the subject had at least 1 dynamically inconsistent allocation choice for the R_{CD} allocation (unconditional committed minus realized uncommitted).

Table A6: Online experiment: Descriptive statistics and covariate balance for task information treatment

	(1) Control Mean/(SD)	(2) Treated Mean/(SD)	(3) Diff./(SE)
Task reallocation (R_{CI}) week 1	-0.82	-1.20	0.39
	(3.5)	(3.5)	(0.29)
Dyn. inconsistent (R_{CI}) wk 1 (indicator)	0.90	0.91	-0.0077
	(0.30)	(0.29)	(0.025)
Commitment demand week 1	0.63	0.66	-0.028
	(1.45)	(1.47)	(0.12)
Subject win prob. week 1	54.1	54.2	-0.13
	(25.2)	(25.2)	(2.08)
Female (indicator)	0.40	0.48	-0.074
	(0.49)	(0.50)	(0.041)
Study wave	1.40	1.38	0.013
	(0.49)	(0.49)	(0.040)
Born in US (indicator)	0.93	0.91	0.018
	(0.26)	(0.28)	(0.022)
English first lang. (indicator)	0.94	0.92	0.012
	(0.24)	(0.26)	(0.021)
College degree (indicator)	0.54	0.52	0.021
	(0.50)	(0.50)	(0.041)
Age	39.4	38.8	0.60
	(11.5)	(11.4)	(0.95)
Observations	283	306	

Notes: The table shows summary statistics for the baseline sample from the lab experiment, broken down by treatment status for the task information treatment. Observation counts do not sum to 888 because the contest-information group is not included. Columns 1 and 2 show mean values with standard deviations in parentheses below for the control group and the task information treatment group respectively. Column 3 shows the difference between the means with standard errors below. The first variable ("Dyn. inconsistent (R_{CI})") is an indicator for whether the subject had at least 1 dynamically inconsistent allocation choice for the R_{CI} allocation (committed minus uncommitted within piece rate and information condition).

Table A7: Effect of contest information on contest-dependent task reallocation, full sample

	R_{CD} reallocation
0-win message ($\hat{\alpha}_0$)	-0.31
	(0.25)
1-win message ($\hat{\alpha}_1$)	0.52
	(0.28)
2-win message ($\hat{\alpha}_2$)	0.68
	(0.25)
$\hat{\alpha}_2 - \hat{\alpha}_0$	0.99
Right-tailed p value	.00097
Subjects	1178
Observations	3534

Notes: Results are from estimating equation (1) using the full online experiment sample, without the restriction based on week-1 censoring. The dependent variable is contest-dependent reallocation (committed unconditional minus uncommitted easy/hard). In parentheses are standard errors clustered at the subject level.

Table A8: Effect of contest information on contest-dependent task reallocation, design controls only

	R_{CD} reallocation
0-win message ($\hat{\alpha}_0$)	-0.16
	(0.41)
1-win message ($\hat{\alpha}_1$)	0.43
	(0.39)
2-win message ($\hat{\alpha}_2$)	0.88
	(0.42)
$\hat{\alpha}_2 - \hat{\alpha}_0$	1.04
Right-tailed p value	.025
Subjects	888
Observations	2322

Notes: Results are from estimating a variant of equation (1) using the online experiment sample. The dependent variable is contest-dependent reallocation (committed unconditional minus uncommitted easy/hard). All baseline controls are excluded. In parentheses are standard errors clustered at the subject level.

Table A9: Effect of contest information on win belief and contest-dependent task reallocation, PAP specification

	R_{CD} reallocation
0-win message ($\hat{\alpha}_0$)	-0.24
	(0.41)
1-win message ($\hat{\alpha}_1$)	0.41
	(0.43)
2-win message ($\hat{\alpha}_2$)	0.57
	(0.44)
$\hat{\alpha}_2 - \hat{\alpha}_0$	0.82
Right-tailed p value	.088
Subjects	888
Observations	2322

Notes: Results are from estimating a variant of equation (1), exactly as in the PAP, using the online experiment sample. The dependent variable is contest-dependent reallocation (committed unconditional minus uncommitted easy/hard). In parentheses are standard errors clustered at the subject level.

Table A10: Online experiment: Effect of task information on contest-independent task reallocation and commitment demand, full sample

	(1) R_{CI} reallocation	(2) ΔCommitment demand
Task message	0.33	0.093
	(0.12)	(0.098)
Reported reallocation	0.076	-0.0040
	(0.015)	(0.013)
Task message × reported reallocation	-0.032	0.017
	(0.031)	(0.023)
Left-tailed p value, interaction	0.15	0.23
Subjects	1178	1178
Observations	7068	1178

Notes: The table shows results from estimating equation (2) on the full online experiment sample, without the restriction based on week-1 censoring. Column (1) shows the effect on contest-independent task reallocation of being treated with messages about week 1 reported task reallocation. Column (2) shows the effect of the same treatment on the change in commitment demand. In parentheses are standard errors clustered at the subject level.

Table A11: Online experiment: Effect of task information on contest-independent task reallocation and commitment demand, design controls only

	(1) R_{CI} reallocation	(2) ΔCommitment demand
Task message	0.32	0.029
	(0.21)	(0.14)
Reported reallocation	0.10	-0.015
	(0.034)	(0.021)
Task message × reported reallocation	-0.059	0.0099
	(0.060)	(0.034)
Left-tailed p value, interaction	0.16	0.39
Subjects	888	888
Observations	4644	888

Notes: The table shows results from estimating a variant of equation (2) on the online experiment sample. Column (1) shows the effect on contest-independent task reallocation of being treated with messages about week 1 reported task reallocation. Column (2) shows the effect of the same treatment on the change in commitment demand. All baseline controls are excluded. In parentheses are standard errors clustered at the subject level.

Table A12: Online experiment: Effect of task information on contest-independent task reallocation and commitment demand, PAP specification

	(1) R_{CI} reallocation	(2) ΔCommitment demand
Reported reallocation	0.10	0.00020
	(0.033)	(0.015)
Task message × reported reallocation	-0.073	0.014
	(0.059)	(0.027)
Left-tailed p value, interaction	0.11	0.30
Subjects	888	888
Observations	4644	888

Notes: The table shows results from estimating a variant of equation (2), exactly as in the PAP, on the online experiment sample. Column (1) shows the effect on contest-independent task reallocation of being treated with messages about week 1 reported task reallocation. Column (2) shows the effect of the same treatment on the change in commitment demand. In parentheses are standard errors clustered at the subject level.

Table A13: Effect of contest information on alternative weights for easy allocation

	(1) $w_e/1-w_e$	(2) $(u-h)$
0-win message ($\hat{\alpha}_0$)	-0.32	-0.078
	(0.16)	(0.16)
1-win message ($\hat{\alpha}_1$)	-0.16	-0.095
	(0.13)	(0.15)
2-win message ($\hat{\alpha}_2$)	-0.054	0.13
	(0.16)	(0.19)
$\hat{\alpha}_2 - \hat{\alpha}_0$	0.27	0.21
Right-tailed p value	.1	.17
Subjects	460	888
Observations	934	2322

Notes: Results are from estimating versions of equation (1) on the online experiment sample. The dependent variable in Column (1) is relative (rather than absolute) weight on the committed easy allocation, which is not defined for all subjects. In Column (2) it is the numerator of $u-h$ of the weight w_e on the committed easy allocation, which is defined for all subjects. In parentheses are standard errors clustered at the subject level.

B Deviations from the pre-analysis plan, online experiment

No pre-analysis plan (PAP) was registered for the lab experiment. For the online experiment, this list describes deviations from the PAP. Below, we present evidence that these deviations do not meaningfully change point estimates.

- The PAP describes *ad hoc* control selection and mentions LASSO control selection as a robustness check. To maximize statistical power, we employ LASSO selection of baseline controls in the primary estimates. To demonstrate the harmlessness of this choice, Tables A8 and A11 present analogs of our primary results without any baseline controls. Point estimates are strongly similar; the LASSO procedure is not required to correct chance imbalances from the randomization. Precision is predictably reduced, but note that the complete exclusion of baseline controls is not required by the PAP.
- In equation (2), the first term is W_i, the indicator for task information treatment. The PAP erroneously omitted this term, the analog of which was previously included in previous drafts of this paper. Such omission assumes that the effect of a zero-reallocation treatment message on outcomes is zero, which may be empirically false.
- In the PAP, estimating equations included indicators for the number of wins that did not interact with treatment. Conditional on contest score the number of wins is exogenous, and these win-count (contest-information) indicators were not shown to subjects so they could not affect behavior. To simplify notation we omit these variables.

Tables A9 and A12 display results from regressions that follow the PAP exactly. Because LASSO control selection is not employed in these tables, the precision of pre-specified test statistics of interest is weakly less. Differences in point estimates are described below. In Table A9, our pre-specified test statistic of interest ($\hat{\alpha_2} - \hat{\alpha_0}$) is modestly smaller than our primary result (0.82 rather than 1.02). This difference is small relative to the associated standard errors. In Table A12, our pre-specified test statistic of interest (the coefficient on the interaction of the task message and the reported reallocation) for R_{CI} reallocation is slightly larger in magnitude than our primary result (-0.073 rather than -0.062). For the change in commitment demand, it is also slightly larger (0.014 rather than 0.0072). Both differences are small relative to the associated standard errors.

Appendix References

White, H. (1980). A heteroskedasticity-consistent covariance matrix estimator and a direct test for heteroskedasticity. *Econometrica 48*(4), 817–838.

Experimental Instruction Appendix for "Why Do We Procrastinate: Present Bias and Optimism"

I Lab-Based Experiment Instructions

This appendix includes a representative portion of the instructions and surveys for the lab-based experiment. Because the survey presentation varied from subject to subject (due to them receiving a different treatment, having different choices randomly selected to be implemented, etc.), we print the survey instrument directly from Qualtrics. Thus, the surveys rely on Qualtrics' internal logic and referencing system. Subjects only see one version of any question, and any reference fields would be replaced with the information which was appropriate for that subject. The full set of surveys is omitted for space reasons and because they do not differ significantly from the materials provided here. They will be provided by the authors upon request.

Appendix I.1 provides the introductory script with the associated presentation that was read to subjects. Appendix I.2 provides the survey that subjects completed at the introductory session.

Appendix I.3 provides the morning survey that subjects completed on Monday of the second week of the experiment. The survey given on Monday of the first week looked identical except for the treatment, which was omitted. The survey given on every other morning looked identical except that it did not include the treatment, the commitment demand elicitation, or the allocation decision.

Appendix I.3 provides the evening survey and first task that subjects completed on Monday of the second week of the experiment. The evening survey subjects completed on Monday of the first week of the experiment looked identical. The evening survey subjects completed on Wednesday of both weeks looked identical except that it did not include the allocation decision.

I.1 Introductory Script and Slides

Time Use Study Script

Hello everyone, my name is Zachary. Thank you for your participation in this study about sleep and time use.

Slide

This study requires participation over two weeks. To participate, you must be willing to:

- wear the Fitbit wristband on Sunday, Monday, Tuesday, and Wednesday nights of this coming week,
- to complete a series of 8 surveys on Monday through Thursday morning of the next two weeks,
- to complete a series of 4 tasks on Monday and Wednesday evenings of the next two weeks,
- to return the Fitbit wristband to the economics department on Thursday or Friday of next week, and
- to pick up your payment on Thursday or Friday of the second week.

Surveys and tasks will be completed online, and the link will be sent to you when it is time to complete the survey.

Slide

This is a picture of the Fitbit wristband that you will be required to wear to bed Sunday through Thursday of this coming week. You should only wear it when you're in bed, not during daytime hours. If the band gets wet, dry it off. You are required to return the band to the Economics Department on Thursday or Friday of next week, between the hours of 8AM and 6PM.

Slide

The tasks you must complete in the evenings consist of moving sliders to a predetermined level, which is given to the left of the slider. You will be unable to move on until you match each slider to the given level. While the morning surveys can be completed on a phone or tablet, it is recommended that you complete these tasks on a computer. You'll complete several example tasks during the initial survey at the end of this session to see what they're like.

Slide

Participation in the study requires completing the surveys and tasks at particular times. Morning surveys must be completed before noon, and evening tasks must be completed between 9PM and 2AM. Furthermore, the Fitbit wristband must be returned during business hours on Thursday or Friday next week, and the final payment must be picked up on Thursday or Friday in two weeks. If you are unable to complete

the requirements of this study, you are free to leave at this point, as payments will be forfeit if the requirements are not met.

Slide

You have been given a consent form that describes your rights. Please look it over now while we prepare the remainder of this session. If you have questions, please raise your hand and I'll come address them. We'll pick up the forms before continuing.

Unplug projector and prepare survey

You may open your computers now. The link for the initial survey should now have been sent to the email address you gave us, although it may take a few minutes to arrive. Please log in to your email account and follow the directions. When you have completed the initial survey, you may come to me to receive your Fitbit wristband.

Give band and payment

Here is your sleep tracker. Remember to wear it when you sleep Sunday night through Wednesday night. Your first survey will arrive on Monday morning.

Time Use Study

Requirements

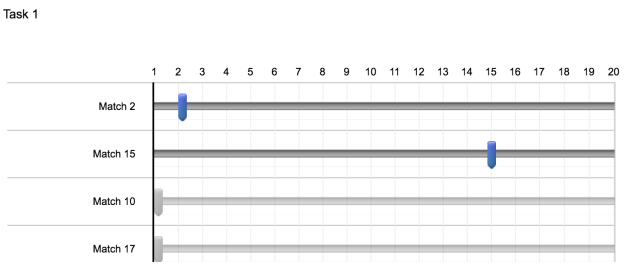
- Wear the Fitbit wristband on Sunday, Monday, Tuesday, and Wednesday nights of the first week
- Complete eight morning surveys, and 4 sets of evening tasks, all of which will be emailed to you
- Return the Fitbit wristband and pick up $10 payment at the economics department on Thursday or Friday of the first week
- Pick up your $30 payment from the economics department on Thursday or Friday of the second week

Fitbit Wristband

- Wear the band on Sunday, Monday, Tuesday, and Wednesday nights of the first week, *only when you are in bed*
- Return them to the economics department next Thursday or Friday

Tasks

- You'll be asked to move sliders to a particular level



Timing

To participate in the study, you must be able to do all of the following:

- eight surveys completed at home, lasting 5-10 minutes, **to be completed before noon** on Monday-Thursday of next week and the week after
- four surveys completed at home, lasting 15-20 minutes, **to be completed between 9PM and 2AM** on Monday and Wednesday night of next week and the week after
- return the Fitbit wristband during business hours (8AM to 6PM) on Thursday June 1st or Friday June 2nd.
- pick up your payment during business hours (8AM to 6PM) on Thursday June 8th or Friday June 9th.

Informed Consent

- You received a consent form after you entered that describes your rights as a subject. Please read it.
- If you would no longer like to be part of this study, you are free to leave at this point.

I.2 Introductory Survey

Intro Survey

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First Click: **0 seconds**
Last Click: **0 seconds**
Page Submit: **0 seconds**
Click Count: **0 clicks**

Welcome to the experiment. Before explaining what will be happening for the rest of the experiment, please answer this short survey about yourself and your sleep and work habits.

What is your first name?

What is your last name?

What is your email address?

What is your phone number?

What is your PID?

What is your college major?
If you do not have a college major, write "undeclared".

What is your current GPA?

What is your gender?
If you do not wish to provide gender information, you may leave this question blank.

What is your current age?

Are you employed?

- Yes
- No

If you are employed, what is your hourly wage?
If you are not employed, please leave blank.

How many hours per day did you usually spend studying last quarter?

Would you say that you tend to procrastinate?

- Yes
- No

How much do you generally sleep on weekdays?

How much do you generally sleep on weekends?

How many hours would you like to sleep per night?

Please choose the response that best represents about how many nights per week, on average, you had trouble sleeping last quarter.

- 0
- 1
- 2
- 3
- 4
- 5
- 6
- 7

How many hours do you feel like you would need to sleep every night for 1 week to feel completely rested?

What is your usual bedtime on weekdays?

	Hour	Minute	AM/ PM
Weekday Bedtime			

What is your usual bedtime on weekends?

	Hour	Minute	AM/ PM
Weekend Bedtime			

What time do you usually wake up on weekdays?

	Hour	Minute	AM/ PM
Weekday wake-up			

What time do you usually wake up on weekends?

	Hour	Minute	AM/ PM
Weekend wake-up			

Do you live in UC San Diego on-campus housing?

- ○ Yes
- ○ No

Please select your residence hall from the following list

What is the name of the neighborhood where you live?

From the following items, select any that you generally keep in the room where you most often sleep.

- ☐ Television
- ☐ Desktop computer
- ☐ Video game console
- ☐ iPad or other tablet
- ☐ Laptop computer

Do you take naps?

- ○ Yes
- ○ No

If you take naps, how long do you usually nap?
If you do not usually take naps, please leave blank.

How often do you wake up and feel like you wish you had gone to bed earlier?

- Never
- Rarely
- 1-2 times per week
- 3-4 times per week
- 5-6 times per week
- Every day

How often do work or studying make it hard to go to bed when you'd like?

- Never
- Rarely
- 1-2 times per week
- 3-4 times per week
- 5-6 times per week
- Every day

Personality

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Click Count: **0 clicks**

Here are a few more questions about your attitude and behavior. Please be as honest and accurate as you can throughout. Try not to let your response to one statement influence your responses to other statements. There are no "correct" or "incorrect" answers. Answer according to your own feelings, rather than how you think "most people" would answer.

In uncertain times, I usually expect the best.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly Disagree

It's easy for me to relax.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

If something can go wrong for me, it will.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

I'm always optimistic about my future.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

I enjoy my friends a lot.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

It's important for me to keep busy.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

I hardly ever expect things to go my way.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree
- Strongly disagree

I don't get upset too easily.

- Strongly agree
- Agree
- Neither agree nor disagree
- Disagree

I.3 Week 2 Monday Morning Survey

Default Question Block

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First Click: **0 seconds**

Last Click: **0 seconds**

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Click Count: **0 clicks**

Hi! This is a quick survey to better understand your daily sleep habits. Most of the questions ask you about your sleep last night. Please answer them as honestly as possible. You'll make decisions about tonight's tasks on the next page. Thanks!

How long did you study yesterday (in hours)?
Note: Study time does not include class time.

I want to ask you about how you spent your time yesterday. For each hour of the day, please select the activities you did.

	Sleeping	Socializing	Class	Studying	Exercising	Working at a job	Watching TV	Other
12:00 a.m. (Midnight)	☐	☐	☐	☐	☐	☐	☐	☐
1:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
2:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
3:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
4:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
5:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
6:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
7:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
8:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
9:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
10:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
11:00 a.m.	☐	☐	☐	☐	☐	☐	☐	☐
12:00 p.m. (noon)	☐	☐	☐	☐	☐	☐	☐	☐
1:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
2:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
3:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
4:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
5:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
6:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
7:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
8:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
9:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
10:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐
11:00 p.m.	☐	☐	☐	☐	☐	☐	☐	☐

What time do you plan on turning off the light to go to sleep tonight?

	Hour	Minute	AM/PM
Planned bedtime			

Which of the following did you do in the hour before you went to bed (select all that apply)?

- [ ] Watch TV
- [ ] Play video or computer games
- [ ] Use a computer or smart phone (other than for games)
- [ ] Exercise
- [ ] Read a book or non-backlit e-reader
- [ ] Other:

What time did you turn off the light intending to go to sleep last night?

	Hour	Minute	AM/PM
Light off last night			

How long did it take you to fall asleep last night (in minutes)?

How many times did you wake up last night?

What time did you wake up this morning (for the last time)?

	Hour	Minute	AM/PM
Wake-up time this morning			

What time did you get out of bed this morning (for the last time)?

	Hour	Minute	AM/PM
Out of bed this morning			

How well do you feel like you slept last night?

Slept very badly — Slept very well

0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

How tired did you feel today?

Not tired at all — Extremely tired

0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

How busy are you today?

Less busy than usual				About as busy as usual						More busy than usual
0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

Block 3

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, you moved {Invalid Expression} -${e://Field/Extra12})^2))} task from Wednesday to Monday.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, you moved {Invalid Expression} -${e://Field/Extra12})^2))} tasks from Wednesday to Monday.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, you moved {Invalid Expression} -${e://Field/Extra12})^2))} task from Monday to Wednesday.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, you moved {Invalid Expression} -${e://Field/Extra12})^2))} tasks from Monday to Wednesday.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Choosing the Implemented Allocation

Last week, on Monday morning you said you'd do +${e://Field/Extra11}} tasks on Monday evening and +10-${e://Field/Extra11}} tasks on Wednesday. When you were asked in the evening, you decided to do +${e://Field/Extra12}} on Monday, and +10-${e://Field/Extra12}} on Wednesday. **Thus, your choices did not change.**

Also, on average you predicted that your bedtime would be ${e://Field/PredictedBedtime}, and your actual average bedtime was ${e://Field/ActualBedtime}, **so you missed your predicted bedtime by about ${e://Field/DifferenceBedtime} minutes.**

Why might someone's choices and predictions change throughout the day?

Commitment

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Choosing the Implemented Allocation

Here is a series of choices that can affect the probability that the morning allocation will be the one that is chosen. To affect the probability, you may have to agree to do more baseline tasks. **You will never have to do tasks in the morning - these decisions just affect which allocation is implemented.**

This decision will measure the strength of your preference for which decision is implemented.

We'll randomly select which one of these decisions we implement. When you make these decisions, **treat every decision as if it is the one that counts because each decision is the one that could be implemented.**

16 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
14 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
12 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented

11 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
10 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
9 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
8 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
6 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented
4 mandatory tasks each night, 4 out of 5 chance of morning allocation being implemented	10 mandatory tasks each night, 1 out of 5 chance of morning allocation being implemented

Allocation Decision

We've randomly selected out of the previous choices, and you'll complete 10 mandatory tasks each night with a 1 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 16 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 14 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 12 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 11 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 10 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 9 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 8 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 6 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 4 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

I.4 Week 2 Monday Evening Survey

Default Question Block

These page timer metrics will not be displayed to the recipient.
First Click: **0 seconds**
Last Click: **0 seconds**
Page Submit: **0 seconds**
Click Count: **0 clicks**

Hi! This is a quick survey to better understand your daily sleep habits. Most of the questions ask you about your sleep last night. Please answer them as honestly as possible. You'll make decisions about tonight's tasks on the next page. Thanks!

How long did you study today (in hours)?
Note: Study time does not include class time.

What time do you plan on turning off the light to go to sleep tonight?

	Hour	Minute	AM/PM
Planned bedtime			

What time do you plan on waking up tomorrow?

	Hour	Minute	AM/PM
Planned wake-up			

What time do you plan on getting out of bed tomorrow?

	Hour	Minute	AM/PM
Getting out of bed			

How tired did you feel today?

Extremely tired ... Not tired at all

0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

How busy were you today?

Less busy than usual ... More busy than usual

0	1	2	3	4	5	6	7	8	9	10
○	○	○	○	○	○	○	○	○	○	○

Allocation Decision

We've randomly selected out of the previous choices, and you'll complete 10 mandatory tasks each night with a 1 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 16 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 14 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 12 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 11 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 10 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 9 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 8 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 6 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

We've randomly selected out of the previous choices, and you'll complete 4 mandatory tasks each night with a 4 out of 5 chance that your morning allocation will be the one that is implemented. There are 10 extra tasks to allocate between the nights. How many tasks would you like to do tonight?

Tasks 1-10

Completing Tasks

We've randomly selected that your **morning allocation** be the one implemented, so **tonight you will complete ${e://Field/Tasks21} tasks**, and Wednesday you will complete ${e://Field/Tasks22} tasks.

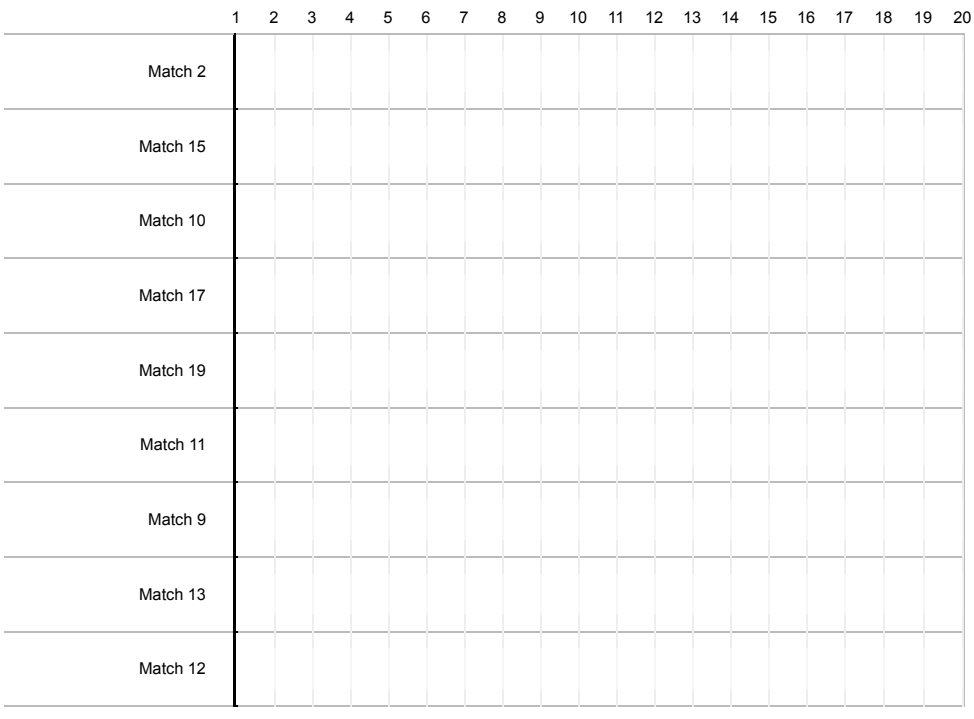
You may complete tonight's tasks below.

Completing Tasks

We've randomly selected that your **evening allocation** be the one implemented, so **tonight you will complete ${e://Field/Tasks21} tasks**, and Wednesday you will complete ${e://Field/Tasks22} tasks.

You may complete tonight's tasks below.

Task 1



Match 6	
Match 3	
Match 20	
Match 16	
Match 4	
Match 7	
Match 5	
Match 18	
Match 8	
Match 14	

Task 2

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20
Match 14																				
Match 4																				
Match 5																				
Match 8																				
Match 17																				
Match 7																				

II Online Experiment Instructions

This appendix contains the survey instrument for the online experiment. Because the survey presentation varied from subject to subject (due to them receiving a different treatment, being randomized into different incentives, having different contest outcomes, etc.), we print the survey instrument directly from Qualtrics. Thus, the surveys rely on Qualtrics' internal logic and referencing system. Subjects only see one version of any question, and any reference fields would be replaced with the information which was appropriate for that subject. We also remove pages that contain nothing but sliders in Surveys 2-5.

II.1 Survey 1

Basic Instructions

What is your Prolific ID? Please know that this response should auto-fill with the correct ID

Warning: If your Prolific ID is not entered correctly, you will not have access to the other parts of the study and you will not receive payment.

${e://Field/PROLIFIC_PID}

Thank you for agreeing to participate in the study. This study is about decision-making.

This is the first of **five parts** in this study. You will earn money based on the choices you make and the activities you complete throughout the study. Your submissions will be approved if you complete **all five parts of the study**.

The second, third, fourth, and fifth parts of this study will also be completed through Prolific. We expect that those who participate in the study will receive at least $10 per hour on average *for every part* completed. You will also earn *bonus payments* for completing

experimental tasks.

The link below will allow you to download an information sheet about the study. Please read the information sheet.

Participant Information Sheet

Please refer to the calendar below to see when each part of the study must be completed. The second part of the study must be completed on Monday or Tuesday of next week. The third part of the study must be completed on Thursday or Friday of next week. The fourth part must be completed on Monday or Tuesday of the week after next. The fifth and final part must be completed on Thursday or Friday of the week after next. Subject to completion, **all submissions and bonuses will be approved on Saturday the week after next.**

Once you understand the conditions of participation, please respond to the consent form on the next page.

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
This Week					Part 1: Introduction		
Next Week		Part 2: 1.Complete IQ quiz 2.Choose tasks for Part 3			Part 3: 1. Choose tasks for Part 3 2. Complete Part 3 tasks		
Week After Next		Part 4: 1.Complete IQ quiz 2.Choose tasks for Part 5			Part 5: 1. Choose tasks for Part 5 2. Complete Part 5 tasks		Submissions approved and payments sent

Current Part
Future Parts

Consent

Consent Statement:

I consent to participate in this research project. It has been explained to me that the purpose of this research is to investigate decision-making. I have also been provided with a written project information sheet in a language that I can understand.

The possible risks of participating in this research have been explained to my satisfaction. I understand that in this research I will be required to complete five separate surveys, including this one, over the next two weeks.

I understand that my participation is voluntary and I am free to withdraw from this research anytime without needing to provide any explanation, and I would not receive any penalty or bias as a result of my withdrawal. Should I decide to withdraw, I understand that my data will be destroyed and will not be used in the research.

I understand that data collected for this research will be stored in a secure online database, and only the survey company and the researchers listed on the Participant Information Sheet will have access to the data. I consent for my data to be used in future research that is an extension of or related to this project.

I understand that this research adheres to the Guidelines of the ethical review process of The University of Queensland and the

National Statement on Ethical Conduct in Human Research. I have been provided with contact details of the researcher, as well as UQ Ethics Coordinator.

- ◯ I agree with the above statements and consent to participate in this research.
- ◯ I do not consent to participate in this research.

Demographic Questions

What is the highest level of school you have completed or the highest degree you have received?

- ◯ Less than high school degree
- ◯ High school graduate (high school diploma or equivalent including GED)
- ◯ Some college but no degree
- ◯ Associate degree in college (2-year)
- ◯ Bachelor's degree in college (4-year)
- ◯ Master's degree
- ◯ Doctoral degree
- ◯ Professional degree (JD, MD)

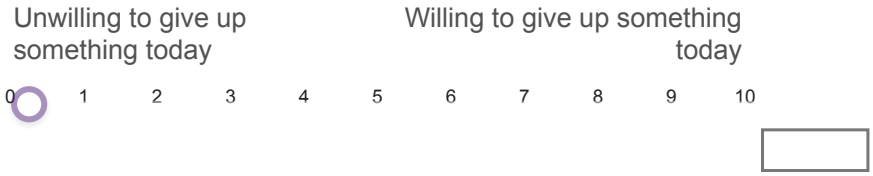
Are you employed as a manager?

- ◯ I am employed as a manager.
- ◯ I am employed, but not as a manager.
- ◯ I am not employed.

In general, how willing or unwilling you are to take risks. Please use a scale from 0 to 10, where 0 means “completely unwilling to take

risks” and a 10 means you are “very willing to take risks”.

How willing are you to give up something that is beneficial for you today in order to benefit more from that in the future? Please use a scale from 0 to 10, where 0 means “completely unwilling to give up something today” and a 10 means you are “very willing give up something today”.



Block 3

Earning Money

During the study, you will be paid in two ways.

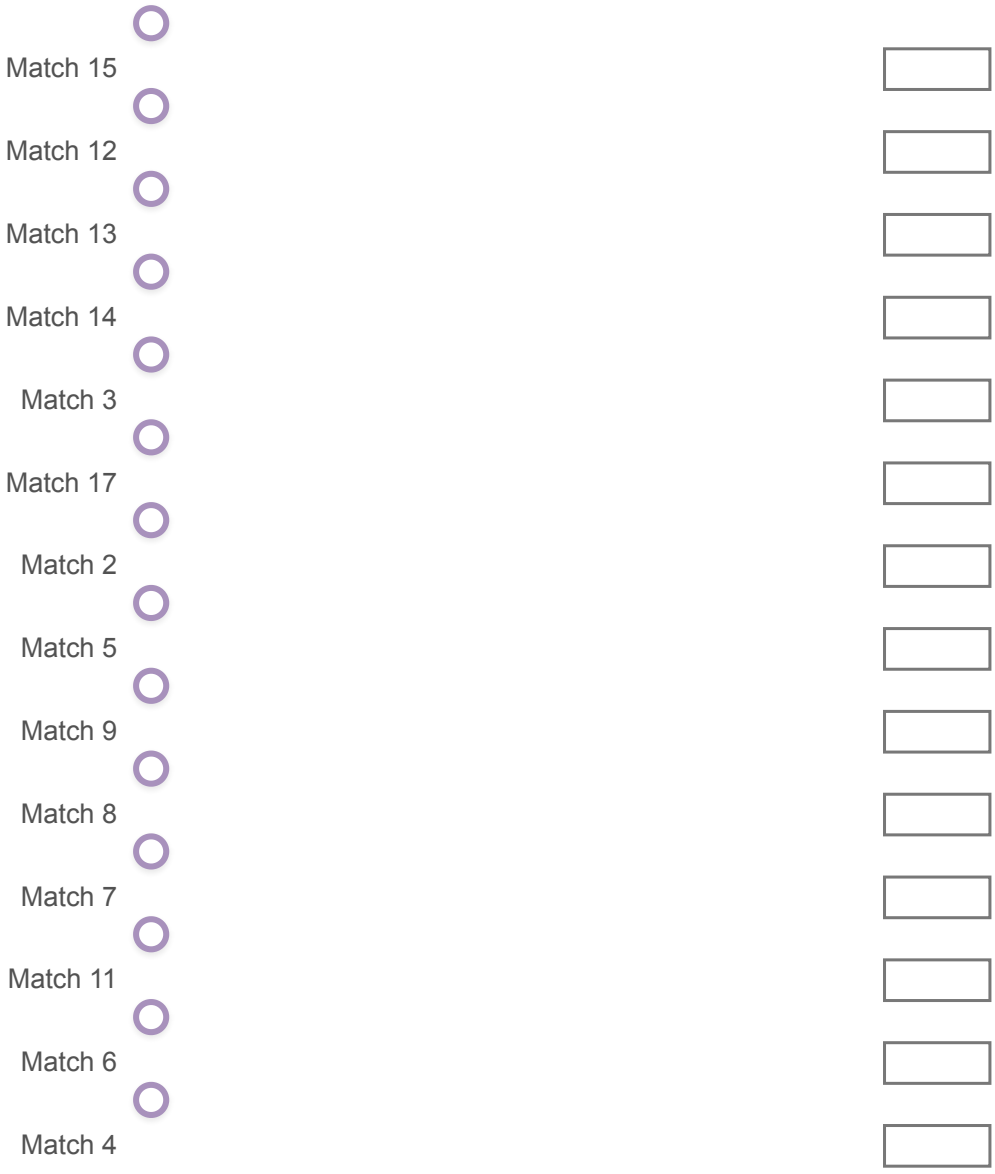
First, you will receive the normal Prolific payment for completing all of the five study surveys. You are completing the first survey now! Participants will receive $10/hour on average for completing the surveys.

Second, you will receive *bonus payments* for completing tasks. Each task involves moving a number of sliders to a predetermined point. There will be two kinds of tasks: each “easy” task will involve moving a set of 20 sliders to predetermined points, while each “hard” task will involve moving a set of 30 sliders to predetermined points. The bonus will be paid for each set of sliders (20 for easy sets, 30 for hard sets) that you do, and you will make choices about how many tasks to complete at a variety of different wage rates.

Please complete the following 4 sets of sliders to familiarize yourself with the process. You will first complete 2 “easy” sets of sliders and then 2 “hard” sets of sliders.

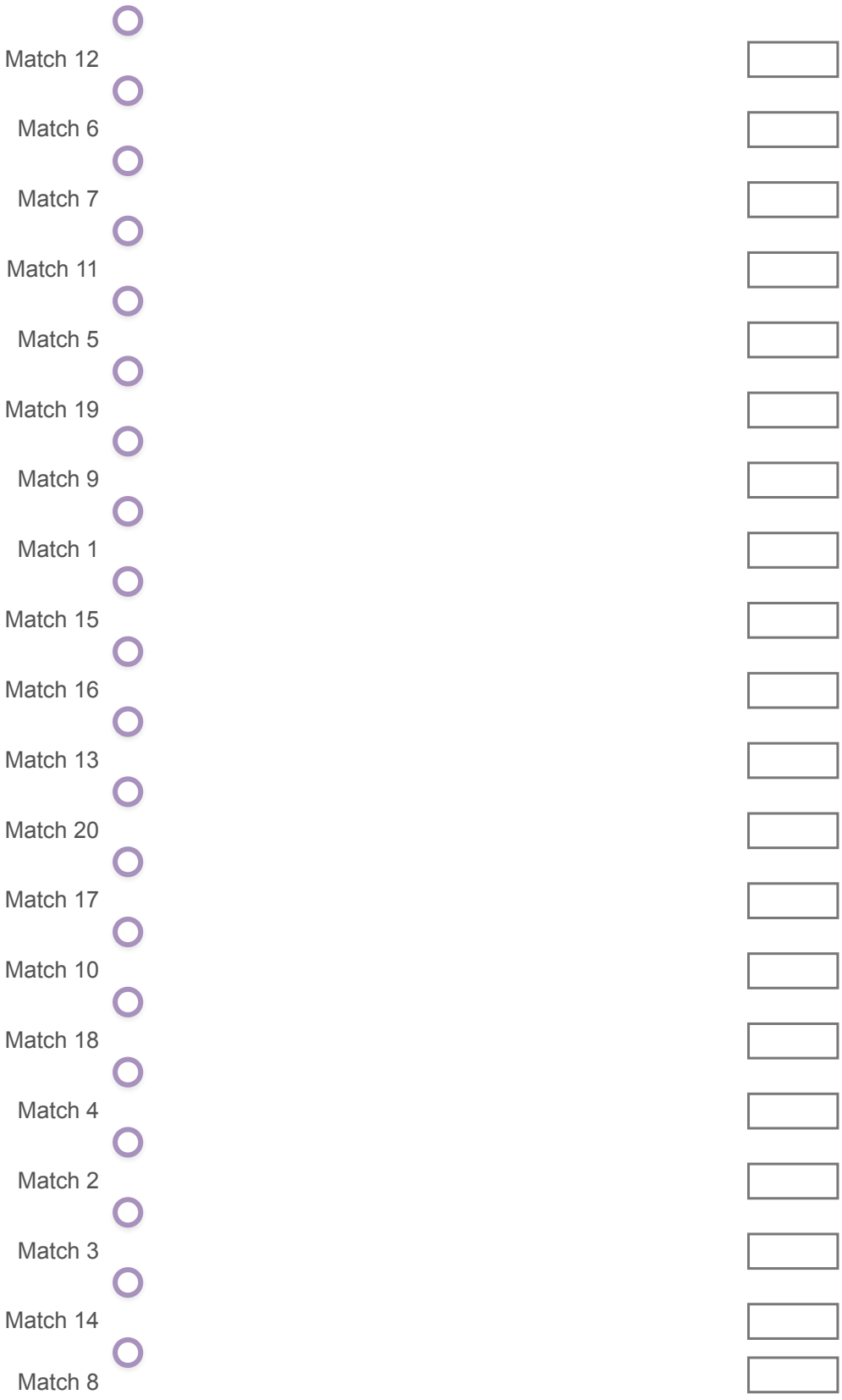
Easy Task Block

Easy set 1: Match each of the 20 sliders to the amount written on the left. You will not be able to continue until all sliders are matched correctly.



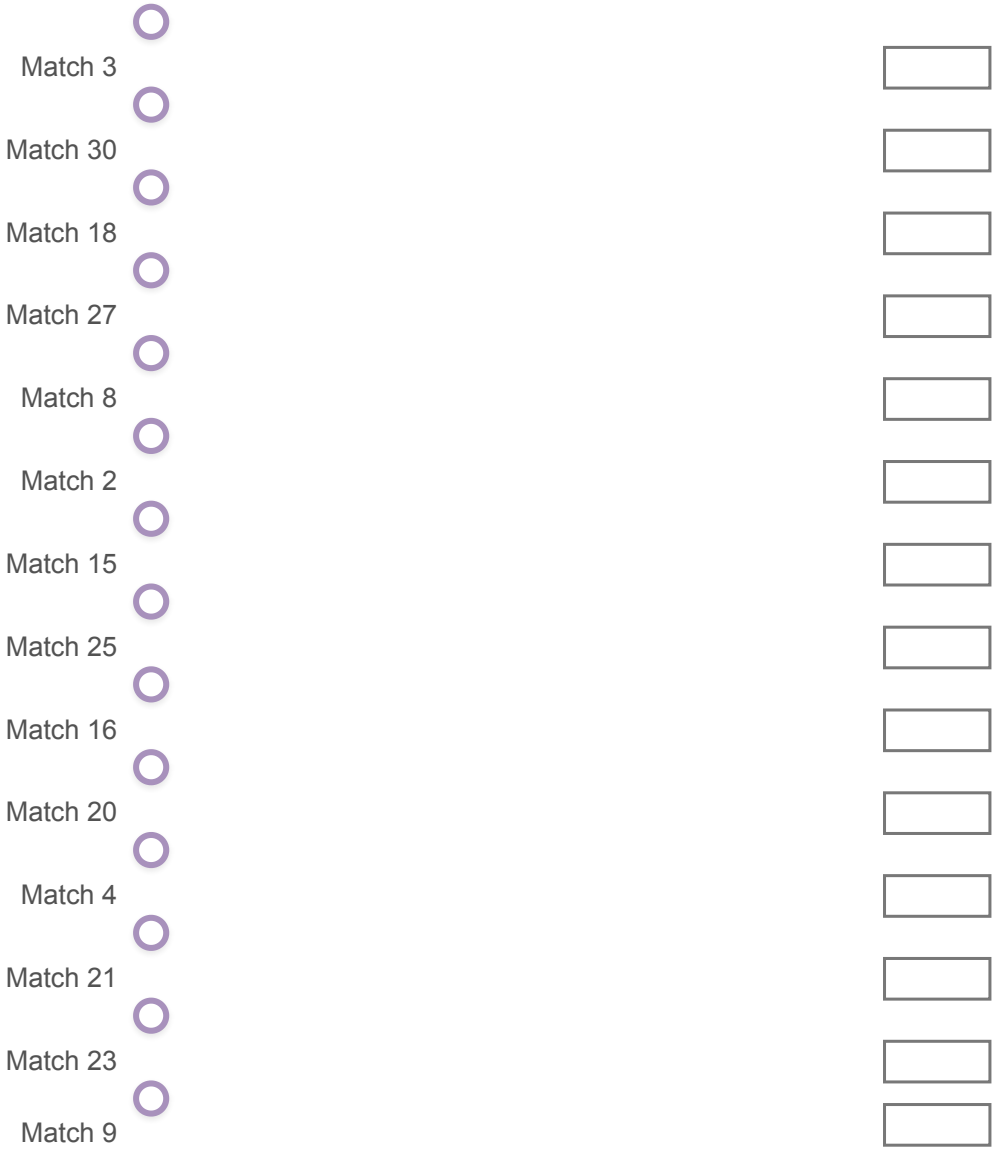
Block 5

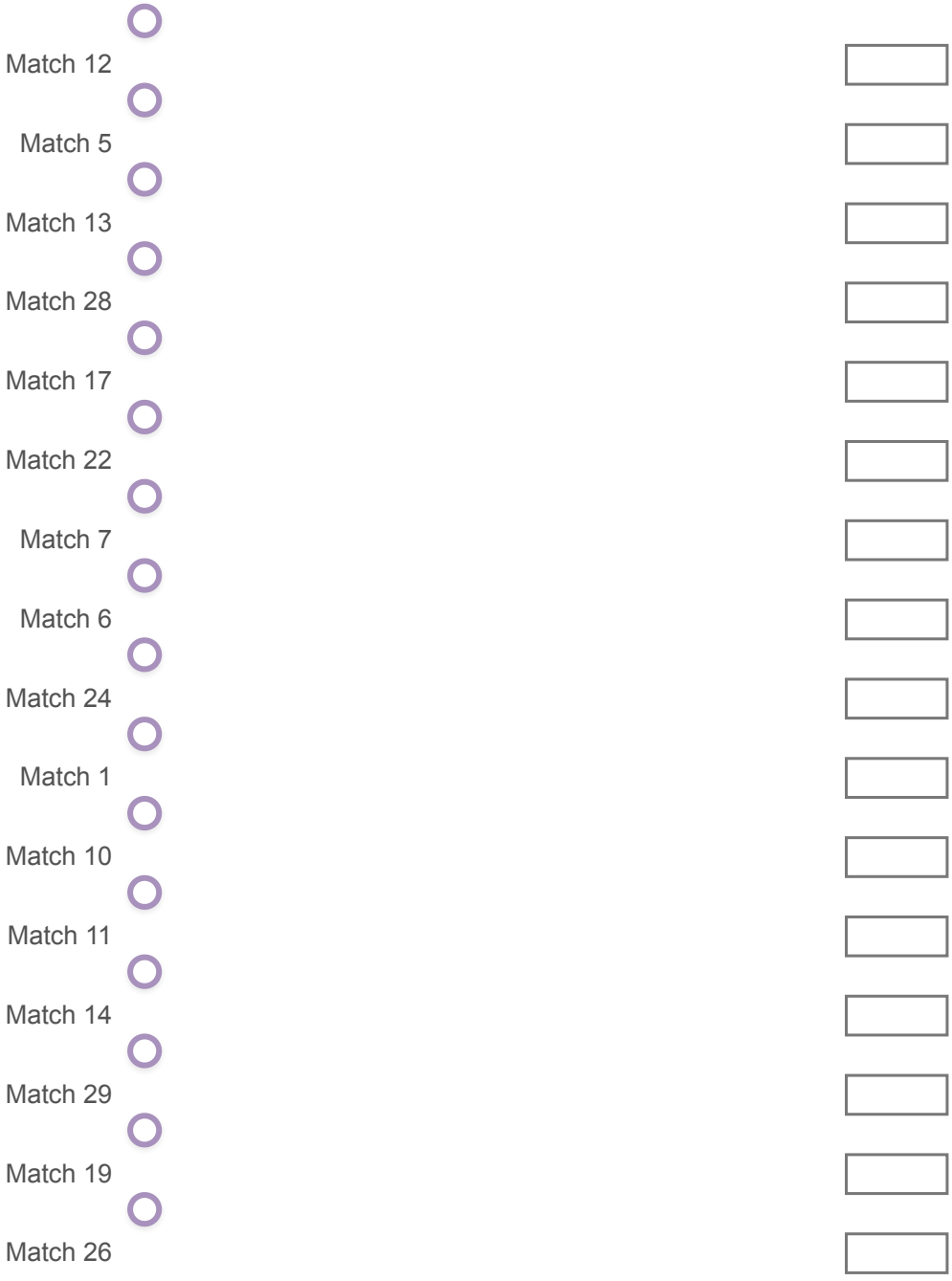
Easy set 2: Match each of the 20 sliders to the amount written on the left. You will not be able to continue until all sliders are matched correctly.


Match 12
Match 6
Match 7
Match 11
Match 5
Match 19
Match 9
Match 1
Match 15
Match 16
Match 13
Match 20
Match 17
Match 10
Match 18
Match 4
Match 2
Match 3
Match 14
Match 8

Hard Task Block

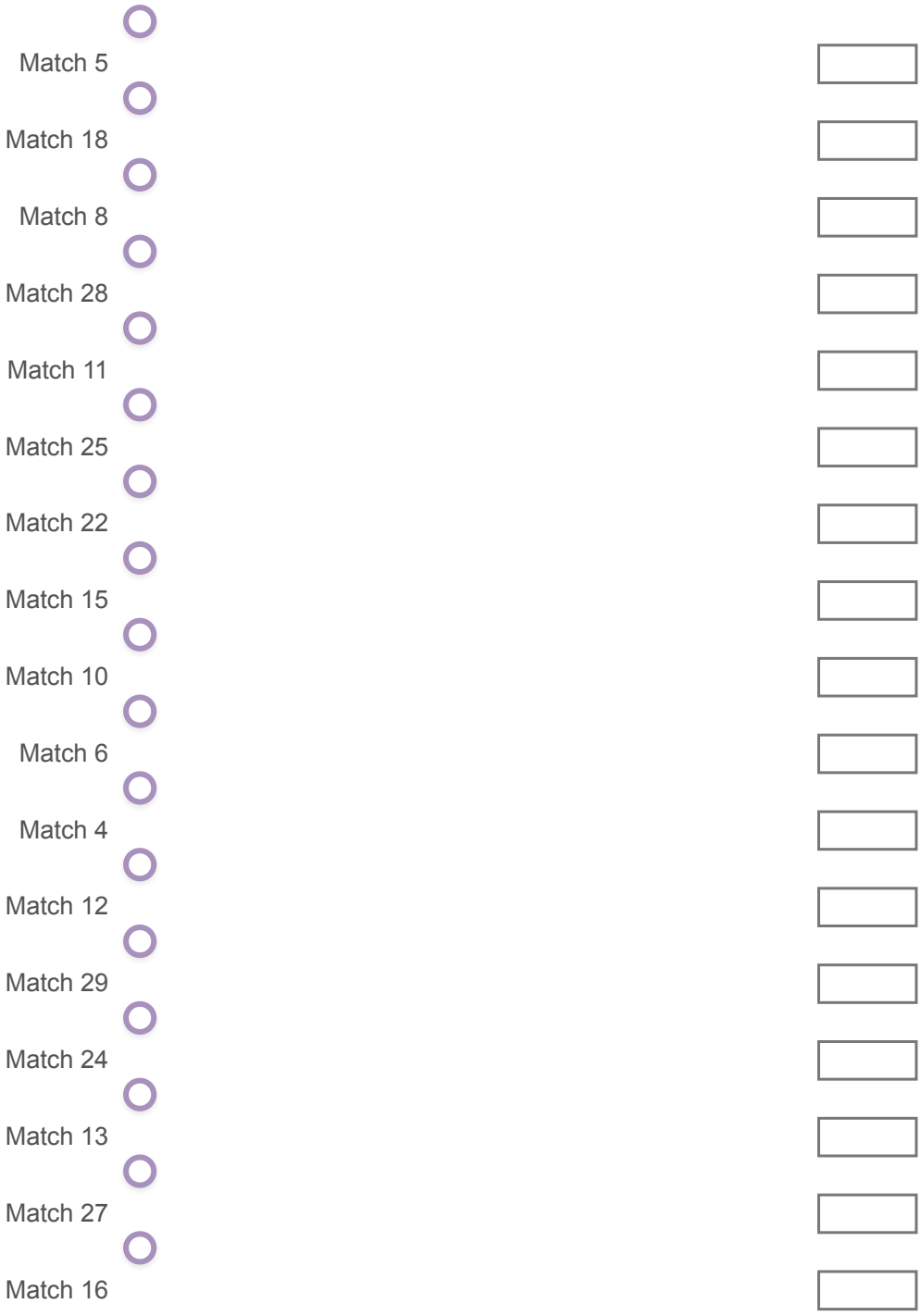
Hard set 1: Match each of the 30 sliders to the amount written on the left. You will not be able to continue until all sliders are matched correctly.

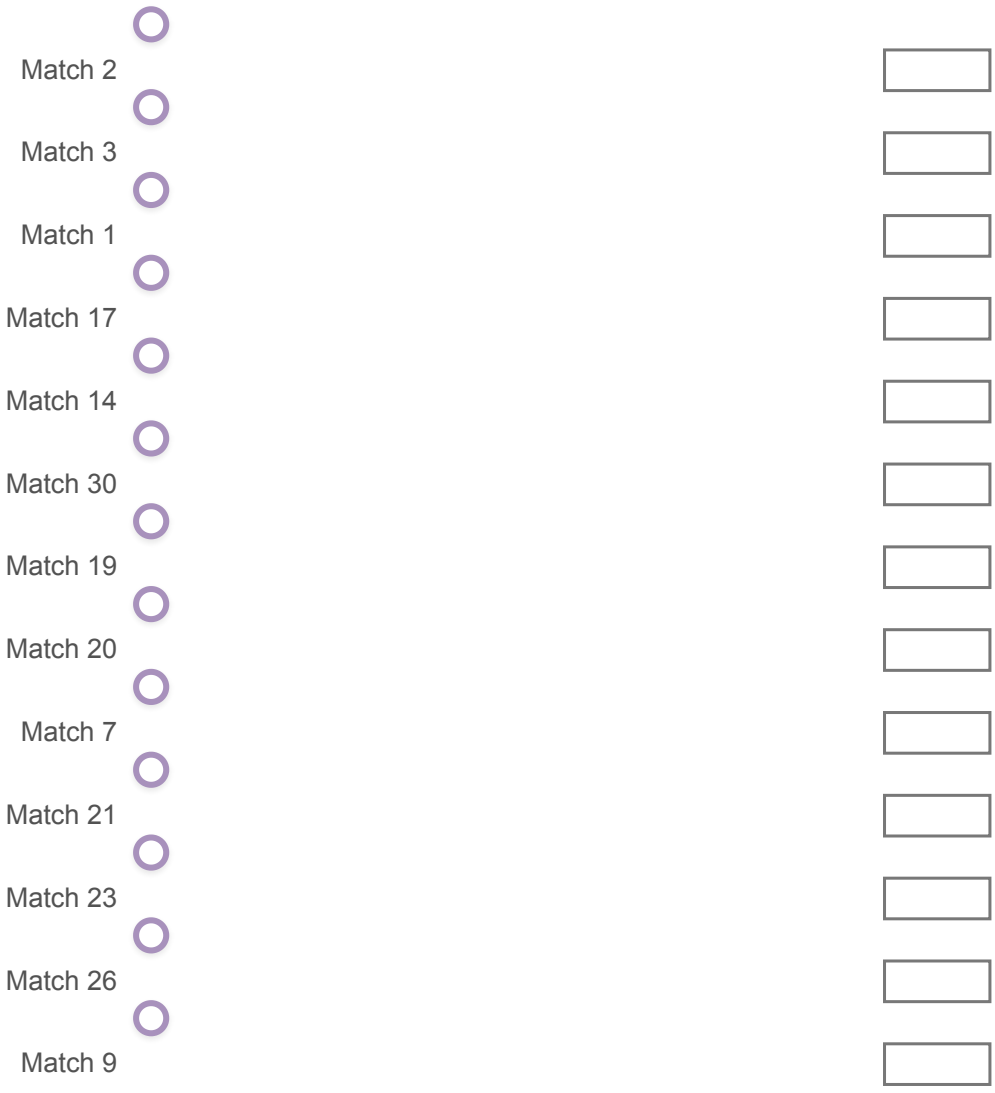



Match 12
Match 5
Match 13
Match 28
Match 17
Match 22
Match 7
Match 6
Match 24
Match 1
Match 10
Match 11
Match 14
Match 29
Match 19
Match 26

Block 7

Hard set 2: Match each of the 30 sliders to the amount written on the left. You will not be able to continue until all sliders are matched correctly.





Allocation & Commitment Explanation

Earning Bonus Payments

You will complete sets of sliders for bonus payments in the third and fifth parts of this study. We will ask you how many sets of sliders you would like to complete for three different payment rates ($0.06 per

set, $0.12 per set, or $0.18 per set of sliders). An example of what this choice will look like can be found below.

EXAMPLE SLIDER CHOICE QUESTION (your choice here will not count):

How many **sets** of sliders would you like to complete if **the sets are hard**? Please choose a number between 0 and 19 for each payment rate. Remember: you are paid per **set** of sliders, not per slider!

$0.06/set of sliders

$0.12/set of sliders

$0.18/set of sliders

As an example, if you choose to do 5 sets of sliders at a payment rate of $0.12 per set, then you would receive a bonus payment of $0.60.

You will be asked **two times** to say how many sliders you would like to complete. In both the second and third parts of the study, we will ask you how many sets of sliders you would like to complete in the

third part. In other words, in the second part of the study, you will make an initial decision. Then in the third part, you will have a chance to make a different decision, if you wish, before actually completing the sets of sliders.

Similarly, in both the fourth and fifth parts of the study, we will also ask you how many sets of sliders you would like to complete in the fifth part.

Because we will ask you twice about how many sets of sliders you would like to complete for payment, we will randomly select which choice will count. For instance, the number of sets of sliders you complete in the third part could be determined by your choice in the second part with 1 out of 5 chance and your choice in the third part with 4 out of 5 chance.

In addition, we will allow you to choose the likelihood that each choice is the one that counts (potentially) at the cost of doing a few more easy sets of sliders. For instance, we will ask you questions like “would you rather complete 1 easy set of sliders now with 4 out of 5 chance of today’s choices being the ones that count or would you like to complete 3 easy sets of sliders now and have 1 out of 5 chance of today’s choices being the ones that count.”

Let's review these instructions on the next page.

Rules Quiz

Please answer the following questions to make sure that you understand the process.

You can review the instructions that you received earlier at this link: Instructions from Survey 1

How many parts are there in the study, counting this one?

- ◯ 2
- ◯ 3
- ◯ 4
- ◯ 5

Recall that some sets of sliders will be “easy” and some will be “hard”. How many individual sliders are there in an “easy” set? Your answer should be a number between 1 and 100.

In which parts of the study will we ask you how many sets of sliders you want to complete in the third part of the study? Check all boxes that apply.

- ☐ Part 1
- ☐ Part 2
- ☐ Part 3
- ☐ Part 4

☐ Part 5

Imagine that in Part 2 you made the choices that you see in the first picture, while in Part 3 you made the choices in the second picture. What is the **minimum** number of sets of sliders that it is possible you will complete in Part 3? Your answer should be a number between 1 and 100.

Part 2 choices:

$0.06/set of sliders	13
$0.12/set of sliders	10
$0.18/set of sliders	15

Part 3 choices:

$0.06/set of sliders	5
$0.12/set of sliders	12
$0.18/set of sliders	20

Input your answer here:

Imagine that in Part 4 you make the choice that you see below and it ends up being a choice that counts. What is the likelihood that the number of tasks you choose to complete in Part 5 ends up being the one that counts?

- ◯ 1 out of 5
- ◯ 2 out of 5
- ◯ 3 out of 5
- ◯ 4 out of 5

Calendar Reminder

Thank you for completing this part of the study. Remember that there are 4 more parts of the study, and you must complete them all for your submissions to be approved. The next part of the study will appear on your Prolific dashboard on Monday and you will be required to complete that part by Tuesday night. The calendar below shows the schedule for all parts of the study.

Please don't forget to click the arrow at the bottom to finalize this part of the survey. Not doing this could delay your payment or even result in being dropped from the experiment.

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
This Week					Part 1: Introduction		
Next Week		Part 2: 1.Complete IQ quiz 2.Choose tasks for Part 3			Part 3: 1. Choose tasks for Part 3 2. Complete Part 3 tasks		
Week After Next		Part 4: 1.Complete IQ quiz 2.Choose tasks for Part 5			Part 5: 1. Choose tasks for Part 5 2. Complete Part 5 tasks		Submissions approved and payments sent

Current Part
Future Parts

II.2 Survey 2

Quiz 1 Block

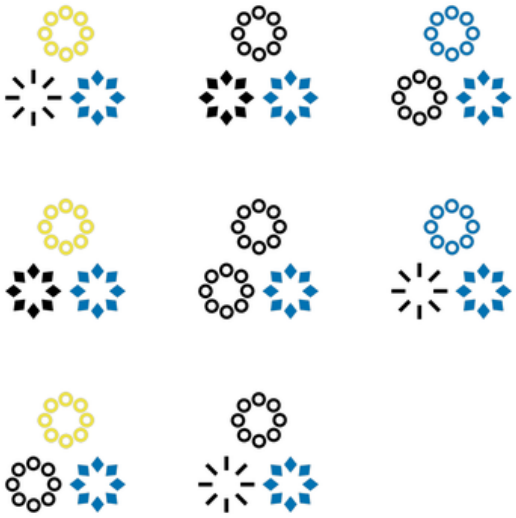
Thank you for logging in to complete the second part of the study!

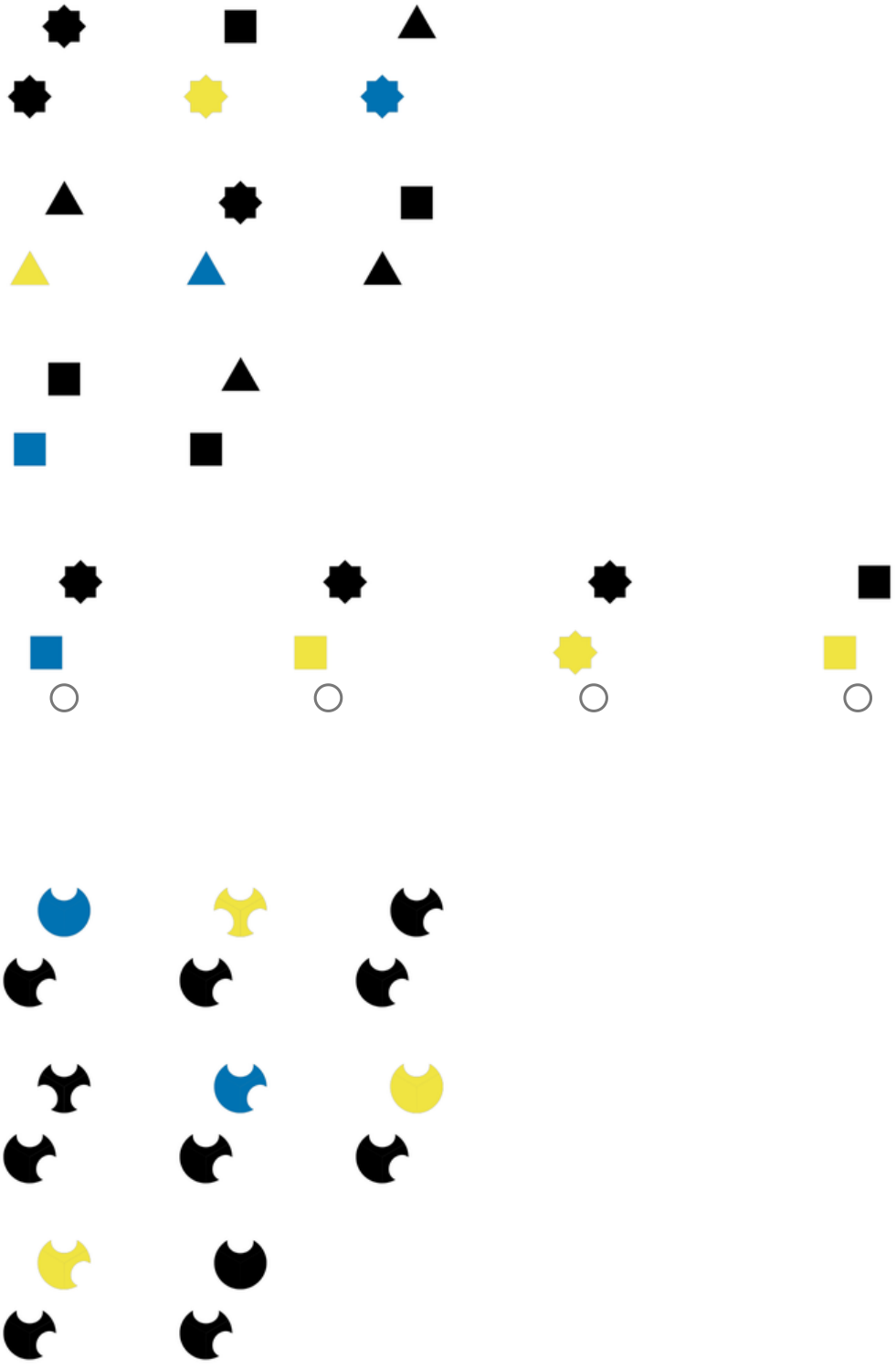
Before continuing with the study, you will compete in a contest against one other player that you are randomly matched with. The outcome of this contest will determine the difficulty of the sets of sliders that you will be asked to complete in the next part of the study. If you win the contest, you will need to complete “easy” sets that involve 20 sliders each. If you lose the contest, you will need to complete “hard” sets that will involve 30 sliders each. The payment that you receive for completing each set of sliders will be the same whether they are easy or hard.

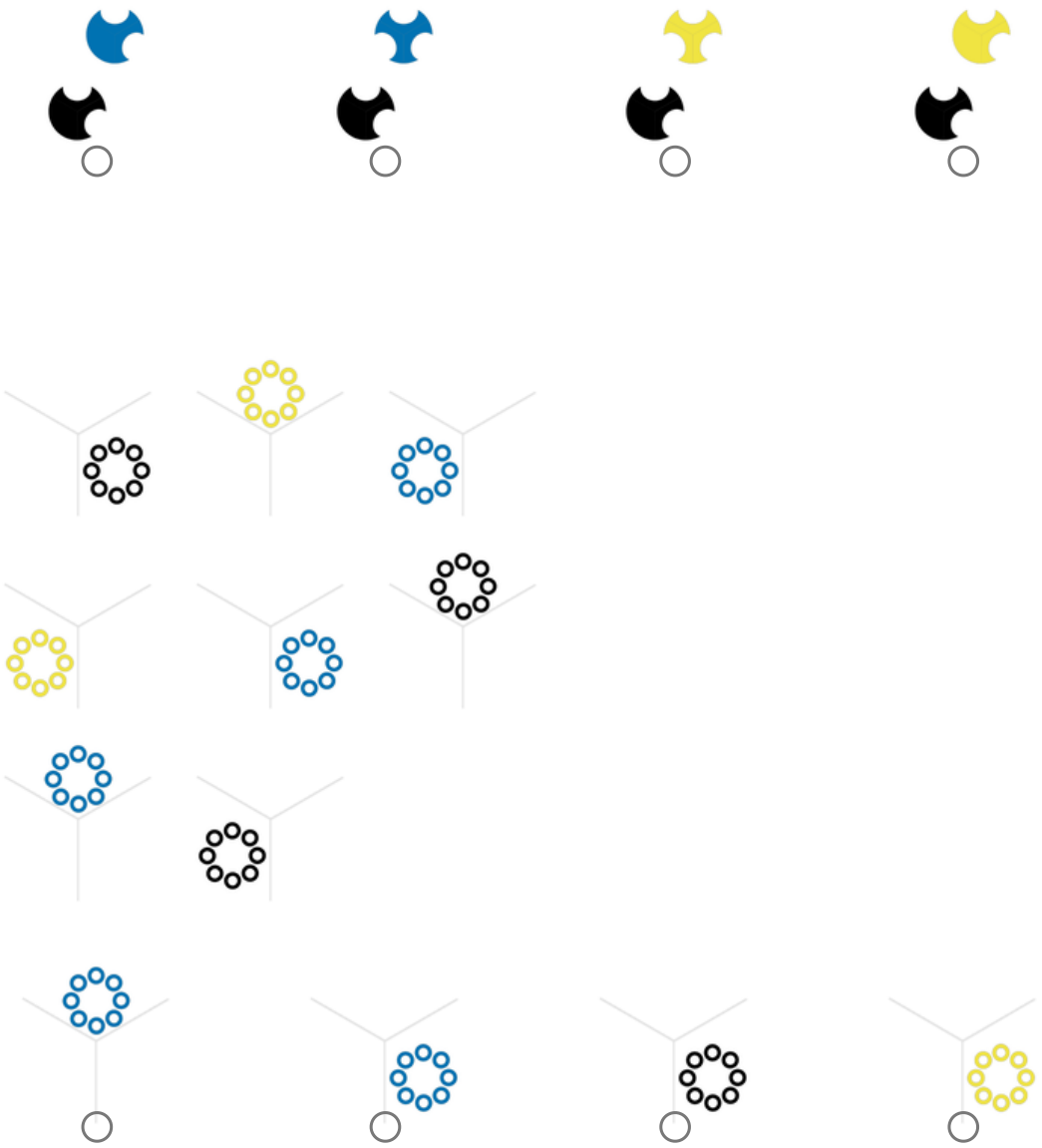
The contest will involve an IQ quiz with 10 questions. In each question you will see eight images with a missing slot for a ninth. You will choose the best fit from among the four options that you are given.

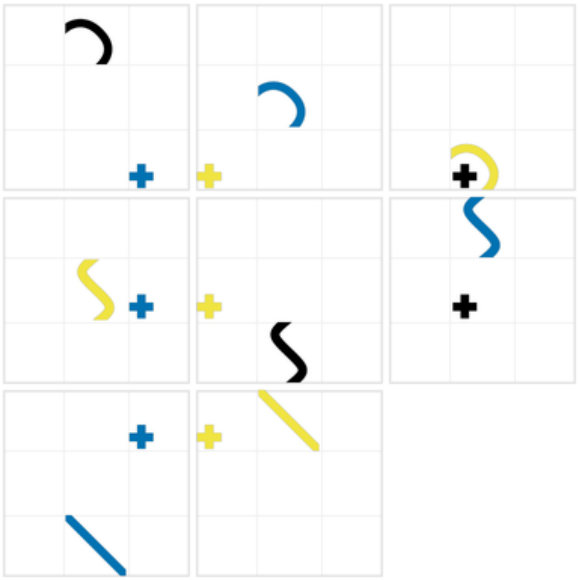
We will count the number of correct answers you and your opponent give. You will win the contest if you have a higher score than the person you are matched with. If you have the same score as the person you are matched with, the winner will be chosen randomly.

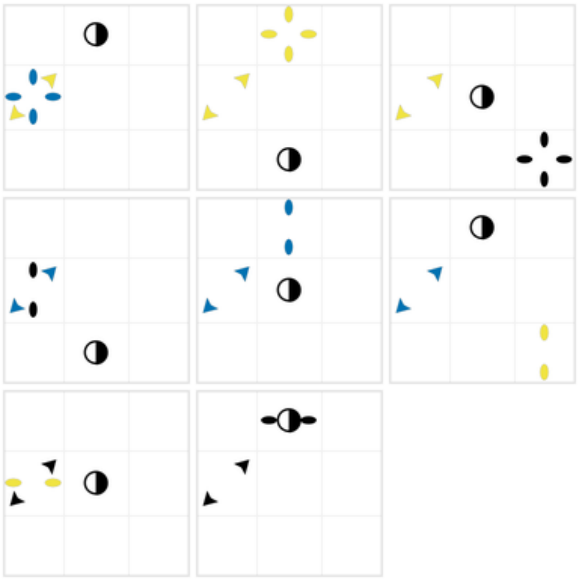
Please choose the best fit among the four options below for each question:

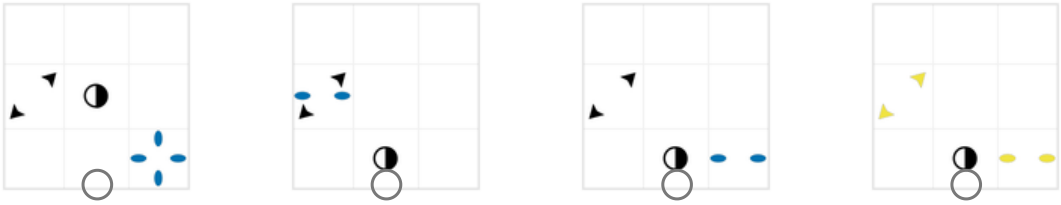


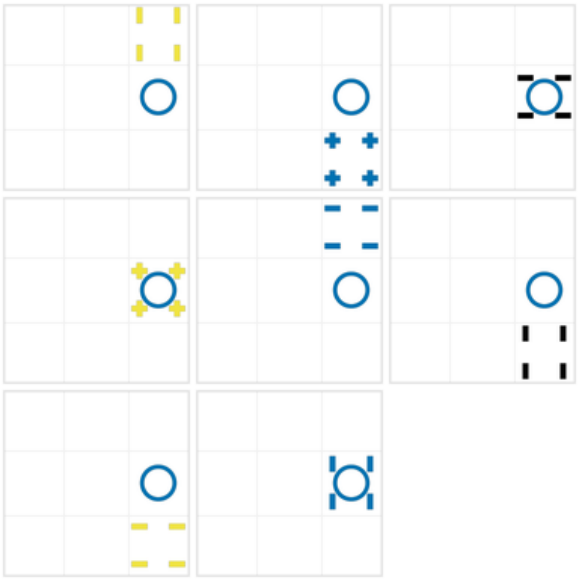


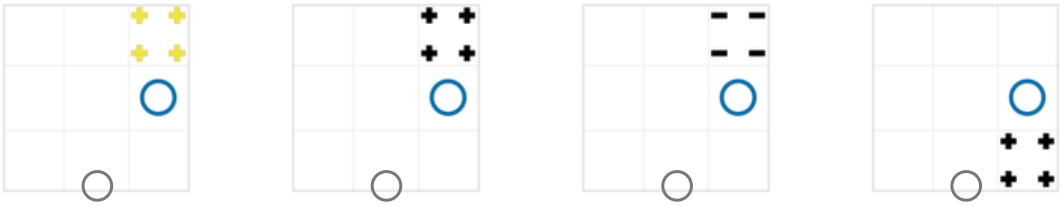


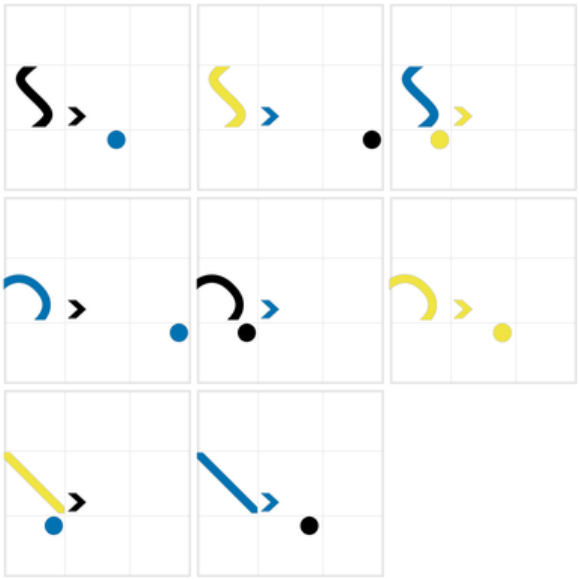


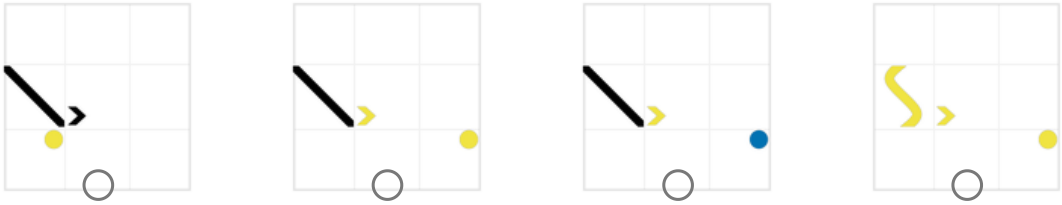


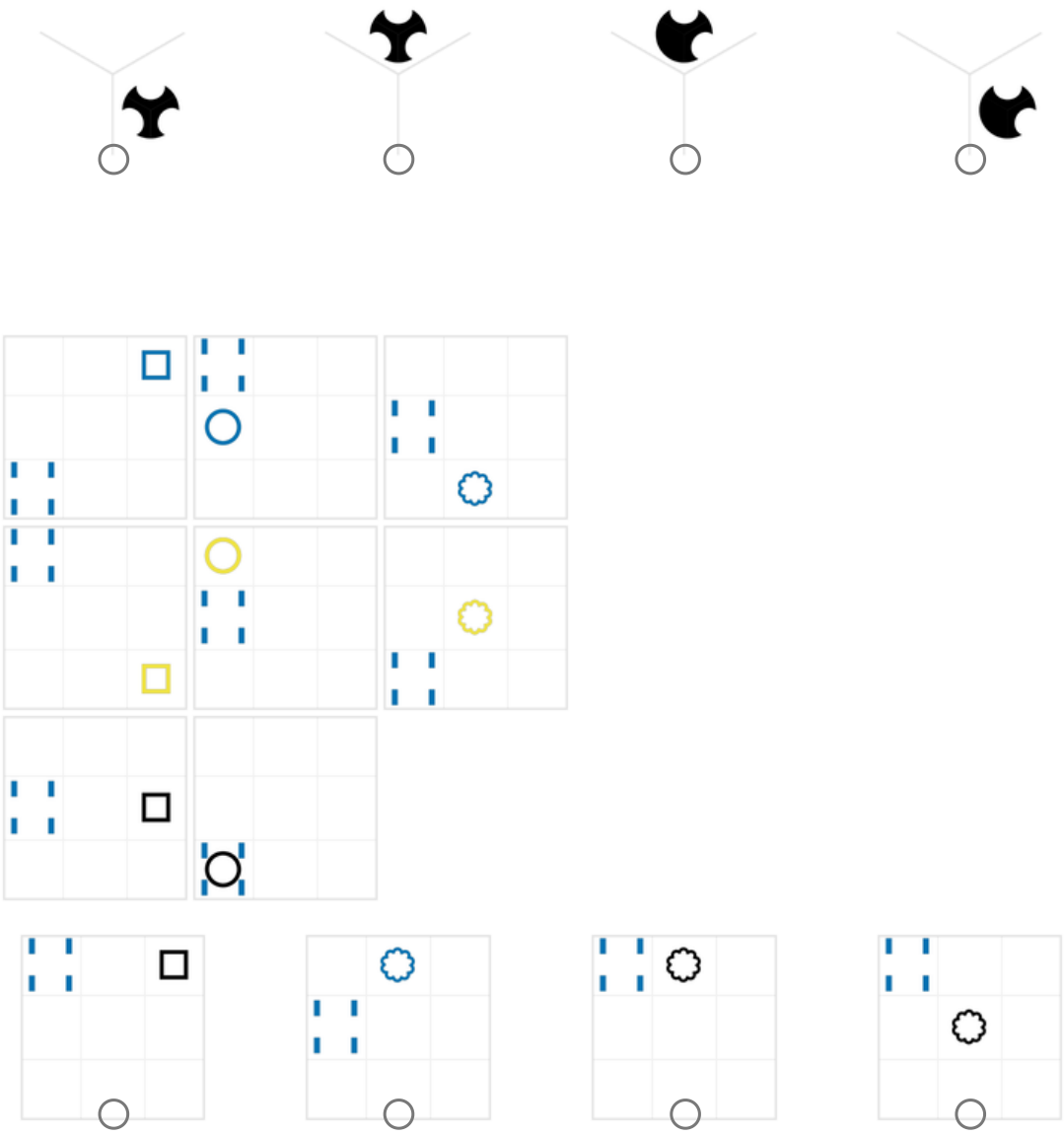












Quiz 1 Beliefs

As a reminder, you will win the contest if you have a higher score than the person you are matched with. If you have the same score as the person you are matched with, the winner will be chosen randomly.

What do you think are the chances, out of 100, that you will win the contest? You can write down any number from 0 to 100 out of 100.

Commitment Choice

You will have the opportunity to complete sets of sliders for payment in the third part of the study. We will ask you how many sets you want to complete for various payment rates both in the second part of the study (this part) and in the third part of the study. However, only one of these choices will be the one that counts.

Now, you will make a series of choices that allow you to affect the chance that your choices in this part of the study are the ones that count. These choices will involve extra sets of easy sliders. By choosing more or fewer easy sliders, you will change the probability that your choices in this part of the study are the ones that count.

This decision will measure the strength of your preference for which choices count.

We will randomly select which one of these choices will be the one that counts. When you make these decisions, treat every decision as if it is the one that counts because each decision could count.

Choose either the left or right option from each pair of options below.

Make this survey's choices more likely:

1 easy set of sliders today, 4 out of 5 chance of today's choices being the ones that count

Make the next survey's choices more likely:

3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Make this survey's choices more likely:

2 easy sets of sliders today, 4 out of 5 chance of today's choices being the ones that count

Make the next survey's choices more likely:

3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Make this survey's choices more likely:

3 easy sets of sliders today, 4 out of 5 chance of today's choices being the ones that count

Make the next survey's choices more likely:

3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Make this survey's choices more likely:

4 easy sets of sliders today, 4 out of 5 chance of today's choices being the ones that count

Make the next survey's choices more likely:

3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Make this survey's choices more likely:	**Make the next survey's choices more likely**:
5 easy set of sliders today, 4 out of 5 chance of today's choices being the ones that count	3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Task Allocation

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line, you chose "${q://QID28/ChoiceGroup/SelectedChoices}."

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line, you chose "${q://QID29/ChoiceGroup/SelectedChoices}."

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line, you chose "${q://QID30/ChoiceGroup/SelectedChoices}."

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line, you chose "${q://QID31/ChoiceGroup/SelectedChoices}."

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line, you chose "${q://QID32/ChoiceGroup/SelectedChoices}."

In the third part of the study, you will be asked to complete a number of slider tasks. Each set of sliders will contain either 20 sliders (if you win the contest) or 30 sliders (if you lose the contest). You will be paid for each set of sliders that you complete.

We would like to know how many sets of sliders you would like to complete in the third part of the study for different potential payment rates. Furthermore, you must choose how many sets of sliders you would like to complete if the sets of sliders are easy (20 sliders each), hard (30 sliders each), or without knowing whether they are easy or hard.

One choice will be randomly selected to be the one that counts. Recall that there is a 1 out of 5 chance of today’s choices being the ones that count and 4 out of 5 chance of the next survey's choices being the ones that count. Because any choice can be selected, **it is**

in your interest to make every choice as if it will be the one that counts.

In the third part of the study, you will be asked to complete a number of slider tasks. Each set of sliders will contain either 20 sliders (if you win the contest) or 30 sliders (if you lose the contest). You will be paid for each set of sliders that you complete.

We would like to know how many sets of sliders you would like to complete in the third part of the study for different potential payment rates. Furthermore, you must choose how many sets of sliders you would like to complete if the sets of sliders are easy (20 sliders each), hard (30 sliders each), or without knowing whether they are easy or hard.

One choice will be randomly selected to be the one that counts. Recall that there is a 4 out of 5 chance of today's choices being the ones that count and 1 out of 5 chance of the next survey's choices being the ones that count. Because any choice can be selected, **it is in your interest to make every choice as if it will be the one that counts.**

How many sets of sliders would you like to complete if you **win** the contest (so **the sets are easy**)? Please choose a number between 0 and 19 for each payment rate. Because this choice will only count if

you win the contest, the choice that is best for you **should not** depend on whether you think you will win.

$0.06/set of sliders	
$0.12/set of sliders	
$0.18/set of sliders	

How many sets of sliders would you like to complete if you **lose** the contest (so **the sets are hard**)? Please choose a number between 0 and 19 for each payment rate. Because this choice will only count if you lose the contest, the choice that is best for you **should not** depend on whether you think you will win.

$0.06/set of sliders	
$0.12/set of sliders	
$0.18/set of sliders	

How many sets of sliders would you like to complete **without knowing** whether you won the contest (so **the sets may be easy or hard**)? Please choose a number between 0 and 19 for each

payment rate. The choice that is best for you **should** depend on whether you think you will win the contest.

$0.06/set of sliders	
$0.12/set of sliders	
$0.18/set of sliders	

Commitment Payment Tasks

Recall that you chose "${q://QID28/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_1} set(s) of sliders now.

Recall that you chose "${q://QID29/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_1} set(s) of sliders now.

Recall that you chose "${q://QID30/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_1} set(s) of sliders now.

Recall that you chose "${q://QID31/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_1} set(s) of sliders now.

Recall that you chose "${q://QID32/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_1} set(s) of sliders now.

Easy Block 1

Set 1 of ${e://Field/Task_Price_Commitment_1}: match each of the 20 sliders to the amount written on the left. You will not be able to continue until all sliders are matched correctly.

Goodbye

Thank you for completing this part of the study. Remember that there are 3 more parts of the study, and you must complete them all for your submissions to be approved. The next part of the study will appear on your Prolific dashboard on Thursday and you will be required to complete that part by Friday night.

Please don't forget to click the arrow at the bottom to finalize this part of the survey. Not doing this could delay your payment or even result in being dropped from the experiment.

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
This Week					Part 1: Introduction		
Next Week		Part 2: 1.Complete IQ quiz 2.Choose tasks for Part 3			Part 3: 1. Choose tasks for Part 3 2. Complete Part 3 tasks		
Week After Next		Part 4: 1.Complete IQ quiz 2.Choose tasks for Part 5			Part 5: 1. Choose tasks for Part 5 2. Complete Part 5 tasks		Submissions approved and payments sent

Completed Parts
Current Part
Future Parts

II.3 Survey 3

Task Allocation

Thank you for logging in to complete the third part of the study!

In this part of the study, you will be asked to complete a number of slider tasks. Each slider task will contain either 20 sliders (if you win the contest) or 30 sliders (if you lose the contest). You will be paid for each set of sliders that you complete.

We would like to know how many sets of sliders you would like to complete in this part of the study for different potential payment rates. You must choose how many sets of sliders you would like to complete without knowing whether they are easy or hard. We will then tell you whether you won the contest and again ask you how many sets of sliders you would like to complete.

One choice will be randomly selected to be the one that counts. Recall that there is a 1 out of 5 chance of the last survey's choices being the ones that count and 4 out of 5 chance of today's choices being the ones that count. Because any choice can be selected, **it is in your interest to make every choice as if it will be the one that counts**.

In this part of the study, you will be asked to complete a number of slider tasks. Each slider task will contain either 20 sliders (if you win the contest) or 30 sliders (if you lose the contest). You will be paid for each set of sliders that you complete.

We would like to know how many sets of sliders you would like to complete in this part of the study for different potential payment rates. You must choose how many sets of sliders you would like to complete without knowing whether they are easy or hard. We will then tell you whether you won the contest and again ask you how many sets of sliders you would like to complete.

One choice will be randomly selected to be the one that counts. Recall that there is a 4 out of 5 chance of the last survey's choices being the ones that count and 1 out of 5 chance of today's choices being the ones that count. Because any choice can be selected, **it is in your interest to make every choice as if it will be the one that counts.**

How many sets of sliders would you like to complete **without knowing** whether you won the contest (so **the sets may be easy or hard**)? Please choose a number between 0 and 19 for each payment rate. The choice that is best for you **should** depend on whether you think you will win the contest.

$0.06/set of sliders

$0.12/set of sliders

$0.18/set of sliders

Contest Outcome

We can now reveal that **you won the contest**.

We can now reveal that **you lost the contest**.

Now that you know that you have won the contest (so **the sets are easy**), how many sets of sliders would you like to complete? Please choose a number between 0 and 19 for each payment rate.

$0.06/set of sliders

$0.12/set of sliders

$0.18/set of sliders

Now that you know that you have lost the contest (so **the sets are hard**), how many sets of sliders would you like to complete? Please choose a number between 0 and 19 for each payment rate.

$0.06/set of sliders

$0.12/set of sliders

$0.18/set of sliders

Allocation Information

We randomly selected among the potential payment rates, and the rate that will count for you will be $0.06/set of sliders.

We randomly selected among the potential payment rates, and the rate that will count for you will be $0.12/set of sliders.

We randomly selected among the potential payment rates, and the rate that will count for you will be $0.18/set of sliders.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_1_10} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Easy_1_10} sets of sliders if you won the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_1_10} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Easy_1_10} sets of sliders knowing you won the contest.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_1_20} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Easy_1_20} sets of sliders if you won the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_1_20} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Easy_1_20} sets of sliders knowing you won the contest.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_1_30} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Easy_1_30} sets of sliders if you won the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_1_30} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Easy_1_30} sets of sliders knowing you won the contest.

In the second part of the study, you indicated that you would do ${e://Field/All_Com_Unc_1_10} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Hard_1_10} sets of sliders if you lost the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_1_10} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Hard_1_10} sets of sliders knowing you lost the contest.

In the second part of the study, you indicated that you would do ${e://Field/All_Com_Unc_1_20} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Hard_1_20} sets of sliders if you lost the contest. Earlier in this part of the study, you indicated that you would

do ${e://Field/All_Uncom_Unc_1_20} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Hard_1_20} sets of sliders knowing you lost the contest.

In the second part of the study, you indicated that you would do ${e://Field/All_Com_Unc_1_30} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Hard_1_30} sets of sliders if you lost the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_1_30} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Hard_1_30} sets of sliders knowing you lost the contest.

We have randomly selected among these four options and today you will complete ${e://Field/All_Implemented_1} sets of sliders.

Prolific Completion Code

Before completing the sets of sliders that you agreed to, please go to Prolific and type in the completion code CG0QV8IB. You must type in this code before starting with the sliders so that you do not

time out of the study. **It is important that you enter the code now because you will likely time out of the Prolific study if you do not.**

Please remember that you **must** complete the sets of sliders that you agreed to today in order to continue with the study and have your submissions approved.

If you fail to enter the Prolific code now or you do not return and complete the sliders, you will not receive payment for any of the surveys you have completed as part of this study.

Please go to Prolific and type in the completion code CG0QV8IB. After typing in the code, please return to this survey and complete it.

You **must** complete the survey in order to continue with the study and have your submissions approved.

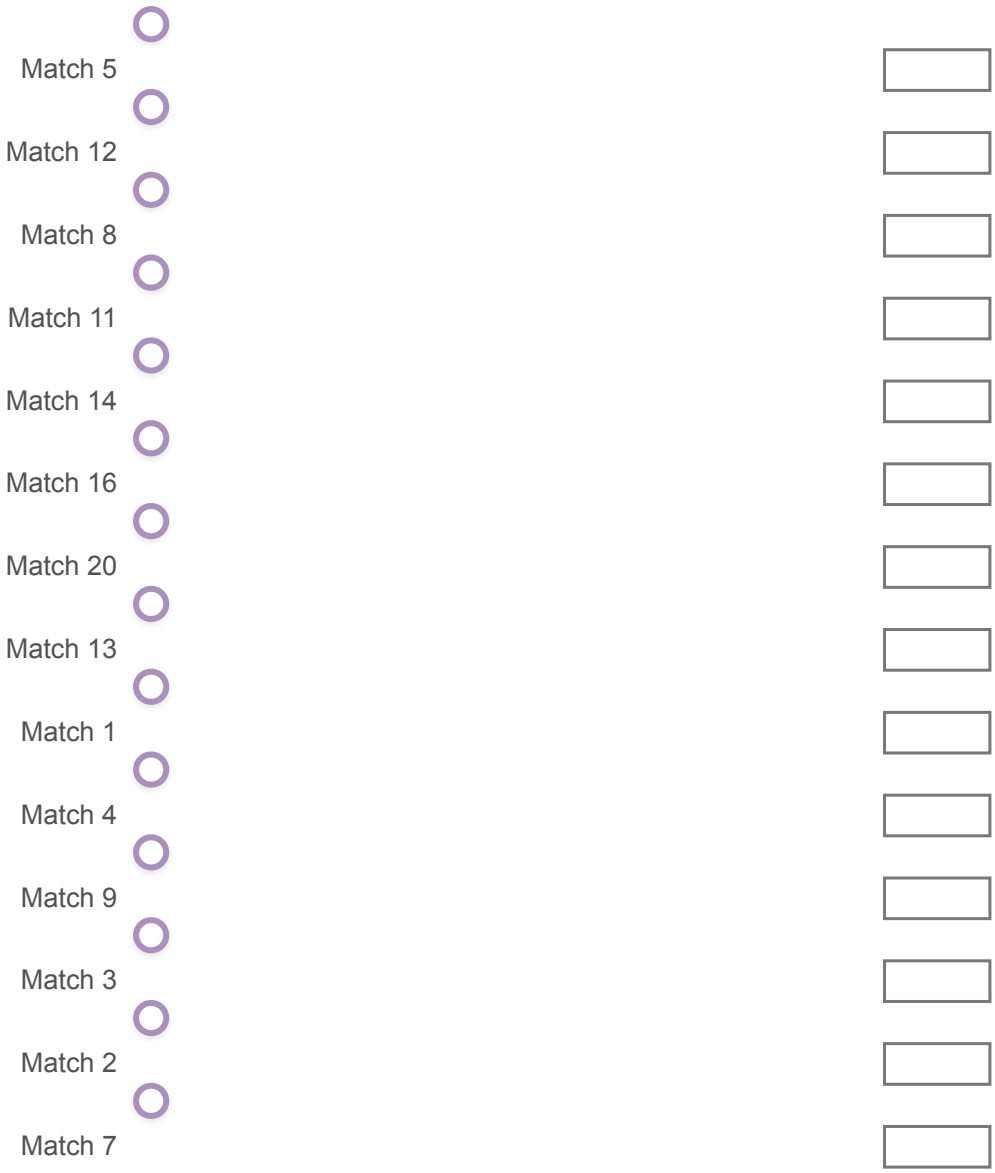
If you fail to enter the Prolific code now or you do not return and complete the survey, you will not receive payment for any of the studies you have completed as part of this survey.

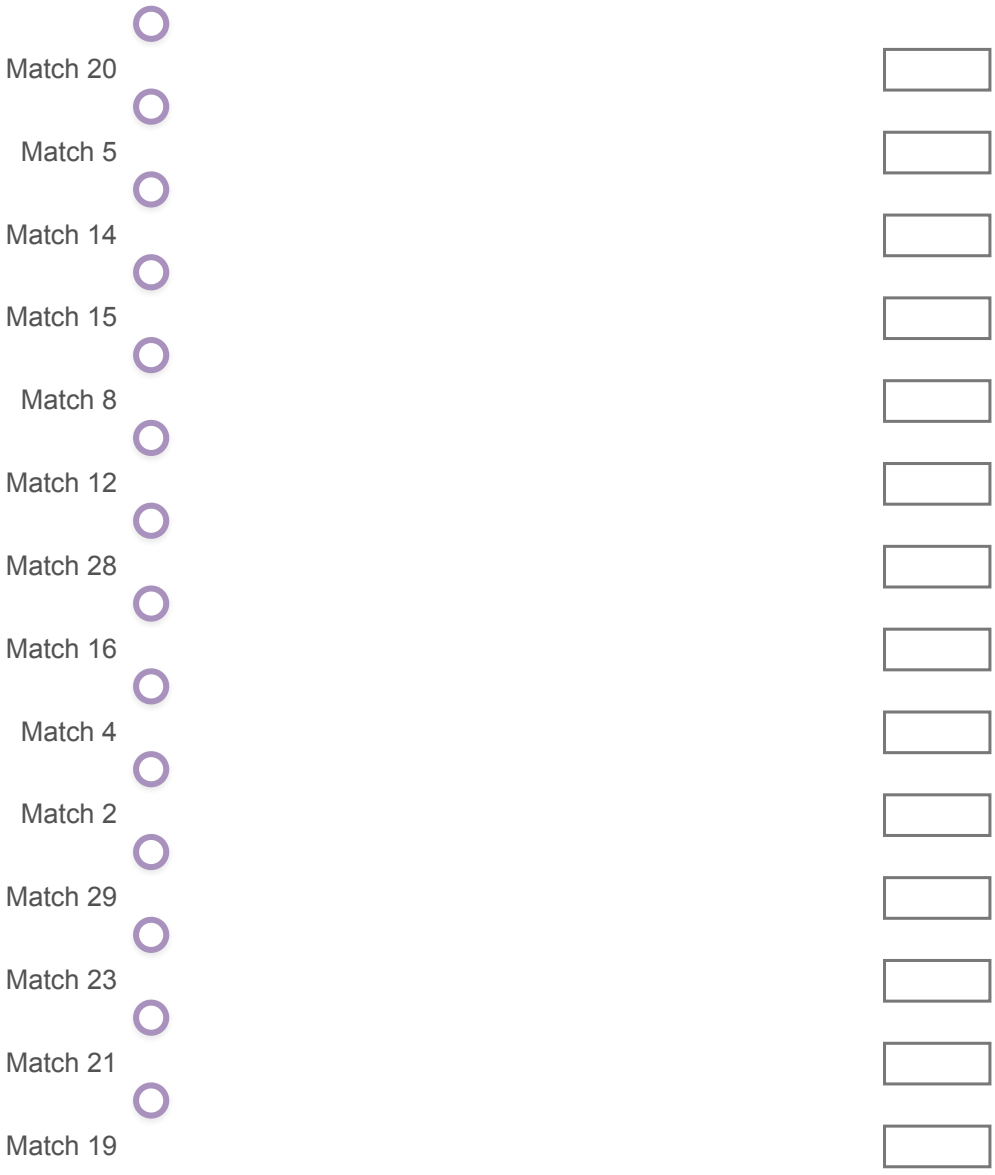
Please confirm that you have typed the completion code into Prolific.

- ◯ Yes, I have typed the completion code into Prolific.
- ◯ No, I have not typed the completion code into Prolific yet.

Easy1

Easy set 1 of ${e://Field/All_Implemented_1}: Match each of the 20 sliders to the amount written on the left. You will not be able to continue until all sliders are matched correctly.





Block 43

Thank you for completing this part of the study. Remember that there are 2 more parts of the study, and you must complete them all for your submissions to be approved. The next part of the study will

appear on your Prolific dashboard on Monday and you will be required to complete that part by Tuesday night.

Please don't forget to click the arrow at the bottom to finalize this part of the survey. Not doing this could delay your payment or even result in being dropped from the experiment.

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
This Week					Part 1: Introduction		
Next Week		Part 2: 1.Complete IQ quiz 2.Choose tasks for Part 3			Part 3: 1. Choose tasks for Part 3 2. Complete Part 3 tasks		
Week After Next		Part 4: 1.Complete IQ quiz 2.Choose tasks for Part 5			Part 5: 1. Choose tasks for Part 5 2. Complete Part 5 tasks		Submissions approved and payments sent

Completed Parts
Current Part
Future Parts

II.4 Survey 4

Quiz 2 Block

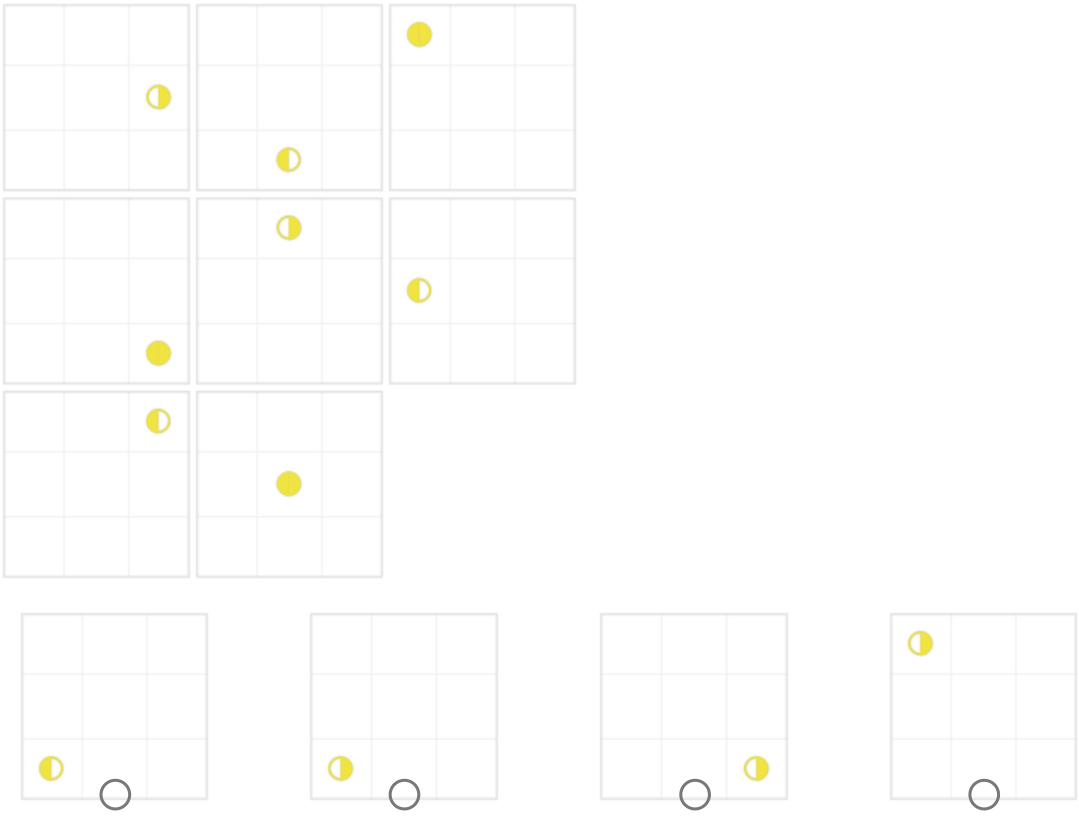
Thank you for logging in to complete the fourth part of the study!

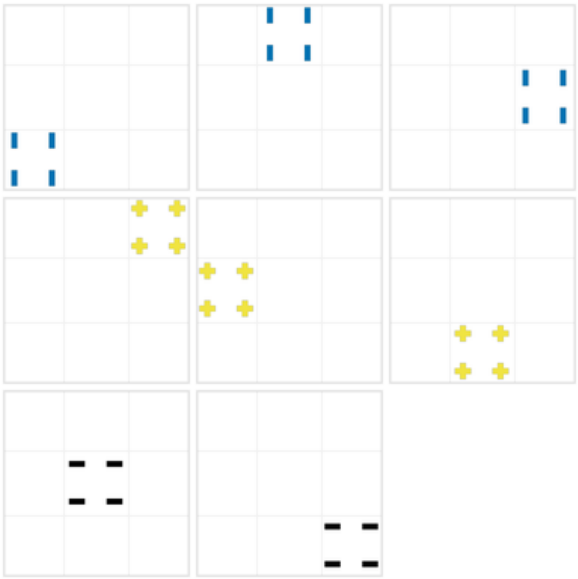
Before continuing with the study, you will compete in a contest against one other player that you are randomly matched with. The outcome of this contest will determine the difficulty of the sets of sliders that you will be asked to complete in the next part of the study. If you win the contest, you will need to complete “easy” sets that involve 20 sliders each. If you lose the contest, you will need to complete “hard” sets that will involve 30 sliders each. The payment that you receive for completing each set of sliders will be the same whether they are easy or hard.

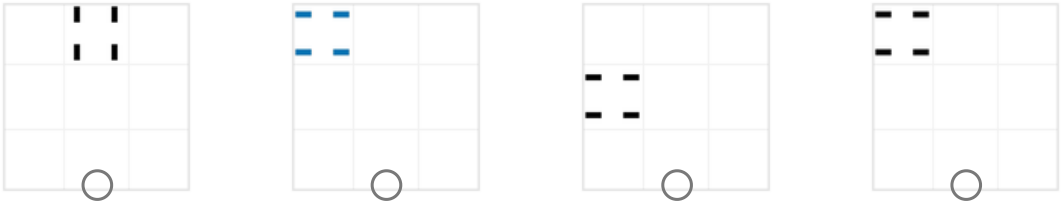
The contest will involve an IQ quiz with 10 questions. The quiz is similar to the quiz from the second part of the study, but the questions are different. In each question you will see eight images with a missing slot for a ninth. You will choose the best fit from among the four options that you are given.

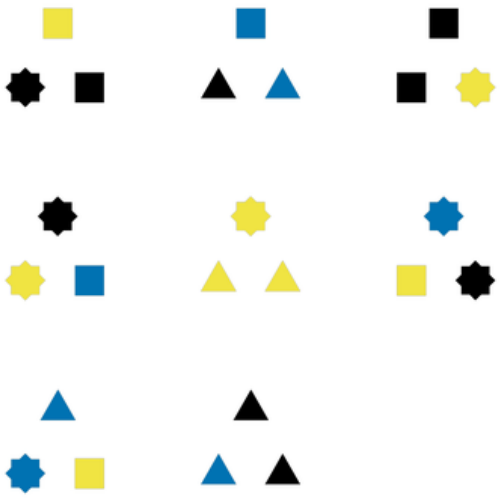
We will count the number of correct answers you and your opponent give. You will win the contest if you have a higher score than the person you are matched with. If you have the same score as the person you are matched with, the winner will be chosen randomly.

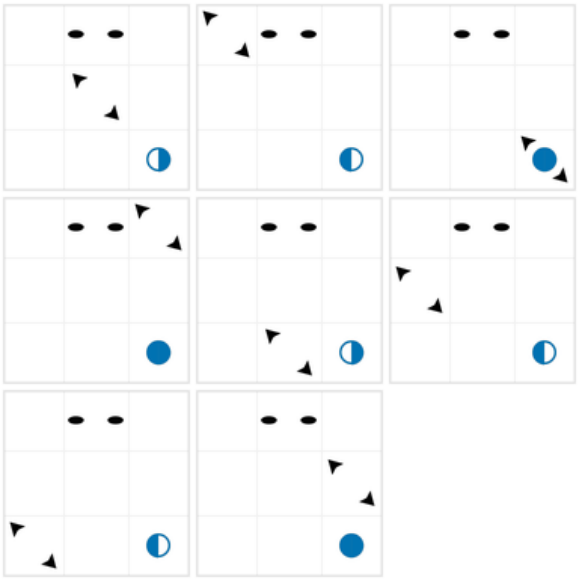
Please choose the best fit among the four options below for each question:

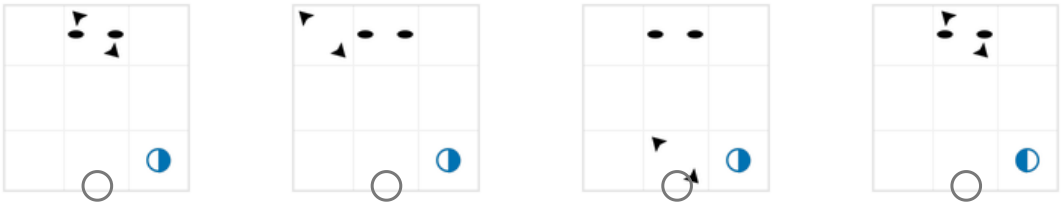


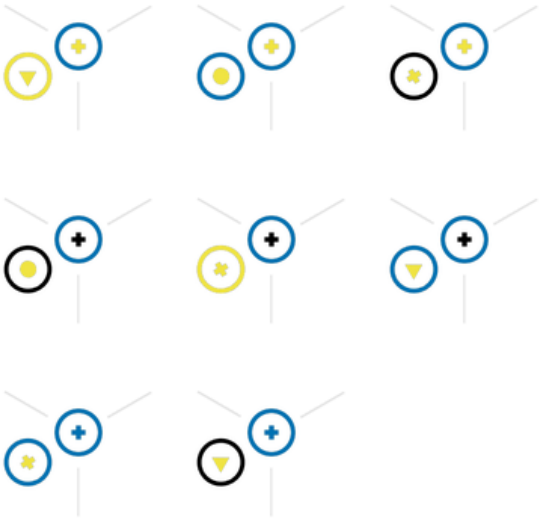


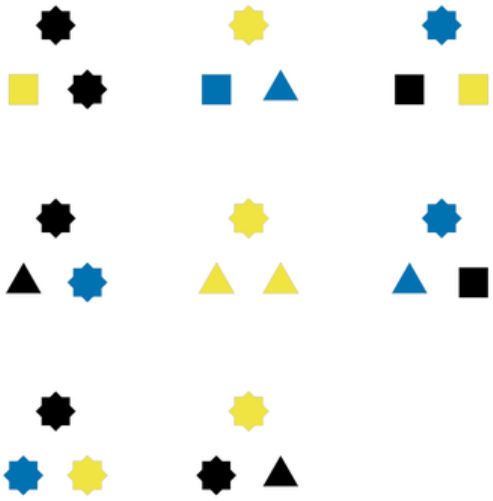


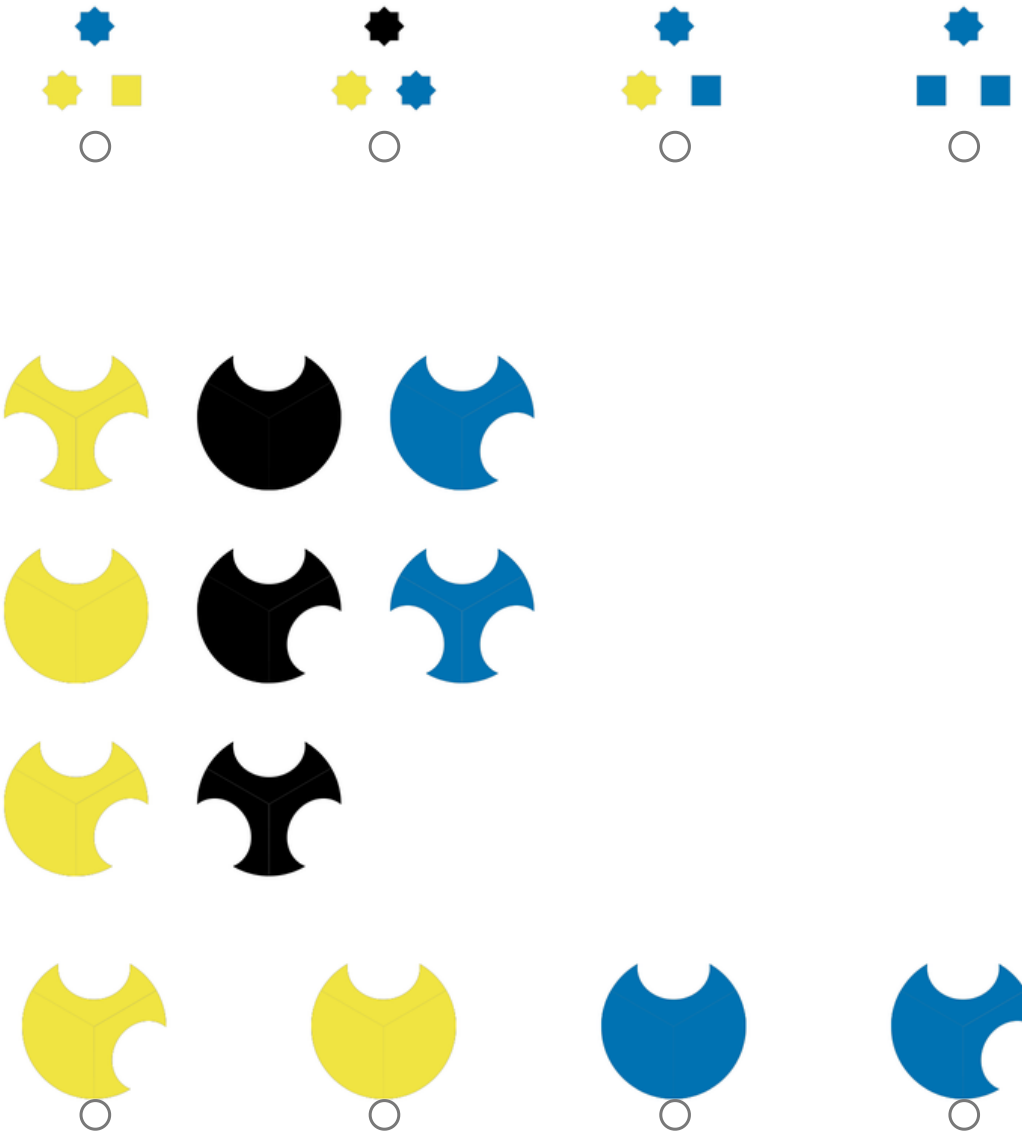


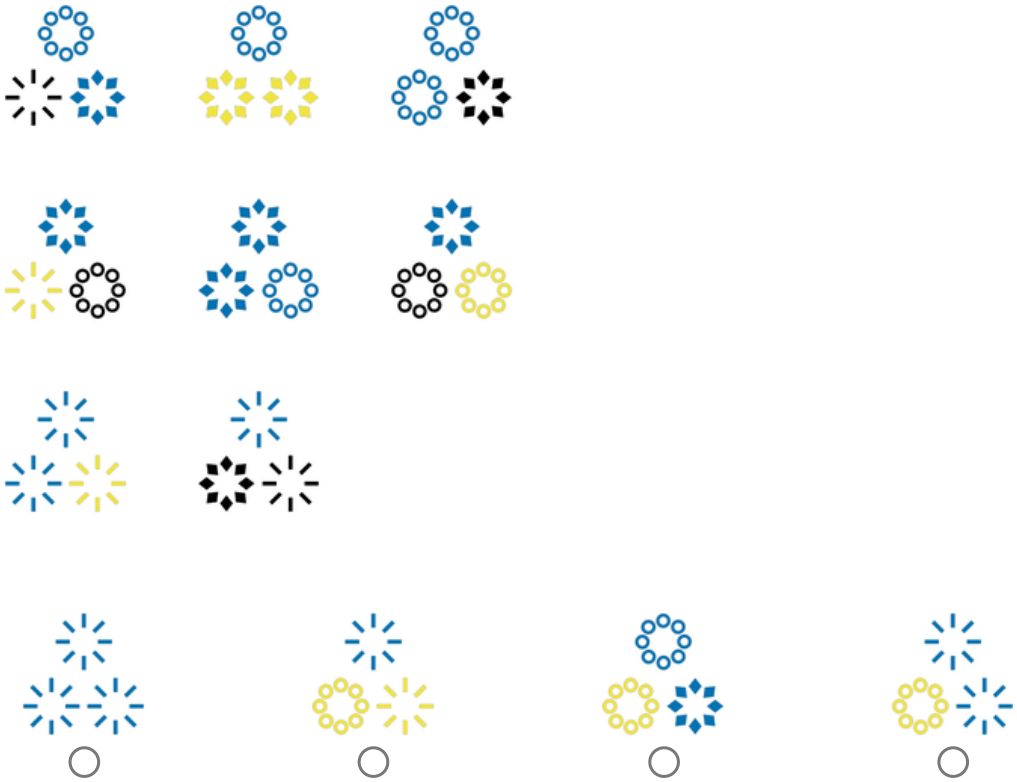


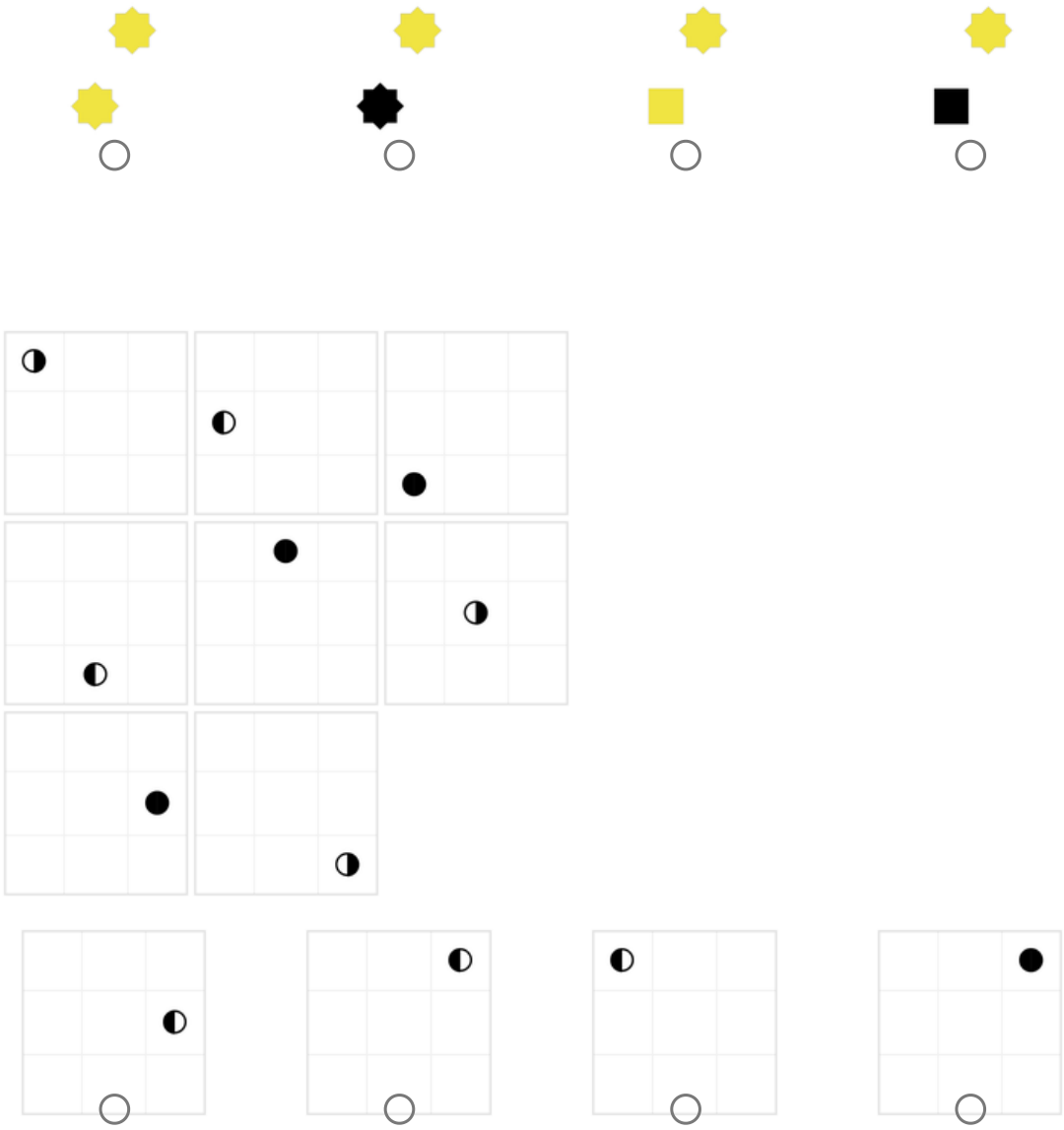












Contest Beliefs: Control and Task Info Treatment

As a reminder, you will win the contest if you have a higher score than the person you are matched with. If you have the same score as the person you are matched with, the winner will be chosen randomly.

What do you think are the chances, out of 100, that you will win the contest? You can write down any number from 0 to 100 out of 100.

Contest Signal Treatment

We already told you that you won the contest from Part 2 of the study. Please confirm that you understand this information

- ◯ I won the contest from the previous study.
- ◯ I did not win the contest from the previous study.

We already told you that you did not win the contest from Part 2 of the study. Please confirm that you understand this information

- ◯ I won the contest from the previous study.
- ◯ I did not win the contest from the previous study.

We also matched you with two other randomly drawn participants from the previous study, and **you lost against both of them**. Please confirm that you understand this information.

- ◯ I won against 0 out of 2 other randomly drawn participants.
- ◯ I won against 1 out of 2 other randomly drawn participants.
- ◯ I won against 2 out of 2 other randomly drawn participants.

We also matched you with two other randomly drawn participants from the previous study, and **you won against one of them**. Please confirm that you understand this information.

- ◯ I won against 0 out of 2 other randomly drawn participants.
- ◯ I won against 1 out of 2 other randomly drawn participants.
- ◯ I won against 2 out of 2 other randomly drawn participants.

We also matched you with two other randomly drawn participants from the previous study, and **you won against both of them**. Please confirm that you understand this information.

- ◯ I won against 0 out of 2 other randomly drawn participants.
- ◯ I won against 1 out of 2 other randomly drawn participants.
- ◯ I won against 2 out of 2 other randomly drawn participants.

As a reminder, you will win the contest if you have a higher score than the person you are matched with. If you have the same score as the person you are matched with, the winner will be chosen randomly.

What do you think are the chances, out of 100, that you will win the contest? You can write down any number from 0 to 100 out of 100.

Task Info Treatment

In Session 2, for a payment rate of $0.06 per set and knowing the sets would be easy, you agreed to complete ${e://Field/All_Com_Easy_1_10} sets. In Session 3, in the same setting, you agreed to complete ${e://Field/All_Uncom_Easy_1_10} sets.

In Session 2, for a payment rate of $0.12 per set and knowing the sets would be easy, you agreed to complete ${e://Field/All_Com_Easy_1_20} sets. In Session 3, in the same setting, you agreed to complete ${e://Field/All_Uncom_Easy_1_20} sets.

In Session 2, for a payment rate of $0.18 per set and knowing the sets would be easy, you agreed to complete ${e://Field/All_Com_Easy_1_30} sets. In Session 3, in the same setting, you agreed to complete ${e://Field/All_Uncom_Easy_1_30} sets.

In Session 2, for a payment rate of $0.06 per set and knowing the sets would be hard, you agreed to complete ${e://Field/All_Com_Hard_1_10} sets. In Session 3, in the same setting, you agreed to complete ${e://Field/All_Uncom_Hard_1_10} sets.

In Session 2, for a payment rate of $0.12 per set and knowing the sets would be hard, you agreed to complete ${e://Field/All_Com_Hard_1_20} sets. In Session 3, in the same setting, you agreed to complete ${e://Field/All_Uncom_Hard_1_20} sets.

In Session 2, for a payment rate of $0.18 per set and not knowing whether the sets would be easy or hard, you agreed to complete ${e://Field/All_Com_Unc_1_30} sets. In Session 3, in the same setting, you agreed to complete ${e://Field/All_Uncom_Unc_1_30} sets.

In Session 2, for a payment rate of $0.06 per set and not knowing whether the sets would be easy or hard, you agreed to complete ${e://Field/All_Com_Unc_1_10} sets. In Session 3, in the

same setting, you agreed to
complete ${e://Field/All_Uncom_Unc_1_10} sets.

In Session 2, for a payment rate of $0.12 per set and not knowing whether the sets would be easy or hard, you agreed to complete ${e://Field/All_Com_Unc_1_20} sets. In Session 3, in the same setting, you agreed to
complete ${e://Field/All_Uncom_Unc_1_20} sets.

In Session 2, for a payment rate of $0.18 per set and not knowing whether the sets would be easy or hard, you agreed to complete ${e://Field/All_Com_Unc_1_30} sets. In Session 3, in the same setting, you agreed to
complete ${e://Field/All_Uncom_Unc_1_30} sets.

So, **the amount of sets you chose in Session 2 is lower than the amount you chose in Session 3.** Please confirm you understand this information.

- ◯ The amount of sets I chose in Session 2 is higher than the amount I chose in Session 3.
- ◯ The amount of sets I chose in Session 2 is the same as the amount I chose in Session 3.
- ◯ The amount of sets I chose in Session 2 is lower than the amount I chose in Session 3.

So, **the amount of sets you chose in Session 2 is the same as the amount you chose in Session 3**. Please confirm you understand this information.

- ◯ The amount of sets I chose in Session 2 is higher than the amount I chose in Session 3.
- ◯ The amount of sets I chose in Session 2 is the same as the amount I chose in Session 3.
- ◯ The amount of sets I chose in Session 2 is lower than the amount I chose in Session 3.

So, **the amount of sets you chose in Session 2 is higher than the amount you chose in Session 3**. Please confirm you understand this information.

- ◯ The amount of sets I chose in Session 2 is higher than the amount I chose in Session 3.
- ◯ The amount of sets I chose in Session 2 is the same as the amount I chose in Session 3.
- ◯ The amount of sets I chose in Session 2 is lower than the amount I chose in Session 3.

Why might someone's choices change over time?

Commitment Choice 2

You will have the opportunity to complete sets of sliders for payment in fifth part of the study. We will ask you how many sets of sliders you want to complete for various payment rates both in the fourth part of the study (this part) and in the fifth part of the study. However, only one of these choices will be the one that counts.

Now, you will make a series of choices that allow you to affect the chance that your choices in this part of the study are the ones that count. These choices will involve extra sets of easy sliders. By choosing more or fewer easy sliders, you will change the probability that your choices in this part of the study are the ones that count.

<u>This decision will measure the strength of your preference for which choices count.</u>

We will randomly select which one of these choices will be the one that counts. When you make these decisions, treat every decision as if it is the one that counts because each decision could count.

Make this survey's choices more likely:	**Make the next survey's choices more likely:**
1 easy set of sliders today, 4 out of 5 chance of today's choices being the ones that count	3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Make this survey's choices more likely:	Make the next survey's choices more likely:
2 easy sets of sliders today, 4 out of 5 chance of today's choices being the ones that count	3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Make this survey's choices more likely:	Make the next survey's choices more likely:
3 easy sets of sliders today, 4 out of 5 chance of today's choices being the ones that count	3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Make this survey's choices more likely:	Make the next survey's choices more likely:
4 easy sets of sliders today, 4 out of 5 chance of today's choices being the ones that count	3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Make this survey's choices more likely:	Make the next survey's choices more likely:
5 easy set of sliders today, 4 out of 5 chance of today's choices being the ones that count	3 easy sets of sliders today, 1 out of 5 chance of today's choices being the ones that count

Task Allocation 2

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line

you chose "${q://QID28/ChoiceGroup/SelectedChoices}."

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line you chose "${q://QID29/ChoiceGroup/SelectedChoices}."

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line you chose "${q://QID30/ChoiceGroup/SelectedChoices}."

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line you chose "${q://QID31/ChoiceGroup/SelectedChoices}."

We randomly selected one of the lines from the previous page, and your choice from that line will count. In the randomly selected line you chose "${q://QID32/ChoiceGroup/SelectedChoices}."

In the fifth part of the study, you will be asked to complete a number of slider tasks. Each slider task will contain either 20 sliders (if you win the contest) or 30 sliders (if you lose the contest). You will be paid for each set of sliders that you complete.

We would like to know how many sets of sliders you would like to complete in the fifth part of the study for different potential payment rates. Furthermore, you must choose how many sets of sliders you would like to complete if the sets of sliders are easy (20 sliders each), hard (30 sliders each), or without knowing whether they are easy or hard.

One choice will be randomly selected to be the one that counts. Recall that there is a 1 out of 5 chance of today's choices being the ones that count and 4 out of 5 chance of the next survey's choices being the ones that count. Because any choice can be selected, **it is in your interest to make every choice as if it will be the one that counts.**

In the fifth part of the study, you will be asked to complete a number of slider tasks. Each slider task will contain either 20 sliders (if you win the contest) or 30 sliders (if you lose the contest). You will be paid for each set of sliders that you complete.

We would like to know how many sets of sliders you would like to complete in the fifth part of the study for different potential payment rates. Furthermore, you must choose how many sets of sliders you would like to complete if the sets of sliders are easy (20 sliders

each), hard (30 sliders each), or without knowing whether they are easy or hard.

One choice will be randomly selected to be the one that counts. Recall that there is a 4 out of 5 chance of today's choices being the ones that count and 1 out of 5 chance of the next survey's choices being the ones that count. Because any choice can be selected, **it is in your interest to make every choice as if it will be the one that counts.**

How many sets of sliders would you like to complete if you **win** the contest (so **the sets are easy**)? Please choose a number between 0 and 19 for each payment rate. Because this choice will only count if you win the contest, the choice that is best for you **should not** depend on whether you think you will win.

$0.06/set of sliders

$0.12/set of sliders

$0.18/set of sliders

How many sets of sliders would you like to complete if you **lose** the contest (so **the sets are hard**)? Please choose a number between 0 and 19 for each payment rate. Because this choice will only count if

you lose the contest, the choice that is best for you **should not** depend on whether you think you will win.

$0.06/set of sliders	
$0.12/set of sliders	
$0.18/set of sliders	

How many sets of sliders would you like to complete **without knowing** whether you won the contest (so **the sets may be easy or hard**)? Please choose a number between 0 and 19 for each payment rate. The choice that is best for you **should** depend on whether you think you will win the contest.

$0.06/set of sliders	
$0.12/set of sliders	
$0.18/set of sliders	

Commitment Payment Tasks

Recall that you chose "${q://QID28/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_2} set of sliders now.

Recall that you chose "${q://QID29/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_2} set of sliders now.

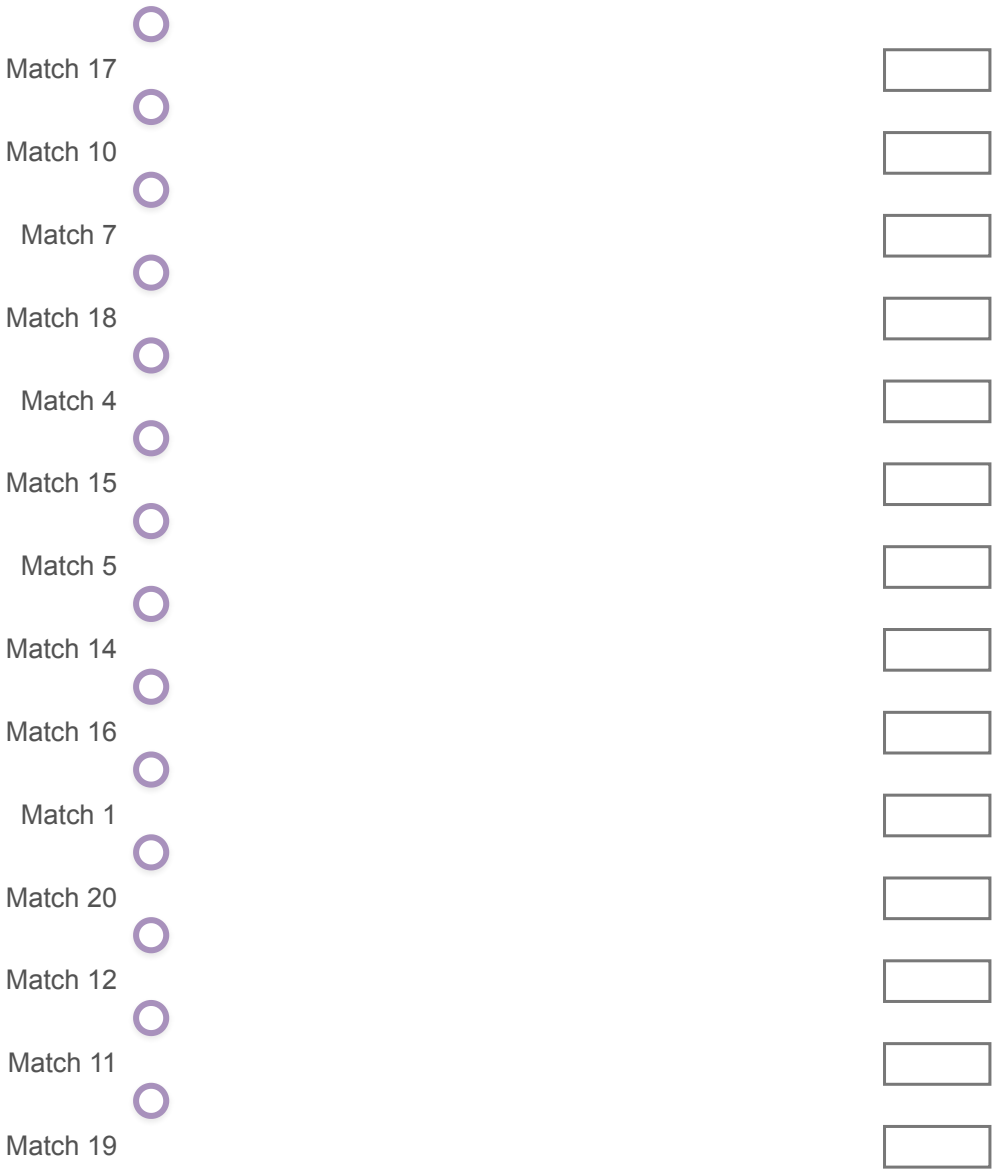
Recall that you chose "${q://QID30/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_2} set of sliders now.

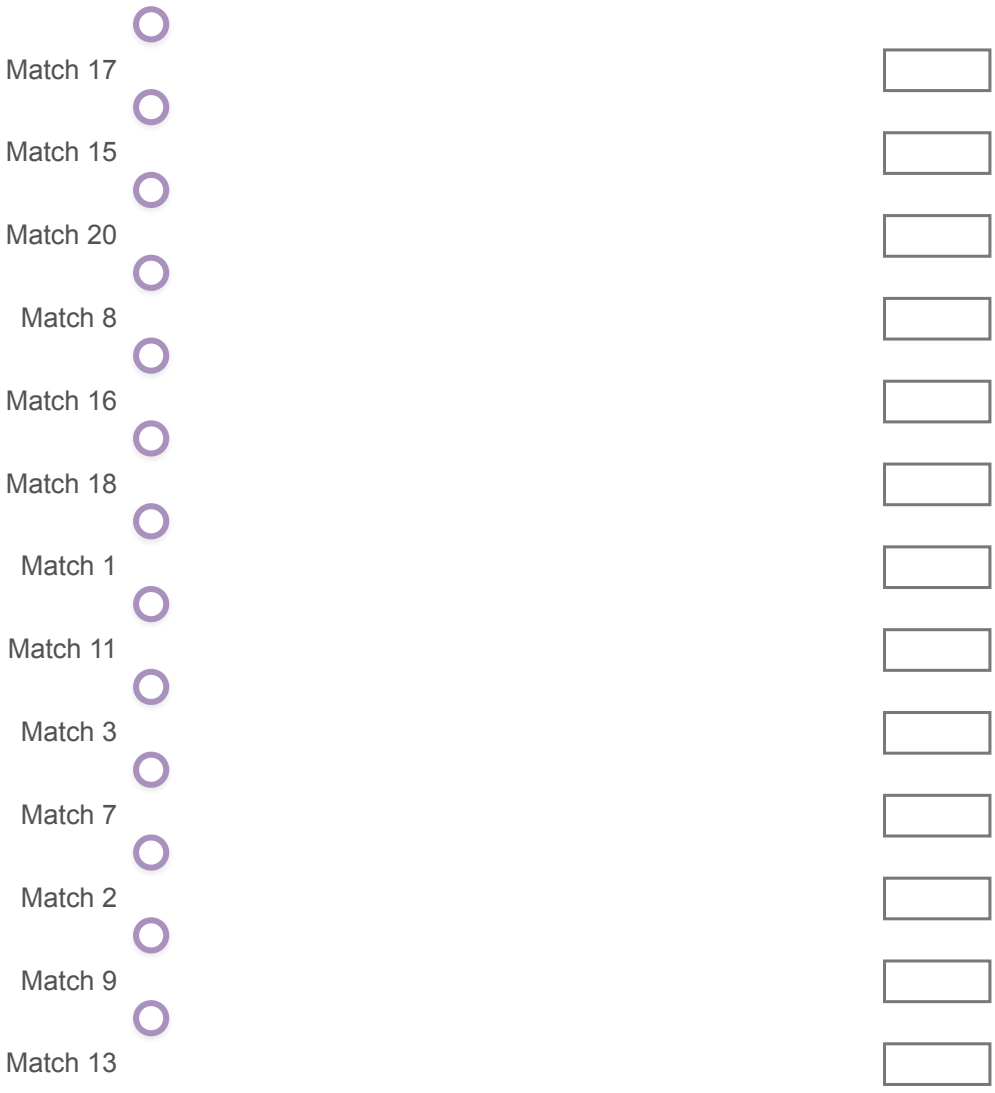
Recall that you chose "${q://QID31/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_2} set of sliders now.

Recall that you chose "${q://QID32/ChoiceGroup/SelectedChoices}." So you will complete ${e://Field/Task_Price_Commitment_2} set of sliders now.

Easy Block 1

Set 1 of ${e://Field/Task_Price_Commitment_2}: match each of the 20 sliders to the amount written on the left. You will not be able to continue until all sliders are matched correctly.





Goodbye

Thank you for completing this part of the study. Remember that there is 1 more part of the study, and you must complete it for your submissions to be approved. The next part of the study will appear on your Prolific dashboard on Thursday and you will be required to complete that part by Friday night.

Please don't forget to click the arrow at the bottom to finalize this part of the survey. Not doing this could delay your payment or even result in being dropped from the experiment.

	Sunday	Monday	Tuesday	Wednesday	Thursday	Friday	Saturday
This Week					Part 1: Introduction		
Next Week		Part 2: 1.Complete IQ quiz 2.Choose tasks for Part 3			Part 3: 1. Choose tasks for Part 3 2. Complete Part 3 tasks		
Week After Next		Part 4: 1.Complete IQ quiz 2.Choose tasks for Part 5			Part 5: 1. Choose tasks for Part 5 2. Complete Part 5 tasks		Submissions approved and payments sent

Completed Parts
Current Part
Future Parts

II.5 Survey 5

Task Allocation

Thank you for logging in to complete the fifth and final part of the study!

In this part of the study, you will be asked to complete a number of slider tasks. Each slider task will contain either 20 sliders (if you win the contest) or 30 sliders (if you lose the contest). You will be paid for each set of sliders that you complete.

We would like to know how many sets of sliders you would like to complete in this part of the study for different potential payment rates. You must choose how many sets of sliders you would like to complete without knowing whether they are easy or hard. We will then tell you whether you won the contest and again ask you how many sets of sliders you would like to complete.

One choice will be randomly selected to be the one that counts. Recall that there is a 1 out of 5 chance of the last survey's choices being the ones that count and 4 out of 5 chance of today's choices being the ones that count. Because any choice can be selected, **it is**

in your interest to make every choice as if it will be the one that counts.

In this part of the study, you will be asked to complete a number of slider tasks. Each slider task will contain either 20 sliders (if you win the contest) or 30 sliders (if you lose the contest). You will be paid for each set of sliders that you complete.

We would like to know how many sets of sliders you would like to complete in this part of the study for different potential payment rates. You must choose how many sets of sliders you would like to complete without knowing whether they are easy or hard. We will then tell you whether you won the contest and again ask you how many sets of sliders you would like to complete.

One choice will be randomly selected to be the one that counts. Recall that there is a 4 out of 5 chance of the last survey's choices being the ones that count and 1 out of 5 chance of today's choices being the ones that count. Because any choice can be selected, **it is in your interest to make every choice as if it will be the one that counts**.

How many sets of sliders would you like to complete **without knowing** whether you won the contest (so **the sets may be easy or hard**)? Please choose a number between 0 and 19 for each

payment rate. The choice that is best for you **should** depend on whether you think you will win the contest**.**

$0.06/set of sliders

$0.12/set of sliders

$0.18/set of sliders

Contest Outcome

We can now reveal that **you won the contest**.

We can now reveal that **you lost the contest**.

Now that you know that you have won the contest (so **the sets are easy**), how many sets of sliders would you like to complete? Please choose a number between 0 and 19 for each payment rate.

$0.06/set of sliders

$0.12/set of sliders

$0.18/set of sliders

Now that you know that you have lost the contest (so **the sets are hard**), how many sets of sliders would you like to complete? Please choose a number between 0 and 19 for each payment rate.

$0.06/set of sliders

$0.12/set of sliders

$0.18/set of sliders

Allocation Information

We randomly selected among the potential payment rates, and the rate that will count for you will be $0.06/set of sliders.

We randomly selected among the potential payment rates, and the rate that will count for you will be $0.12/set of sliders.

We randomly selected among the potential payment rates, and the rate that will count for you will be $0.18/set of sliders.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_2_10} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Easy_2_10} sets of sliders if you won the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_2_10} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Easy_2_10} sets of sliders knowing you won the contest.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_2_20} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Easy_2_20} sets of sliders if you won the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_2_20} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Easy_2_20} sets of sliders knowing you won the contest.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_2_30} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Easy_2_30} sets of sliders if you won the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_2_30} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Easy_2_30} sets of sliders knowing you won the contest.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_2_10} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Hard_2_10} sets of sliders if you lost the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_2_10} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Hard_2_10} sets of sliders knowing you lost the contest.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_2_20} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Hard_2_20} sets of sliders if you lost the contest. Earlier in this part of the study, you indicated that you

would do ${e://Field/All_Uncom_Unc_2_20} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Hard_2_20} sets of sliders knowing you lost the contest.

In the second part of the study, for the payment rate that counts, you indicated that you would do ${e://Field/All_Com_Unc_2_30} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Com_Hard_2_30} sets of sliders if you lost the contest. Earlier in this part of the study, you indicated that you would do ${e://Field/All_Uncom_Unc_2_30} sets of sliders without knowing the outcome of the contest, and that you would do ${e://Field/All_Uncom_Hard_2_30} sets of sliders knowing you lost the contest.

We have randomly selected among these four options and today you will complete ${e://Field/All_Implemented_2} sets of sliders.

Prolific Completion Code

Before completing the sets of sliders that you agreed to, please go to Prolific and type in the completion code CP8TQ9VA. **You must type in this code before starting with the sliders so that you do**

not time out of the study. It is important that you enter the code now because you will likely time out of the Prolific study if you do not.

Please remember that you **must** complete the sets of sliders that you agreed to today in order to continue with the study and have your submissions approved.

If you fail to enter the Prolific code now or you do not return and complete the sliders, you will not receive payment for any of the surveys you have completed as part of this study.

Please go to Prolific and type in the completion code CP8TQ9VA. After typing in the code, please return to this survey and complete it.

You **must** complete the survey in order to continue with the study and have your submissions approved.

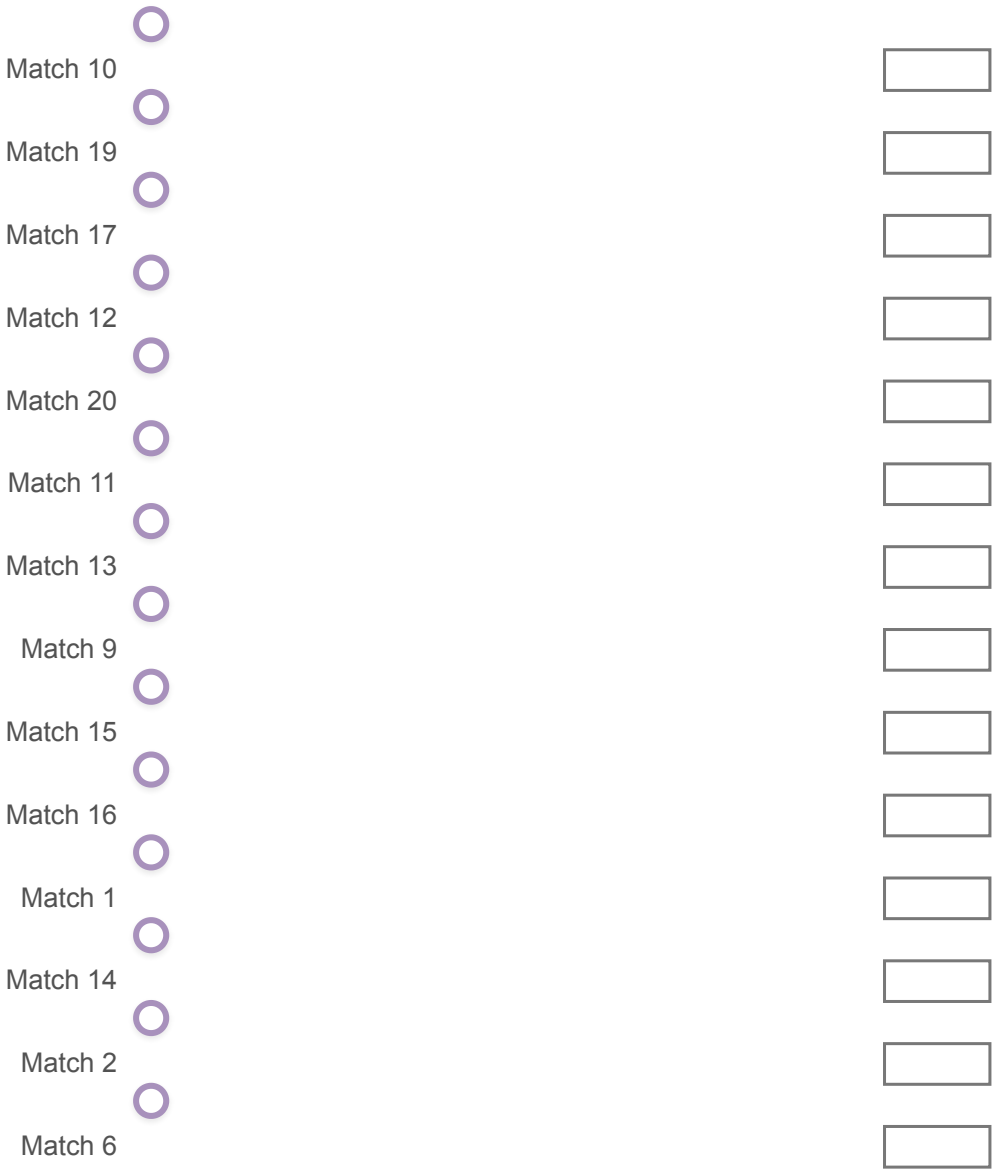
If you fail to enter the Prolific code now or you do not return and complete the survey, you will not receive payment for any of the studies you have completed as part of this survey.

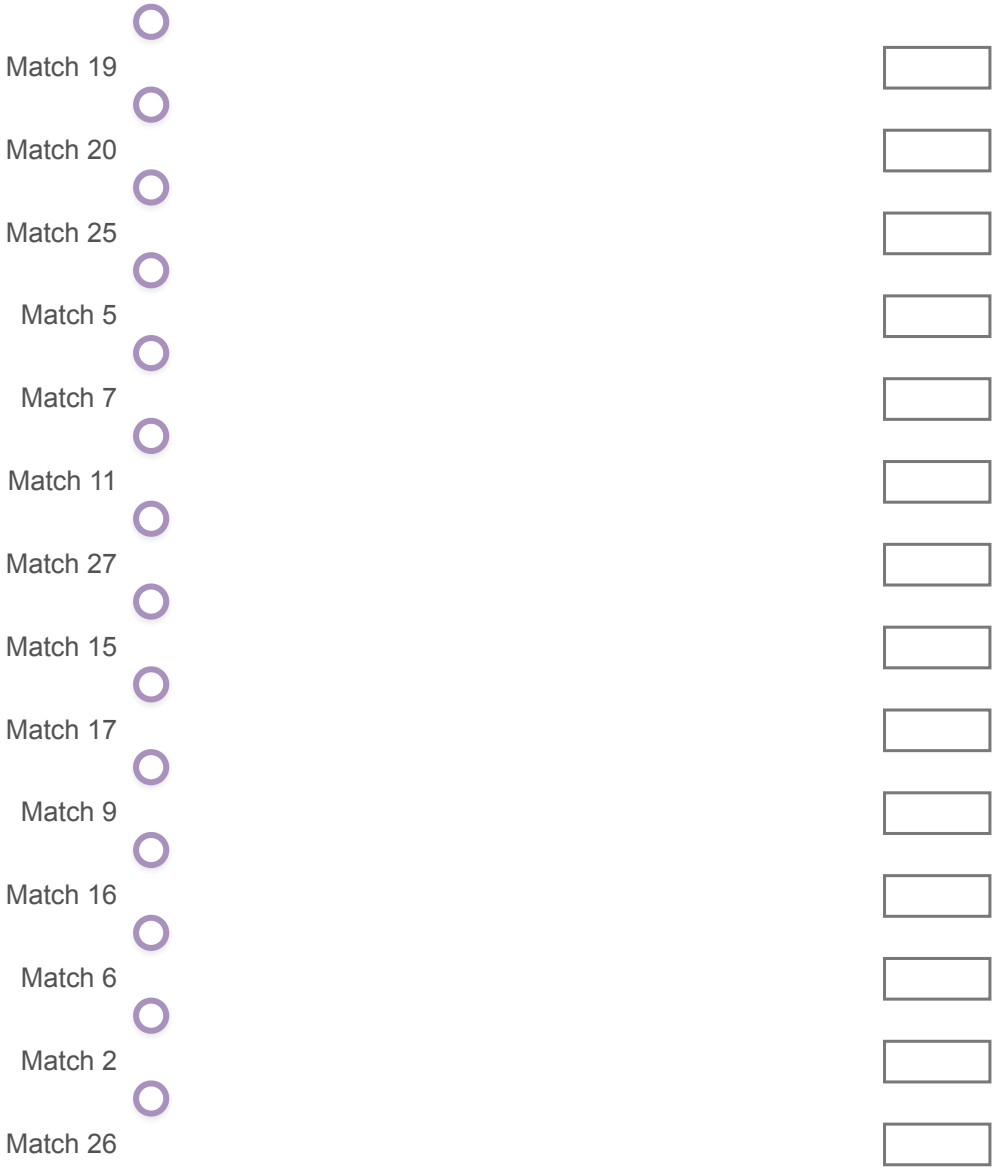
Please confirm that you have typed the completion code into Prolific.

- ◯ Yes, I have typed the completion code into Prolific.
- ◯ No, I have not typed the completion code into Prolific yet.

Easy1

Easy set 1 of ${e://Field/All_Implemented_2}: Match each of the 20 sliders to the amount written on the left. You will not be able to continue until all sliders are matched correctly.





Goodbye

That was the final part of the study. Thank you for completing all parts.

Please don't forget to click the arrow at the bottom to finalize this part of the survey. Not doing this could delay your payment or even result in being dropped from the experiment.