Temptation: Immediacy and certainty*

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Job market paper. Latest version available here.

Abstract Is an option especially tempting when it is both immediate and certain? I test the effect of risk on the present-bias factor under quasi-hyperbolic discounting. In my experiment workers allocate about thirty to fifty minutes of real-effort tasks between two weeks. I study dynamic consistency by comparing choices made two days in advance of the workday with choices made when work is imminent. My novel design permits estimation of present bias using a decision with a consequence that is both immediate and certain. I find far greater present bias when the consequence is certain, with broad implications for any economic decision involving a present-biased individual. I offer a methodological remedy for experimental economists.

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Keywords present bias, dynamic inconsistency, quasi-hyperbolic discounting, time preferences, risk preferences, immediacy effect, certainty effect, experimental economics

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1 Introduction

While risk and time preferences are fundamental to the theory of decision-making, much remains unknown about the interplay between these two dimensions. A future prospect is inherently risky if any circumstance may arise that precludes consumption of the consequence. This implies that outcomes are obtained with certainty only if obtained without delay. Accordingly a preference for certain outcomes results in a preference for immediate outcomes. Conversely, the introduction of risk may especially diminish the appeal of an immediate reward, relative to a delayed reward. I explore in this paper such an interaction between immediacy and certainty with an experiment of dynamic decision-making over risky and delayed prospects.

Present-biased preferences explain oft-bemoaned consumer behavior, such as the failure to meet one's own physical exercise goals and the over-utilization of credit-card debt (Royer, Stehr, and Sydnor 2015; Meier and Sprenger 2010). Firms exploit consumer present bias and successfully extract welfare (DellaVigna and Malmendier 2004). Meanwhile, incentives, commitment devices, and other interventions may (or may not) help consumers improve their long-term welfare (Ashraf, Karlan, and Yin 2006; Carrera et al. 2022). A better understanding of present bias assists this body of research.

As an example, my study informs labor contract design, especially those used in the modern gig economy. Consider drivers for ride-hail companies—these workers face decisions similar to those of the workers in my experiment. Ride-hail companies carefully withhold selective information (such as ride length or destination) when offering a gig to a driver and require commitment prior to revealing all of these ride details (Rana 2020). Such uncertainty in a spot labor contract theoretically affects labor supply; my results confirm that a present-biased worker with a weekly income target may procrastinate less given greater uncertainty.

My novel experimental design allows estimation of present bias for subjects making a single decision with a certain consequence. This is in contrast to a baseline treatment that implements an allocation choice made on a randomly-selected day and at a randomly-selected intertemporal price ratio, in accord with prevailing experimental methodology.

Workers in my experiment allocate a workload between two weeks. Each worker first makes allocation decisions two days before the first workday. Each worker then returns on the first workday and makes identical decisions with the work being imminent. If a worker is present-biased, she will in advance choose some allocation between the two weeks, but then on the first workday, prefer an allocation with less work for the present day. In my experiment, once the implementation mechanism selects a particular allocation, the worker must complete the tasks allocated to each week to earn a substantial bonus payment.

Ultimately I find that the introduction of risk significantly attenuates the immediacy effect. Specifically, the quasi-hyperbolic present-bias factor β becomes smaller with the elimination of risk, implying greater myopia. In my experiment, when a workload allocation is implemented with certainty, subjects on average discount the future by a factor of $\hat{\beta}$ = 0.281 relative to the present. In the baseline treatment that uses prevailing experimental methodology (with each decision having a 10% implementation probability), I estimate $\hat{\beta}$ = 0.895, a typical result in the literature.

These findings underscore the importance of decision-theoretic frameworks that permit interaction between dimensions of risk and time. The findings also suggest that studies of tempting goods may necessitate decisions with temporally salient and certain consequences; researchers should keep this in mind when designing either lab or field experiments. Further, risk introduced by randomized incentive mechanisms—common experimental methodology—may require augmentation of decisions with certain consequences. I offer such a methodological remedy.

2 Background

To model intertemporal decision-making, Samuelson (1937) introduced *exponential discounted utility* (DU), which describes how an individual values utility flows (of consumption goods, such as leisure) that occur over time. If utility flows $u(x_{t+\tau})$ result from consumption $x_{t+\tau}$ at time $t+\tau \in \mathbb{N}$, given a constant discount factor $\delta \in [0,1]$, the model gives an intertemporal value at time t of

$$U_t^{\mathrm{DU}} = \sum_{\tau=0} \delta^\tau u(x_{t+\tau}). \tag{1}$$

A decision-maker with this value function will make *dynamically consistent* choices, assuming that the felicity function u is time-invariant (Halevy 2015).

To capture a preference for immediate utility, Laibson (1997) introduces the present-bias factor $\beta \geq 0$ to discount all future utility flows contra present utility. The resultant *quasi-hyperbolic discounted utility* (QHD) has an intertemporal value at time t of

$$U_t^{\mathrm{QHD}} = u(x_t) + \beta \sum_{\tau=1} \delta^\tau u(x_{t+\tau}). \tag{2}$$

$\beta < 1$ describes a preference for immediacy, also referred to as *present bias*. This is an example of *diminishing sensitivity to delay*—an individual is more impatient regarding a delay in felicity that happens immediately relative to a delay that occurs in the future. Meanwhile, some individuals may exhibit future bias, with $\beta > 1$.

A decision-maker with $\beta \neq 1$ will make *dynamically inconsistent* choices. A decision-maker with $\beta < 1$ ($\beta > 1$) will continually revise consumption plans to achieve greater (lesser) felicity in the present moment relative to her prior plans.

2.1 Diminishing sensitivity to risk and delay

Let a simple gamble $(x \circ p)$ be a prospect that obtains x with probability p. Given the independence axiom of expected utility theory (EUT), a preference relation is maintained if prospect probabilities are multiplied by a common ratio (Machina 1982). Consider

Menu $\mathcal{A}$:	$a = (1 \circ 0.9)$	or	$a' = (2 \circ 0.6)$;	and
Menu $\mathcal{B}$:	$b = (1 \circ 0.6)$	or	$b' = (2 \circ 0.4)$.	

Under EUT, a is weakly preferred to a' if and only if b is weakly preferred to b'.

Allais (1953) presented empirical violations of this result (see also Kahneman and Tversky 1979). The *common ratio effect* describes a preference reversal in which a decision-maker is indifferent between two prospects, but when the prospect probabilities are scaled down by a common ratio, she then strictly prefers the riskier option (*e.g.*, $a \sim a'$ and $b \prec b'$).

The *certainty effect* is a special case of the common ratio effect when one prospect obtains with probability one. For example, consider

Menu $\mathcal{C}$:	$c = (3 \circ 1.0)$	or	$c' = (4 \circ 0.8)$;	and
Menu $\mathcal{R}$:	$r = (3 \circ 0.5)$	or	$r' = (4 \circ 0.4)$.	

If a decision-maker is indifferent between c and c' but strictly prefers r' over r, she may simply possess diminishing sensitivity to risk as described by the common ratio effect, or she may have a disproportionate preference for a certain outcome.

Prelec and Loewenstein (1991) note equivalent results regarding time delay using discounted utility. Following Halevy (2008), let us simply interpret δ in equation (1) as a failure risk imposed by a unit-time delay that precludes consumption of the consequence (*e.g.*, δ might be one's probability of death in every time period). Under DU, a decision-maker weakly prefers one intertemporal consumption plan to another if and only if this preference is maintained with an additional arbitrary time delay.

Consider a daily survival probability of 0.8. Let us reinterpret the previous menus as

Menu $\tilde{C}$:	$C = 3$ now	or	$C' = 4$ in 1 day;	and
Menu $\tilde{R}$:	$R = 3$ in 3 days	or	$R' = 4$ in 4 days.[1]	

The *common difference effect* describes a preference reversal in which a decision-maker is indifferent between two intertemporal consumption plans, but when an arbitrary time delay is added to each, she then becomes more patient (*e.g.*, $C \sim C'$ and $R \prec R'$).

The *immediacy effect* is a special case of the common difference effect, when only immediate consumption varies between plans. In equation (2), QHD describes an immediacy effect if $\beta < 1$. Chakraborty, Halevy, and Saito (2020) fully characterize the relationship between the immediacy effect (including under QHD) and the certainty effect.[2]

In this sense, a decision-maker only obtains a prospect with certainty when also obtained without delay. Any time delay plausibly eliminates a certainty effect, and similarly any risk plausibly eliminates an immediacy effect. I endeavor to experimentally test the significance of this interaction.

2.2 Evidence of risk moderating present bias

At least three studies have shown that risk moderates present bias using hypothetical or nearly-hypothetical monetary incentives.

Keren and Roelofsma (1995) conduct a between-subject full-factorial experiment with

[1]With approximation, 0.5 is the probability of surviving $\ln 0.5/\ln 0.8 \approx 3$ days, and 0.4 is the probability of surviving $\ln 0.4/\ln 0.8 \approx 4$ days.

[2]Epper and Fehr-Duda (2018), Baucells and Heukamp (2010), and Green and Myerson (2004) also explore this relationship.

hypothetical monetary stakes, wherein subjects choose between $50 and $55 with a four-week delay. When prizes obtain with certainty, 82% of subjects prefer $50 immediately over $55 in four weeks, while only 37% of subjects prefer $50 in twenty-six weeks over $55 in thirty weeks, thereby demonstrating present bias at certainty. When prizes obtain with probability one-half, 39% of subjects prefer $50 immediately over $55 in four weeks, and 33% of subjects prefer $50 in twenty-six weeks over $55 in thirty weeks, failing to show significant present bias.

Weber and Chapman (2005) confirm these findings, again with hypothetical monetary stakes. Baucells and Heukamp (2010) also find that risk moderates present bias with highly-diluted monetary incentives, implementing only three of 3,757 decisions.[3]

However the methodologies employed may not appropriately identify the effect of interest. Hypothetical decisions lack incentive, relying solely on framing and contingent reasoning. Similarly, extremely low probabilities of implementation may dilute the stated probability of the prospects due to isolation failure (which I discuss in section 2.4). Finally, monetary earnings do not necessarily translate to consumption as in the intertemporal models of equations (1) and (2); an individual would need to be extremely liquidity-constrained to trade a few dollars of earnings for a good to be consumed the same day.

Models of present bias are often used to study self-control failure of visceral urges (Cheung, Tymula, and Wang 2025), which are plausibly best elicited with an immediate and certain consequence. For example, many studies of sequential games find costly punishment more prevalent upon eliciting a direct-response action instead of a conditional strategy (Brandts and Charness 2011), perhaps due to a preference for exacting *unconditional* (*i.e.*, certain) revenge.

My study is the first to use truly immediate and certain consequences in studying present bias. I avoid concerns associated with hypothetical decisions, long-shot implementation,

[3]Three of 221 subjects are selected, each of whom has one of their seventeen decisions implemented.

and monetary stakes, thereby establishing an ideal method for capturing present bias.

2.3 Empirical estimates of present bias

While many studies have used monetary rewards to measure present bias, Andreoni and Sprenger (2012) pioneered the "convex time budget" (CTB) methodology, which elicits monetary-prize allocations between two time periods at various interest rates, thereby allowing risk preferences and QHD parameters to be estimated jointly.[4] However, monetary earnings may not adequately capture consumption utility in the absence of liquidity constraints and decision isolation. Augenblick, Niederle, and Sprenger (2015) address this concern with CTB decisions in which individuals allocate real-effort tasks across time (other studies have used alternative primary rewards, such as food).

The meta-analysis by Imai, Rutter, and Camerer (2021) finds no evidence of present bias in monetary rewards, while finding a mean bias-corrected present-bias factor β between 0.90 and 0.99 in real-effort tasks. While the specific value depends on the particular bias correction, present bias is also highly context-dependent.[5]

2.4 Decision framing, isolation, and implementation

A typical subject in an economics experiment makes many decisions. Historically many experiments implement many or all decisions, but this method can yield data rife with wealth effects, hedging, and other confounds (Charness, Gneezy, and Halladay 2016). Consequently most experiments now implement one randomly-selected decision, thus avoiding such

[4]See Cheung, Tymula, and Wang (2025) for a nice review of estimates of present bias using various methodologies. For a comparison of these other methodologies used to elicit time and risk preferences, see Andreoni, Kuhn, and Sprenger (2015). Further, see Andersen et al. (2008), Cheung (2016, 2020), and Abdellaoui et al. (2013).

[5]While Imai, Rutter, and Camerer (2021) restrict their focus to twenty-eight studies that use the CTB methodology, Cheung, Tymula, and Wang (2025) offer a meta-analysis which includes studies that use other methodologies such as the joint-elicitation methodology (Andersen et al. 2008).

complementarity between outcomes (Azrieli, Chambers, and Healy 2018). Yet implementing one decision at random is not a panacea; many subjects still fail to isolate each decision.[6]

Non-expected utility rationalizes isolation failure when a subject views a set of decisions as comprising a single optimization problem. For example, given a Holt and Laury (2002) choice list, a subject is often able to secure a certain outcome by choosing every safe option. Freeman, Halevy, and Kneeland (2019) find evidence of the certainty effect when comparing pairwise choices to choice-list data. Freeman and Mayraz (2019) find evidence of the Allais paradox with the effect independent of the mechanism used.

Freeman and Mayraz (2019) find that presentation has the largest impact on isolation. Brown and Healy (2018) display decisions separately and reclaim incentive compatibility.

3 Experimental design

The present study compares present bias between a risky consequence and a certain consequence. I implement one decision to avoid complementarity (see section 2.4), which must be implemented with certainty in the respective treatment. To obtain useful choice data from a single implemented decision, I use the CTB methodology (see section 2.3). To induce an immediate (primary) reward, I ask subjects to allocate a budget of real-effort tasks between two weeks.

The experiment consists of three sessions: Monday (day zero), Wednesday (day two), and the following Wednesday (day nine). Every subject earns $1.50 per session, which must be completed between noon and midnight. I immediately disqualify subjects who miss a deadline. Every subject that completes all three sessions earns a $5 bonus.

Each session begins with ten mandatory tasks, providing salient experience and a fixed

[6]See Starmer and Sugden (1991), Beattie and Loomes (1997), Cubitt, Starmer, and Sugden (1998), and Cox, Sadiraj, and Schmidt (2015).

baseline effort-level on each day. Each task asks the subject to count the number of zero digits in a sixteen-digit binary string and enter this count into an adjacent text field (figure 1a). The subject must remedy any incorrect response for successful submission.

Following the mandatory tasks on day zero and day two, each subject chooses an allocation of 360 tasks between day two and day nine at each of five substitution rates (the 360-task budget has day-two value). Each decision is presented individually to elicit a tentative choice (figure 1b), which are then juxtaposed on a single page (figure 1c) for any adjustment.

Accordingly, let $e^{t}_{i,d}$ denote effort chosen at rate R_i on decision-day d to be expended on workday t. For example, $e^{\text{day two}}_{4,0}$ is the effort chosen at rate $R_4 = 1.5$ on day zero to be worked on day two. Each subject faces the constraint

$$e^{\text{day two}}_{i,d} + R_i e^{\text{day nine}}_{i,d} = 360, \text{ for each } R_i \in \mathcal{R} \coloneqq \langle 1.25, 0.75, 1, 1.5, 0.5 \rangle \text{ and } d \in \{0, 2\}\,.$$

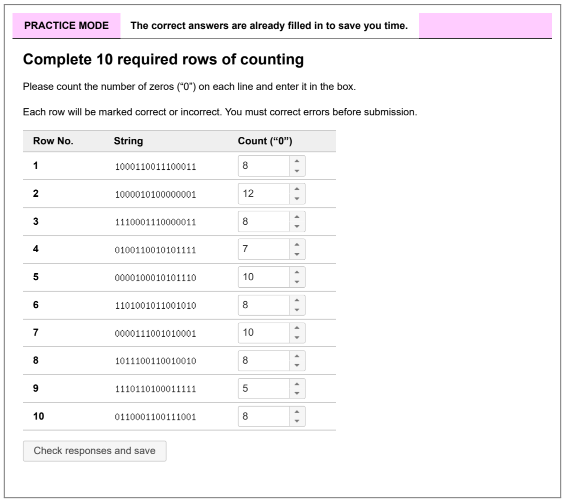
Some rates R_i (which can also be interpreted as productivity ratios or gross interest rates) entail substantial income effects. For example, at $R_4 = 1.5$, a subject may choose to delay all 360 tasks on day two to only 240 tasks on day nine. Yet if a subject delays all 360 tasks at $R_5 = 0.5$, she would need to complete 720 tasks on day nine.

$\mathcal{R}$ gives the ordered sequence of the separated decisions, with randomly-selected subjects receiving the reverse. The juxtaposed presentation always sorts decisions by $R_i \in \mathcal{R}$ ascending.

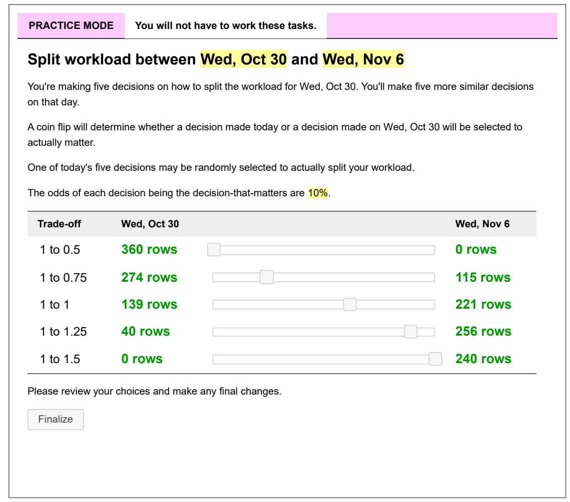
3.1 Treatments

I implement a 2 × 2 factorial between-subject design. I inform all subjects that a decision from either day zero or day two will be selected with equal probability (figure 1d).

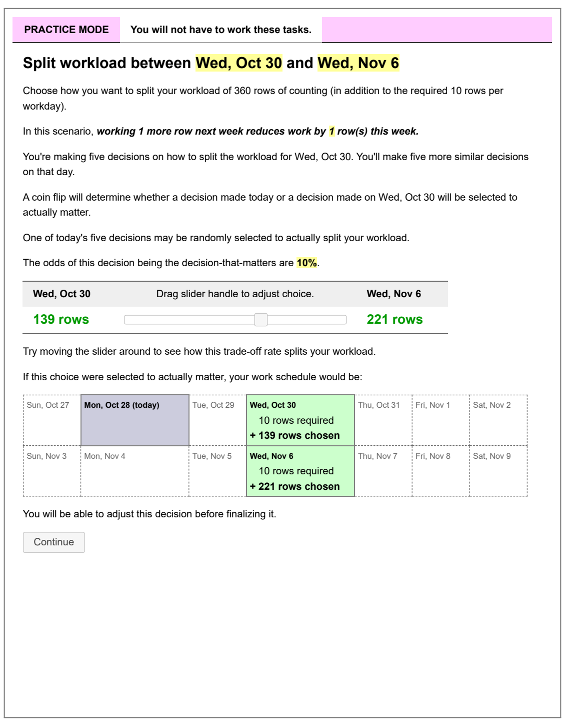
The *baseline treatment* implements one of the ten decisions with uniform probability. Subjects with this treatment have uncertainty regarding the day and the rate selected. Each



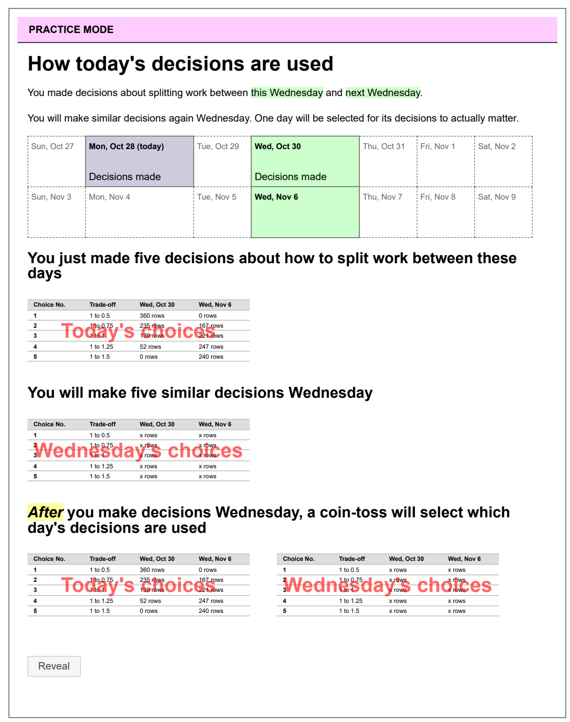
(a) Task interface



(c) Allocation interface, presented juxtaposed



(b) Allocation interface, presented separately



(d) Day selection mechanism interface

Figure 1: Experimental interface

Table 1: Probability of decision implementation

Treatment	Decision chosen on day $d = 0$	Decision chosen on day $d = 2$
Baseline	1/10	1/10
Certain Day	1/10	1/5
Certain Rate	1/2	1/2
Certain Rate, Certain Day	1/2	1

Note: Probabilities of implementation of the effort allocation choice $e_{2,d}$ (chosen on decision-day d at rate $R_1 = 1.25$).

decision in this *risky rate, risky day* treatment thus has a 10% implementation probability.

The *Certain Rate (CR)* treatment dimension eliminates risk regarding the rate to be implemented. In this treatment, subjects are informed that $R_1 = 1.25$ will certainly be implemented; decisions for all prices $R_i \neq 1.25$ are hypothetical, which I exclude from my analysis.

The *Certain Day (CD)* treatment dimension eliminates risk regarding the day from which a decision is selected. I inform subjects in this treatment that I will reveal the randomly-selected day *before* their day-two decisions. Accordingly, the day to be implemented is risky for *all* subjects on day zero, but certain for subjects with CD treatment on day two. Half of the subjects with this treatment learn that their day-two decisions are hypothetical; I exclude all hypothetical decisions from analysis and compensate by doubling the sample size of the CD dimension.[7]

Table 1 shows decision implementation probability by treatment cell T.

My primary interest is the interaction of CR and CD treatments. On day zero subjects know that their choice at $R_1 = 1.25$ made on either day zero or made on day two will be selected with certainty. Then on day two, prior to making a decision, subjects learn from

[7] I considered this alternate design: If day zero is selected, inform after day-two decisions; if day two is selected, inform before day-two decisions. I rejected this design because subjects would lack complete prior information about the timing of the resolution of risk.

which day a decision will be selected. Subjects who learn day two is selected thus make a decision on day two that is certainly implemented. That is, these subjects choose their impending same-day effort level, knowing this choice will be implemented with certainty, with the tasks due imminently. I hypothesize that present bias is more pronounced under certainty than under risk.

3.2 Interface

I carefully designed the interface to bolster subjects' understanding of the choice process and the implementation mechanism. The interface guides every subject through a complete practice round at the beginning of each session before making consequential decisions.

Day selection The interface shows each subject a list of their practice choices made on each decision-day (figure 1d). Upon clicking the button to proceed, the page visualizes random selection between the two decision-days by alternately highlighting the lists in quick succession before the highlight settles on one day as being selected. Every subject simulates two practice coin-flips: the first trial selects the alternate decision-day, then the second selects the present day. The remainder of the practice round uses the choices made in the present session.

Rate selection The interface next shows each subject the five practice choices made on the present day, arranged in a table similar to the juxtaposed allocation page (figure 1c). Subjects with CR treatment see row four (corresponding to $R_1 = 1.25$) permanently highlighted and a reminder that only choices at this rate will be implemented. Other subjects see no highlight at first, but upon clicking the button to proceed, a roulette-wheel sequence highlights each row quickly in succession. After traversing the table twice, the highlight settles on a randomly-selected decision.

With a practice allocation selected, subjects view a practice task interface that requests the corresponding amount of work to be completed on the present day. Subjects then exit the practice round and begin an identical sequence with consequential decisions and corresponding tasks.

4 Model and methodology

Assuming quasi-hyperbolic discounting, within-day power-function effort costs, and background effort of ω, on each decision-day $d = 0$ and $d = 2$ the decision-maker optimizes

$$\min_{e^t_{i,d}} \beta^{\mathbb{1}(d=0)} \left(e^2_{i,d} + \omega\right)^\alpha + \beta\delta^7 \left(e^9_{i,d} + \omega\right)^\alpha, \text{ subject to } e^2_i + R_i e^9_i = 360, \tag{3}$$

choosing effort $e^t_{i,d}$ at $R_i \in \mathcal{R} \coloneqq \langle 1.25, 0.75, 1, 1.5, 0.5 \rangle$ for workday $t \in \{2, 9\}$. This model uses δ as a daily discount factor, while β discounts future-day effort. Following Lawrance (1991), the first-order condition implies an intertemporal Euler equation of

$$\left(\frac{e^2_{i,d} + \omega}{e^9_{i,d} + \omega}\right)^{\alpha-1} = \frac{\beta^{\mathbb{1}(d=2)}\delta^7}{R_i}. \tag{4}$$

Logarithms linearize this equation as

$$\underbrace{\ln \frac{e^2_{i,d} + \omega}{e^9_{i,d} + \omega}}_{=:E_{i,d}} = \underbrace{\frac{\ln \delta}{\alpha - 1}}_{=:\theta_{\text{delay}}} 7 + \underbrace{\frac{-1}{\alpha - 1}}_{=:\theta_{\text{lnrate}}} \ln R_i + \underbrace{\frac{\ln \beta}{\alpha - 1}}_{=:\theta_{\text{present}}} \underbrace{\mathbb{1}(d = 2)}_{=:\mathbb{1}(\text{pr})}. \tag{5}$$

Let us define the variables as shown under braces in equation (5) to simplify notation. An additive error term produces an estimatable reduced-form, with s indexing subjects:

$$E_{i,d,s} = \theta_{\text{delay}}7 + \theta_{\text{lnrate}} \ln R_i + \theta_{\text{present}} \mathbb{1}(\text{pr})_d + \varepsilon_{i,d,s}. \tag{6}$$

Non-linear transformations then recover estimates of β, δ, and α.

Each subject allocates 360 tasks (in day-two valuation) between day two and day nine at various price ratios. Each subject must also complete ten mandatory real-effort tasks on each day. A subject might most prefer a negative effort allocation to a workday (that is, a net gain of leisure on that day), but the environment constrains the day-two effort choice such that $e^2_{i,d} \in [0, 360]$ for all rates R_i on each decision-day d. A two-limit Tobit model accommodates this censoring at corner solutions.

To estimate the model with a power cost function $c(e) \coloneqq (e + \omega)^\alpha$, we must specify some background effort $\omega > 0$. The primary analysis will use $\omega = 10$ as the background effort, corresponding to the mandatory daily tasks. Subjects may perform other tasks throughout the day that we might wish to include in ω; we will find that all qualitative results are robust to many hours of background effort (see supplement section 1).

4.1 Identification

The parameters of interest are identified through variation within and between subjects and treatments.

4.1.1 Present bias

The experiment identifies the present-bias factor β with a two-day window. When making effort allocation decisions on Monday, day zero, the subject views both workdays (Wednesday, day two, and the next Wednesday, day nine) as part of the future. Then on the first workday, when making similar decisions, the subject views that same day as the present and the second workday as being in the future. This variation in decision timing identifies β.[8] For example, a present-biased subject will choose less work for day two when it is the present day.

4.1.2 Discounting and effort-cost curvature

Variation in the intertemporal price ratio, R_i, jointly identifies the daily discount factor, δ, and effort-cost curvature, α, regardless of decision timing. Either a larger δ (greater patience) or a larger α (greater desire to smooth effort across days) results in a greater allocation of tasks to the earlier workday, all else equal. Treatment with CR removes variation in R_i, creating an identification challenge.

4.1.3 Between-treatment identification

To estimate a separate present-bias factor β_T for each treatment cell T while properly identifying δ and α, I employ a two-step estimation procedure. This approach uses the variation in R_i in the baseline treatment to identify δ and α, then applies those estimates to each treatment cell T to identify β_T.

Step 1: I estimate the model in equation (6) using only data from the baseline (Risky Rate, Risky Day) treatment. This step yields initial estimates for the common parameters $\hat{\alpha}$ and $\hat{\delta}$, as well as $\hat{\beta}$ for the baseline cell.

Step 2: I use the estimates $\hat{\alpha}$ and $\hat{\delta}$ from the first step to estimate β_T for each of the three non-baseline treatment cells, CR, CD, and CR–CD. To account for the use of estimated regressors, I report Murphy–Topel (1985) standard errors.

[8] While within-subject estimation of the present-bias factor is possible, each point-estimate β_s would rely on only two observations, $e_{1,0}$ and $e_{1,2}$, per subject s given CR treatment.

This two-step method is preferable to a single pooled (one-step) regression, which would produce biased estimates of the common parameters δ and α due to imbalance in the identifying variation. A pooled model would suffer from a severe imbalance across price ratios. Because the CR treatments only contain observations at $R_1 = 1.25$, a pooled estimation gives this price ratio undue weight, which adds bias to the estimates identified by this variation.

The two-step procedure provides a clean solution to this identification problem: it first uses only the baseline treatment data—which is balanced across all price ratios and holds constant all procedures—to obtain unbiased estimates of δ and α. The second step then uses these cleanly estimated parameters to identify β_T for each of the other treatment cells. Supplement subsection 1.1 details the alternative pooled (one-step) model with similar qualitative results.

Despite my use of a full-factorial design, other researchers can evaluate present bias under certainty using only the baseline treatment and the CR–CD treatment. I implement a full-factorial design in the present study for a thorough investigation.

4.2 Hypotheses

My hypothesis is that an interaction exists between the immediacy effect and the certainty effect. That is, present bias at certainty differs from present bias with risk. The baseline treatment with risk regarding the rate and risk regarding the decision-day is standard in the literature, here with each decision having an implementation probability of 1/10. When treated with Certain Rate and Certain Day, the day-two choice for $R_1 = 1.25$ is implemented with certainty (probability of one). Accordingly, I hypothesize that the present-bias factor differs between the baseline treatment and the CR–CD treatment.

Hypothesis 1 *Present-bias is more intense under implementation certainty (with both CR and CD treatment) than when the decision involves both types of risk (decision-day and rate both* *unrealized), in which each decision has an implementation probability of* 1/10*:* $\beta_{cr,cd} < \beta$*.*

For completeness I further hypothesize that present bias at certainty differs from present bias with any uncertainty—that is, with only one dimension of risk.

Hypothesis 2 *Present-bias is more intense under implementation certainty than when the decision involves rate risk but has decision-day certainty (implementation probability of* 1/5*):* $\beta_{cr,cd} < \beta_{cd}$*.*

Hypothesis 3 *Present-bias is more intense under implementation certainty than when the decision involves decision-day risk but has rate certainty (implementation probability of* 1/2*):* $\beta_{cr,cd} < \beta_{cr}$*.*

I do not hypothesize further how the type of risk may matter. For example, controlling for the implementation probability, perhaps risk regarding the rate is most influential, perhaps driven by the income effect of the price ratios. Research regarding types of risk and underlying mechanisms is left to future research.

5 Results

I recruited subjects from an online piece-rate labor marketplace with an equivalent median hourly wage under $5 and jobs that commonly involve transcribing invoices or tagging photographs (Newman 2019).[9] I described my "multi-day counting project" as three sessions with a combined 30–50 minutes of tasks which paid $1.50 for each session and a $5 bonus for completing all three. Given that the experiment involves tasks similar to those typical of the marketplace, it is a *framed field experiment* (Harrison and List 2004).

[9] On Amazon Mechanical Turk I sampled residents of the United States and Canada with at least 1,000 jobs completed with 98% approval.

My instructions provided sample tasks, explained the task allocation process, and stressed the three dates of participation: Monday 28 October 2019 (day zero), Wednesday 30 October (day two), and Wednesday 6 November (day nine). Consenting subjects answered an eight-question comprehension survey which paid $1.50 regardless of the responses. Supplement section 3 provides all experimental instruments.

Of the 389 comprehension survey submissions, 220 provided informed consent and only correct responses to the comprehension survey; I enrolled these subjects in my experiment. Of these 220 subjects, 206 (93.6%) enrolled in and completed day-zero decisions.[10] From the first session to the last, attrition was only 26 of 206 subjects (12.6%).[11] The median subject completed a total of 340 tasks in 36 minutes (with quartiles of 28 and 47 minutes).

5.1 Descriptive results

While log-effort-ratio is the correctly specified choice variable given the model in equation (3), let us consider a more intuitive outcome: day-two effort-share $\varphi \coloneqq e^2_{i,d}/360$. Because the CR treatment only incentivizes $R_1 = 1.25$, I only analyze choice data at this rate. Note that because day-nine effort is more productive than day-two effort at this rate, effort is split evenly between workdays when $e^2_{2,d} = e^9_{2,d} = 160$ and thus $\varphi = 160/360 = 0.44$.

Figure 2 offers histograms of effort-share choices by treatment. The solid bars depict the distribution of choices made in advance of the first workday (on day zero), and the outlined bars depict choices made on the first workday (day two). Given a day-two effort-share choice made on day zero, a smaller choice on day two indicates present bias. Thus this graph illustrates present bias when the outlined bars shift to the left of the solid bars.

[10] My pre-registration sought 192–208 subjects (Reddinger 2019).

[11] On day two, 192 subjects (93.2%) returned and completed that day's decisions. One of each subject's ten decisions was implemented, upon which 188 subjects (97.9%) completed the selected day-two effort. Finally, on day nine, 180 subjects (95.7%) returned and completed the session, thus earning the completion bonus. Table S.5 in supplement section 2 provides all subject counts by day and treatment.

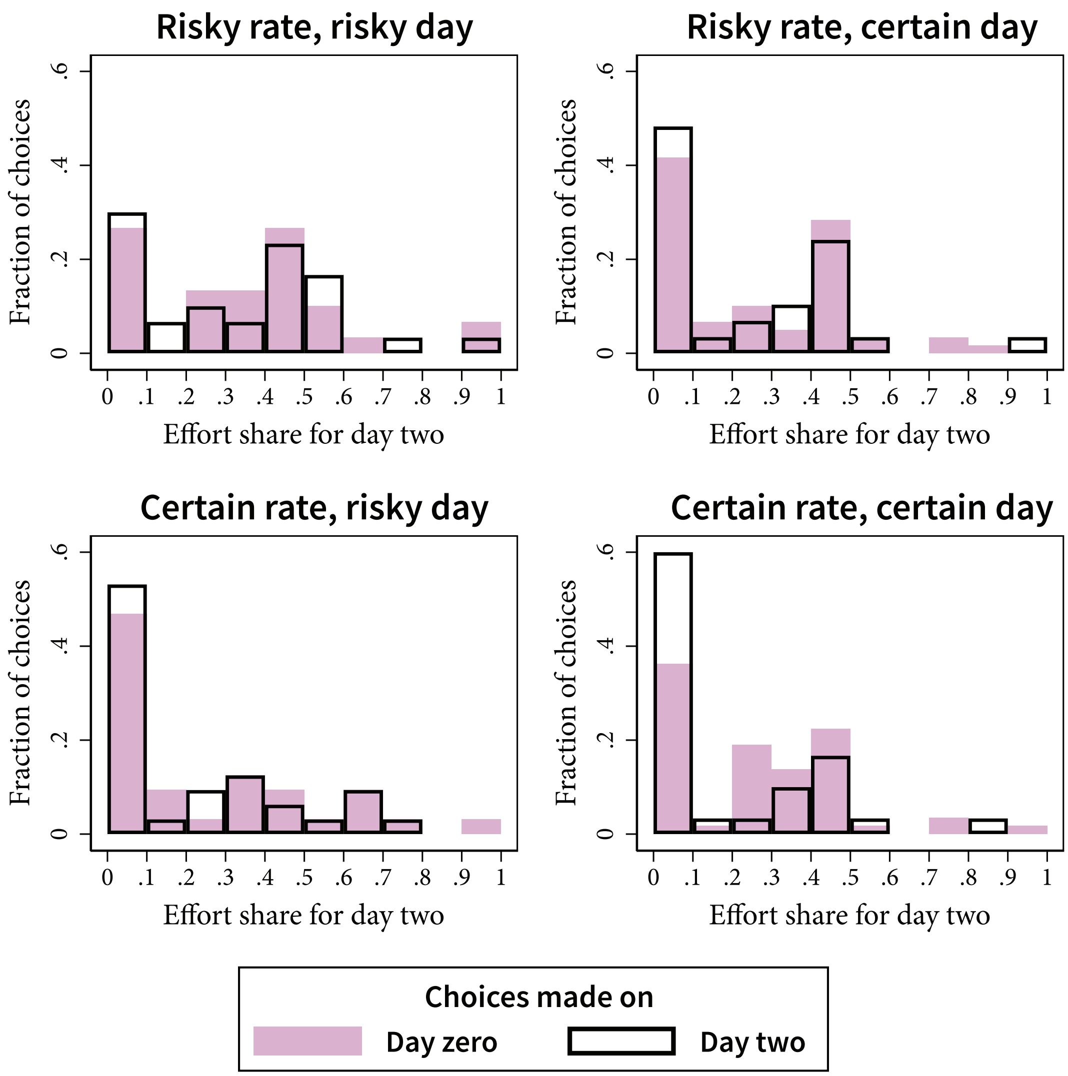


Figure 2: Histograms of effort-share chosen for day two at $R_1 = 1.25$ for each treatment

The histogram for the CR–CD treatment reveals a striking difference between day-zero and day-two choices. On day two, with work imminent, many more subjects choose to exert 0–0.1 of effort on that day than had chosen such an allocation in advance. Recall that this delay comes at a non-trivial cost, as 25% more work must then be performed.

Let us compare the filled bar against the outlined bar for the 0–0.1 bin in each treatment. We find a similar pattern that seems to diminish as certainty is eliminated in either dimension. The baseline treatment exhibits the smallest difference in the 0–0.1 bin. Pairwise comparisons suggest that the addition of each dimension of certainty may increase present bias. They also suggest that the move to complete certainty (CR–CD treatment) results in an especially substantial shift to present-biased choices.

The histogram of the CR–CD treatment clearly illustrates the potential for a substantial treatment effect in contrast to the histograms of the other treatment cells. We next turn to regression analysis to make use of all incentivized choice data.

5.2 Regression results

We now consider the regression results as presented in table 2 and figure 3.

With a point estimate of $\hat{\beta} = 0.895$, the baseline treatment nicely replicates estimates of present bias in real effort from the literature. Augenblick, Niederle, and Sprenger (2015) estimate $\hat{\beta} = 0.89$ in their main experiment and in their replication. Imai, Rutter, and Camerer (2021) provide a meta-analytic estimate of 0.89 in real effort. Cheung, Tymula, and Wang (2025) find a meta-analytic estimate of 0.82 in real effort, which includes all treatments of the present study as one of the twelve real-effort studies that they analyze.

Now we turn to our treatment effects of interest. When treated with both dimensions of certainty (CR and CD), subjects exhibit much greater present bias, with a point-estimate of $\hat{\beta}_{\text{cr,cd}} = 0.281$. Present bias under certainty is statistically different from the baseline

Table 2: Two-step regression results

Param.	Estim.	Std. err.	p-value of χ^2_1 test that parameter equals: 1	β	β_{cd}	β_{cr}
$\beta_{cr,cd}$	0.281	(0.190)	< 0.001	< 0.001	0.002	0.726
β_{cr}	0.336	(0.213)	0.002	0.003	0.016	
β_{cd}	0.775	(0.128)	0.078	0.293		
β	0.895	(0.058)	0.070			
δ	1.008	(0.108)	0.939			
α	1.448	(0.159)	0.005			

Note: 897 observations (161 left- and 95 right-censored) from 180 subjects. Robust standard errors in parentheses are clustered on subject using a two-limit Tobit model. Estimates of β_{cd}, β_{cr}, and $\beta_{cr,cd}$ are recovered using point estimates from a first-step regression of the baseline treatment (β, α, δ) and use Murphy–Topel two-step standard errors. Excludes hypothetical decisions and subjects who did not complete all sessions. Background effort $\omega = 10$. Wald test of the null hypothesis that all β_T are equal gives $p = 0.007$.

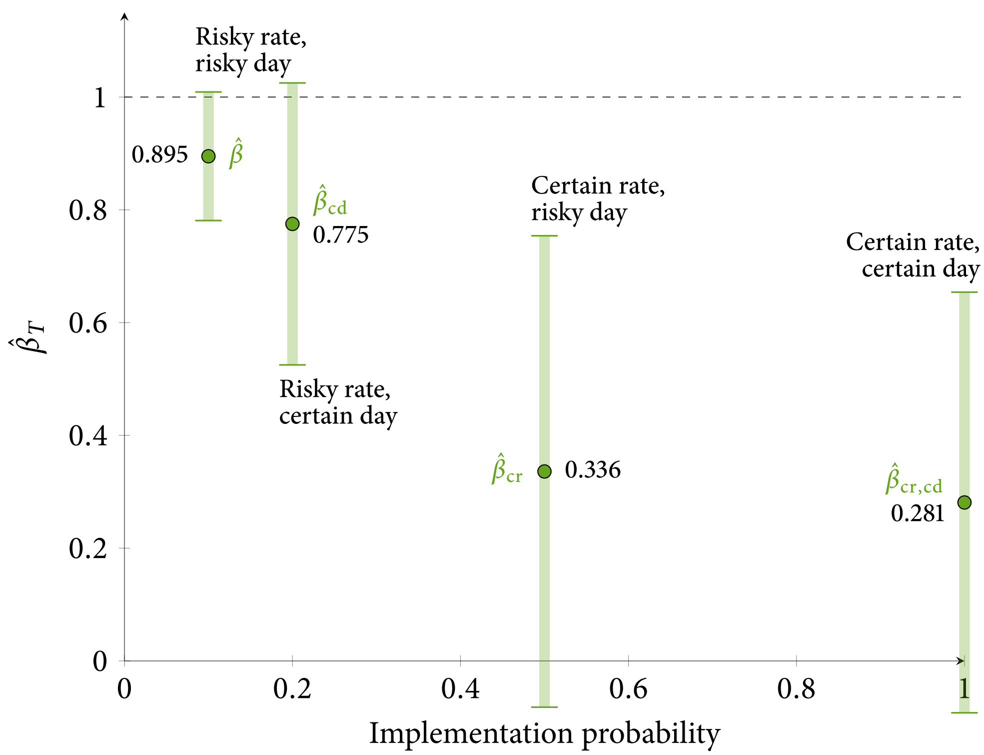


Figure 3: Two-step regression estimates of β_T

treatment with risk and from unity (both χ_1^2 tests with $p < 0.001$). This provides clear evidence that the introduction of a substantial amount of risk significantly moderates present bias. Under certainty, subjects value the present 3.6 times as much as they value the future, while in the baseline treatment, subjects value the present at 1.12 times the future.

Regarding intermediate levels of certainty, a Wald test rejects equality of β across the four treatments with $p = 0.007$. Figure 3 depicts a present-bias factor estimates that are decreasing in implementation probability.

Recall that with CR treatment, we rely on only two observations per subject; otherwise we have ten observations per subject. Indeed, the two CR treatments have standard errors (SEs) that are roughly double the SE of the CD treatment. The CD treatment has, in turn, roughly double the SE of the baseline treatment; we expect this, given our use of Murphy–Topel (1985) SEs that propagate estimation error from the first-step baseline treatment.

In total, the experimental results conclusively demonstrate an economically and statistically significant difference between the baseline treatment with risk and the treatment with certainty.

5.3 Additional considerations

Attrition bias

While sample attrition (12.6%) was remarkably low for an online experiment across ten days, we should look for evidence of selective attrition. For example, Certain Rate treatment might have lower attrition as it guarantees an income effect, whereas the Risky Rate treatment does not in expectation.

Only four subjects completed day-two decisions but did not complete the implemented day-two effort level. Two of these made only hypothetical decisions on day two and are thus excluded from the analysis. Both remaining subjects had Certain Rate treatment. We conclude that attrition during day two was orthogonal to rate resolution.

Attrition between day two and day nine of eight subjects was highly balanced across treatments and rate selection.

Attenuation bias

To the extent that a two-day delay does not capture all present-bias, we obtain a conservative estimate of the treatment effect. One could reasonably argue that Monday and Wednesday of the same week may both feel relatively present, while the following week may feel relatively distant. This would imply that present bias would be better identified from a week-long delay, as in Augenblick, Niederle, and Sprenger (2015). However, this is an empirical question, and Augenblick (2018) studies exactly how present bias varies with short delays. Using similar real-effort tasks, he finds that present bias quickly diminishes within three days, with two days capturing most present bias. In the present study, the use of a two-day window will yield conservative estimates of β (biased upward). This in turn compresses the observable treatment effect of the present experiment. A week-long delay may provide greater statistical power, but likely at the cost of greater attrition.

Framing effects

The CR treatment retained decisions at hypothetical price ratios; it simply explained that the highlighted decision would be implemented with the given probability while the other decisions would implemented with zero probability. I chose this design to hold framing constant, as well as complexity, cognitive costs of decision-making, and other such potential confounds. By holding the framing constant, we likely capture a conservative estimate of the treatment effect. If we were to strip away the hypothetical decisions, we would expect a reduction in perceived complexity and an increase in salience of the consequential decision. These effects would likely strengthen the treatment effect.

Effort-cost curvature

We reject the hypothesis that $\alpha \geq 1$ with $p < 0.001$, satisfying the second-order condition for equation (3). Indeed, of 897 choices, only 161 are left- and 95 right-censored. Moreover, qualitative results are robust to background effort ω of greater orders of magnitude, appropriate for having already worked many hours of similar tasks on the workdays (see supplement section 1).

6 Conclusion

This study of dynamic inconsistency in real-effort provision finds that risk diminishes the intensity of present bias. This includes uncertainty that arises from random-implementation mechanisms popular among experimental economists. My experiment varies the implementation mechanism, thereby altering the probability of decision implementation. The novel design permits estimation of the present-bias factor in each of four treatment cells, including one that implements a single decision with certainty.

The effect of certainty on present bias is striking. Under certainty I estimate substantial present bias with a point-estimate of $\hat{\beta}_{\text{cr,cd}} = 0.281$, while in the baseline treatment I find marginally-significant present bias of $\hat{\beta} = 0.891$. These results present a remarkable treatment effect: risk significantly moderates present bias.

Experiments that seek an accurate point-estimate of the present-bias factor should include a decision with substantial immediate and certain consequences. If complementarities between consequences do not pose a serious concern, the experiment might reasonably implement multiple such decisions.

My findings underscore the importance of unifying theories of time and risk, notably Chakraborty, Halevy, and Saito (2020) (see section 2.1 and footnote 2). Conversely, in testing decision-theoretic models, researchers should mind their incentive mechanisms and use decisions implemented with certainty when appropriate.

Empirical work on tempting goods may require decisions with salient and certain consequences, a potentially-critical design element for any study employing experimental methods. Such work might study models of self-control, the effectiveness of commitment devices, or any application that depends on present-biased preferences.

Uncertainty may interact with non-stationary time preferences, leading to different behavior in strategic interactions. For example, in labor contracts firms may exploit present bias with (un)certainty regarding compensation, effective productivity, or job duration.

Clearly the field of behavioral economics has much yet to learn about present bias, temptation, and related interventions. Continual improvement of experimental methodology will aid this pursuit.

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Temptation: Immediacy and certainty
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S.1 Robustness checks

S.1.1 One-step (pooled) estimation

As a robustness check, this section considers an alternate estimation strategy that simply pools all four treatments for estimation in a single step (as discussed in section 4). This approach can offer efficiency gains at the cost of point-estimate bias. Because the hypothesis tests in section 5.2 are well-powered, we have no need for smaller standard errors; I include these results only to further validate those results.

S.1.1.1 Model specification

Let $\mathbb{1}(\text{cr})$ and $\mathbb{1}(\text{cd})$ indicate CR and CD treatment respectively. We interact the full-factorial of these with the present-workday indicator, $\mathbb{1}(d = 2)$, to obtain an estimatable pooled reduced-form regression model:

$$\begin{aligned} E_{i,d,s} = {} & \theta_{\text{delay}} 7 + \theta_{\text{lnrate}} \ln R_i + \theta_{\text{present}} \mathbb{1}(d = 2)_d + \theta_{\text{cr}} \mathbb{1}(\text{cr})_s \mathbb{1}(d = 2)_d \\ & + \theta_{\text{cd}} \mathbb{1}(\text{cd})_s \mathbb{1}(d = 2)_d + \theta_{\text{cr,cd}} \mathbb{1}(\text{cr})_s \mathbb{1}(\text{cd})_s \mathbb{1}(d = 2)_d + \varepsilon_{i,d,s}. \end{aligned} \tag{7}$$

This specification allows recovery of β_T that varies by treatment cell T as

$$\beta = \exp \frac{\theta_{\text{present}}}{-\theta_{\text{lnrate}}}, \qquad \beta_{\text{cr}} = \exp \frac{\theta_{\text{present}} + \theta_{\text{cr}}}{-\theta_{\text{lnrate}}},$$

$$\beta_{\text{cd}} = \exp \frac{\theta_{\text{present}} + \theta_{\text{cd}}}{-\theta_{\text{lnrate}}}, \qquad \beta_{\text{cr,cd}} = \exp \frac{\theta_{\text{present}} + \theta_{\text{cr}} + \theta_{\text{cd}} + \theta_{\text{cr,cd}}}{-\theta_{\text{lnrate}}},$$

$$\delta = \exp \frac{\theta_{\text{delay}}}{-\theta_{\text{lnrate}}}, \text{ and} \qquad \alpha = 1 - \theta_{\text{lnrate}}^{-1}.$$

This model identifies δ and α from *all* observations, including treatment cells that are unbalanced by design. Recall that CR treatment only includes observations at $R_1 = 1.25$, while CD treatment doubles the subject count on day zero (to compensate for making half of the day-two decisions hypothetical). Use of these unbalanced cells *might* provide a gain in efficiency but almost surely adds bias to the estimates.

In contrast, recall that the two-step model first estimates the baseline model, then uses the first-step estimates of δ and α to estimate β_T for each of the three certainty treatments. The baseline treatment is the only balanced treatment cell; it cleanly and strongly identifies δ and α. We then use these more accurate estimates recover β_T for each of the three certainty treatments.

We expect larger standard errors (SEs) in the two-step procedure for two reasons. First, we are estimating α and δ from fewer observations (as we only use the baseline treatment), resulting in larger SEs. Second, we propagate the first-step SEs to the second-step to obtain Murphy–Topel (1985) SEs, inflating the SEs of the β_T estimates.

In summary, we expect biased estimates and smaller standard errors with this pooled estimation procedure.

S.1.1.2 Results

Table S.3 and figure S.4 present these one-step regression results.

We find the same pattern of risk moderating present bias with the one-step model, only with all estimates of β_T biased upward. Importantly, the average treatment effect remains highly significant (χ^2_1 test, $p = 0.001$). Relative to the baseline treatment with risk, present bias under certainty is vastly different economically, with a point estimate of $\hat{\beta}_{\text{cr,cd}} = 0.58$ under certainty.

We fail to find evidence of significant present bias in the baseline treatment with the pooled approach (χ^2_1 test, $H_0 : \beta = 1$, $p = 0.873$), while the two-step finds marginally-significant present bias with a two-tailed test.

S.1.2 Various background effort levels

Table S.4 provides regression results for both models, illustrating that the qualitative results are robust to various levels of background effort for each model.

S.2 Additional results

Table S.5 provides longitudinal counts of subjects by treatment.

Table S.6 provides the estimated variance-covariance matrix for the two-step estimation results in section 5.2, while table S.7 provides the same for the one-step (pooled) regression in supplement subsection 1.1.

S.3 Experiment details and instruments

Table S.8 summarizes the experimental timeline. Table S.9 provides the questions used in the comprehension test (recall that I enrolled only participants who answered all of these questions correctly).

The interface included animations to convey the random selection procedure to subjects. Only one slide from each animation is included in this document.

- Figures S.5 to S.9: Qualification session instructions
- Figures S.10 to S.12: Day zero session instructions
- Figure S.13: Required tasks
- Figure S.14: Task allocation, separate
- Figure S.15: Task allocation, juxtaposed
- Figure S.16: Decision-day selection
- Figure S.17: Rate selection
- Figure S.18: Example of implemented tasks

Only tables and figures follow.

Table S.3: One-step (pooled) and two-step regression results

	One-step (pooled)			Two-step		
Param.	Estim.	Std. err.	p-value†	Estim.	Std. err.	p-value†
β	1.009	(0.055)	0.873	0.895	(0.058)	0.070
β_{cr}	0.921	(0.057)	0.166	0.775	(0.128)	0.078
β_{cd}	0.679	(0.109)	0.003	0.336	(0.213)	0.002
$\beta_{cr,cd}$	0.581	(0.108)	< 0.001	0.281	(0.190)	< 0.001
δ	0.986	(0.004)	< 0.001	1.008	(0.108)	0.939
α	1.282	(0.045)	< 0.001	1.448	(0.159)	0.005

Note: 897 observations (161 left- and 95 right-censored) from 180 subjects. Robust standard errors in parentheses are clustered on subject using a two-limit Tobit model. Excludes hypothetical decisions and subjects who did not complete all sessions.
† p-values for H_0: parameter = 1.

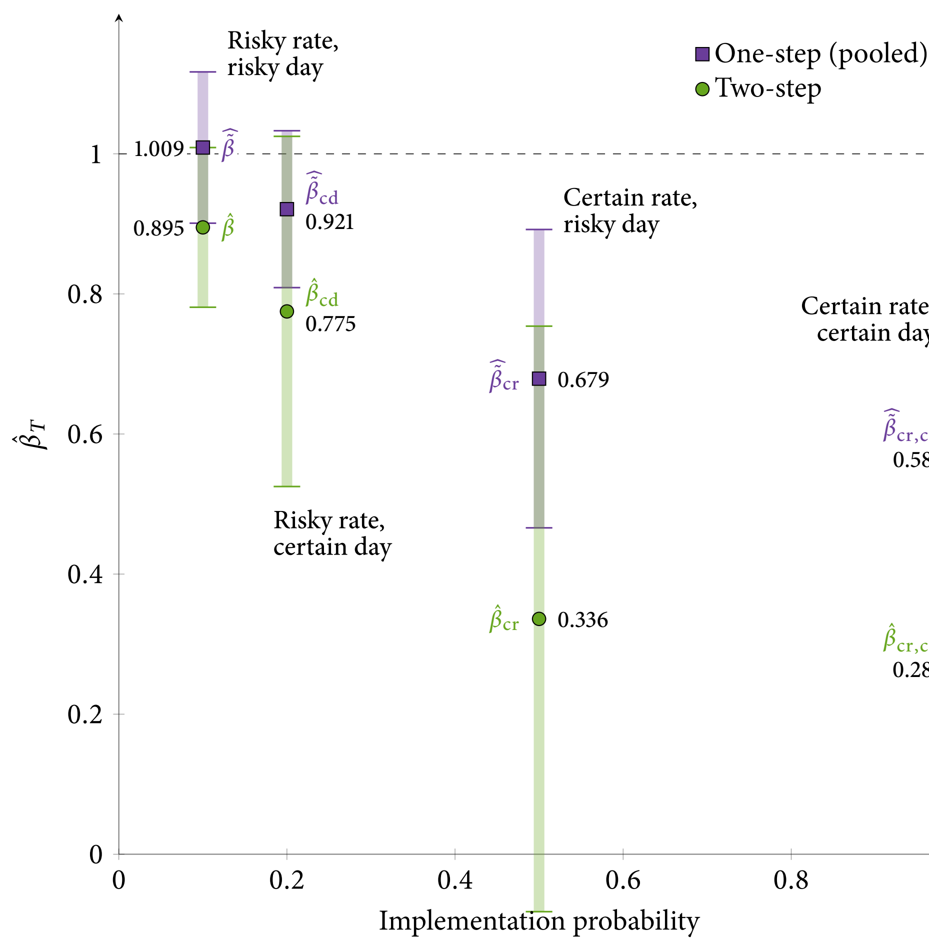

Figure S.4: One-step (pooled) and two-step regression estimates of β_T

Table S.4: Regression results with various background effort

	One-step (pooled)			Two-step		
	(1)	(2)	(3)	(4)	(5)	(6)
Background effort, ω	10	1200	4800	10	1200	4800
Median duration of $\omega^{\dagger}$	1 minute	2 hours	8 hours	1 minute	2 hours	4 hours
β	1.009	0.984	0.981	0.895	0.831	0.826
	(0.055)	(0.068)	(0.069)	(0.058)	(0.095)	(0.099)
β_{cd}	0.921	0.884	0.880	0.775	0.701	0.693
	(0.057)	(0.072)	(0.075)	(0.128)	(0.150)	(0.153)
β_{cr}	0.679	0.665	0.661	0.336	0.327	0.324
	(0.109)	(0.113)	(0.114)	(0.213)	(0.215)	(0.217)
$\beta_{\text{cr,cd}}$	0.581	0.569	0.565	0.281	0.280	0.277
	(0.108)	(0.113)	(0.114)	(0.190)	(0.198)	(0.200)
δ	0.986	0.987	0.987	1.008	1.059	1.063
	(0.004)	(0.005)	(0.005)	(0.108)	(0.132)	(0.136)
α	1.282	5.048	15.898	1.448	7.495	25.002
	(0.045)	(0.688)	(2.575)	(0.159)	(2.506)	(9.443)
p-value of χ^2_1 test:						
$\beta = \beta_{\text{cd}}$	0.327	0.366	0.371	0.293	0.317	0.319
$\beta = \beta_{\text{cr}}$	0.011	0.019	0.020	0.003	0.003	0.003
$\beta = \beta_{\text{cr,cd}}$	0.001	0.002	0.002	< 0.001	< 0.001	< 0.001
$\beta_{\text{cd}} = \beta_{\text{cr}}$	0.046	0.084	0.087	0.016	0.038	0.042
$\beta_{\text{cr,cd}} = \beta_{\text{cd}}$	0.005	0.012	0.012	0.002	0.010	0.012
$\beta_{\text{cr,cd}} = \beta_{\text{cr}}$	0.458	0.481	0.481	0.726	0.752	0.752
$\beta = 1$	0.873	0.808	0.788	0.070	0.077	0.078
$\beta_{\text{cd}} = 1$	0.166	0.109	0.108	0.078	0.046	0.045
$\beta_{\text{cr}} = 1$	0.003	0.003	0.003	0.002	0.002	0.002
$\beta_{\text{cr,cd}} = 1$	0.000	0.000	0.000	< 0.001	< 0.001	< 0.001
$\delta = 1$	0.001	0.014	0.019	0.939	0.654	0.642
$\alpha = 1$	0.000	0.000	0.000	0.005	0.010	0.011

Note: 897 observations (161 left- and 95 right-censored) from 180 subjects. Robust standard errors in parentheses are clustered on subject using a two-limit Tobit model. Excludes hypothetical decisions and subjects who did not complete all sessions. The *ex ante* specification uses background effort $\omega = 10$.
† The median duration of ten real-effort tasks is one minute.

Table S.5: Subject counts by treatment

Treatment					Subject count on day		
No.	Day	Rate	Selects day[1]	Rate order[2]	Zero	Two	Nine
1	Risky	Risky	Two	Original	8	8	8
2	Risky	Certain	Two	Original	10	10	10
3	Risky	Risky	Zero	Original	9	8	7
4	Risky	Certain	Zero	Original	8	7	7
5	Certain	Risky	Two	Original	17	16	15
6	Certain	Certain	Two	Original	17	16	15
7	Certain	Risky	Zero	Original	18	16	16
8	Certain	Certain	Zero	Original	15	15	13
9	Risky	Risky	Two	Reversed	9	8	7
10	Risky	Certain	Two	Reversed	8	8	7
11	Risky	Risky	Zero	Reversed	9	8	8
12	Risky	Certain	Zero	Reversed	9	9	8
13	Certain	Risky	Two	Reversed	17	15	14
14	Certain	Certain	Two	Reversed	17	16	15
15	Certain	Risky	Zero	Reversed	18	16	15
16	Certain	Certain	Zero	Reversed	17	16	15
Total subject count by day					206	192	180

Note: Certain Day treatment recruited twice as many subjects by design.
[1] The day from which a decision was selected for implementation. [2] Decisions are presented in the order of $\mathcal{R} \coloneqq \langle 1.25, 0.75, 1, 1.5, 0.5\rangle$ or its reverse.

Table S.6: Estimated covariance matrix for two-step estimation with $\omega = 10$

	β	β_{cd}	β_{cr}	$\beta_{cr,cd}$	δ	α
β	0.00339					
β_{cd}	0.00340	0.01629				
β_{cr}	0.00706	0.01408	0.04540			
$\beta_{cr,cd}$	0.00672	0.01326	0.02828	0.03628		
δ	−0.00197	−0.00972	−0.00800	−0.00739	0.01126	
α	−0.00619	−0.01006	−0.02617	−0.02501	0.00344	0.02449

Table S.7: Estimated covariance matrix for one-step regression with $\omega = 10$

	β	β_{cd}	β_{cr}	$\beta_{cr,cd}$	δ	α
β	0.00303					
β_{cd}	−0.00086	0.00325				
β_{cr}	−0.00099	0.00020	0.01183			
$\beta_{cr,cd}$	−0.00090	0.00027	0.00297	0.01173		
δ	0.00006	−0.00002	0.00011	0.00012	0.00002	
α	0.00014	−0.00044	−0.00244	−0.00263	−0.00008	0.00206

Table S.8: Experimental timeline

Day zero	“Qualification HIT”	Payment of $1.50 within twenty-four hours
	1. Instructions	
	2. Consent	
	3. Comprehension test	
A subject qualifies for the next session if and only if all comprehension answers are correct.		
Day zero	“Monday’s HIT”	Payment of $1.50 within twenty-four hours
	1. Instructions	
	2. *Practice:* Ten mandatory tasks that would need to be completed	
	3. *Practice:* Effort allocation between day two and day nine, presented separately	
	4. *Practice:* Effort allocation between day two and day nine, presented juxtaposed	
	5. *Practice:* How today’s decisions are used (resolution of decision-day risk)	
	6. *Practice:* How today’s decisions are used (resolution of rate risk)	
	7. *Practice:* View implemented tasks that would need to be completed	
	8. Complete the ten mandatory tasks	
	9. Effort allocation between day two and day nine, presented separately	
	10. Effort allocation between day two and day nine, presented juxtaposed	
A subject qualifies for the next session if and only if all parts of this session are completed.		
Day two	“This Wednesday’s HIT”	Payment of $1.50 within twenty-four hours
	1. Instructions	
	2. *Practice:* Ten mandatory tasks that would need to be completed	
	3. *Practice:* Effort allocation between day two and day nine, presented separately	
	4. *Practice:* Effort allocation between day two and day nine, presented juxtaposed	
	5. *Practice:* How today’s decisions are used (resolution of decision-day risk)	
	6. *Practice:* How today’s decisions are used (resolution of rate risk)	
	7. *Practice:* View implemented tasks that would need to be completed	
	8. Complete the ten mandatory tasks	
	9. *Certain Day treatment only:* One day is selected for implementation	
	10. Effort allocation between day two and day nine, presented separately	
	11. Effort allocation between day two and day nine, presented juxtaposed	
	12. *Risky Day treatment only:* One day is selected for implementation	
	13. *Risky Rate treatment only:* One rate is selected for implementation	
	14. Complete the implemented tasks for today	
A subject qualifies for the next session if and only if all parts of this session are completed.		
Day nine	“Next Wednesday’s HIT”	Payment of $6.50 within twenty-four hours
	1. Instructions	
	2. Complete the ten mandatory tasks	
	3. Complete the implemented tasks for today	

Note: In the labor marketplace used, *HIT* is common nomenclature for a single job.

Table S.9: Comprehension test

1.	On which of the following days do you plan on completing a HIT for this project? *One checkbox per date:* Sun, Oct 27; Mon, Oct 28; Tue, Oct 29; Wed, Oct 30; Thu, Oct 31; Fri, Nov 1; Sat, Nov 2; Sun, Nov 3; Mon, Nov 4; Tue, Nov 5; Wed, Nov 6; Thu, Nov 7; Fri, Nov 8; Sat, Nov 9
2.	After you receive your qualification today, will there be another HIT to complete today? *Radio buttons:* Yes; No
3.	What happens if you make counting errors in the task? *Radio buttons:* The HIT might be rejected. I must start the entire HIT over from the beginning. I will be told which rows have errors, then I'll correct the errors.
4.	Suppose you complete the first HIT later today and you also complete the HIT on this Wednesday. However, you do not complete the HIT next Wednesday. How much will you earn in total from this study? (Do not include earnings from this qualification HIT.) *Text input formatted as USD currency*
5.	How many HITs in total must you complete to earn the bonus? (Do not count this qualification HIT.) *Text input*
6.	On how many days total (including today) must you complete a HIT to earn the bonus? (Do not count this qualification HIT.) *Text input*
7.	How much will you earn in total by fully participating in this study, including the bonus? (Do not include earnings from this qualification HIT.) *Text input formatted as USD currency*
8.	Each day's HIT will definitely be available during what times? Specify a time range using *Pacific Time*: *Selection menu:* Beginning at: 07:00 am; 08:00 am; 09:00 am; 10:00 am; 11:00 am; 12:00 pm; 01:00 pm; 02:00 pm; 03:00 pm; 04:00 pm; 05:00 pm; 06:00 pm Ending at: 12:00 pm; 1:00 pm; 2:00 pm; 3:00 pm; 4:00 pm; 5:00 pm; 6:00 pm; 7:00 pm; 8:00 pm; 9:00 pm; 10:00 pm; 11:00 pm; 12:00 am (midnight); 01:00 am; 02:00 am

Note: In the labor marketplace used, *HIT* is common nomenclature for a single job.

Introduction

Hello! I'm [researcher name], a [researcher position] at [researcher institution].

I'm running this research project to complete my degree. Thank you for taking the time to read this!

I watch my email closely while my HITs are live, so feel free to email me at [researcher email].

I've tried my best to ensure that you are fairly compensated for your time. I pay all earnings within 24 hours.

If you complete the project, you are paid the same amount no matter what. The amount you have to work is partly determined by chance, but your decisions give you control over how much you work and when you work.

Absolutely no deception is used in this experiment. All random coin tosses were generated using a computer program that will be provided (along with the results) to the American Economic Association. My research will not be accepted if it is not completely transparent and truthful.

You may contact the [IRB] regarding your rights and participation at [IRB email].

Qualification HIT

- This HIT may qualify you for a sequence of 3 HITs for an academic research project.
- This qualification HIT pays $1.50, independently of any earnings described below.
- Please read these instructions carefully and answer the questionnaire at the bottom. Thank you!

What you may qualify for…

Earnings

- 1 more HIT today after you have been qualified; it pays $1.50
- 1 HIT this Wednesday; it pays $1.50
- 1 HIT next Wednesday; it pays $1.50
- Earn a $5 bonus for completing all 3 HITs

Today's HIT requirements

This HIT should only take 5-10 minutes to complete; it pays $1.50

1. Work 10 rows of the counting task (like the example below)
2. Decide how to split a workload of 360 rows of counting between Wednesday and next Wednesday

This Wednesday's HIT requirements

This HIT may take 10-20 minutes, depending on how you split the work between the days; it pays $1.50

1. Work 10 rows of the counting task (like the example below)
2. Decide how to split a workload of 360 rows of counting between Wednesday (that same day) and next Wednesday

Figure S.5: Qualification session instructions (1 of 6)

Summary

Earn $9.50 total (in addition to this $1.50 qual HIT) for about 30 to 40 minutes of work total, assuming you complete all 3 HITs.

Example: working 10 rows of the counting task

This is a completed example of the counting task that would be in each HIT.

Please count the number of zeros (“0”) on each line and enter it in the box.

Each row will be marked correct or incorrect. You must correct errors before submission.

Row No.	String	Count (“0”)
1	1000110011100011	8
2	1000010100000001	12
3	1110001110000011	8
4	0100110010101111	7
5	0000100010101110	10
6	1101001011001010	8
7	0000111001010001	10
8	1011100110010010	8
9	1110110100011111	5
10	0110001100111001	8

Check responses and save

Decide how to split a workload between this Wednesday and next Wednesday

You start with a workload of 360 rows of counting. You will decide how to split up the workload between this Wednesday and Wednesday of next week.

Figure S.6: Qualification session instructions (2 of 6)

You decide for different trade-off scenarios. For example:

- Working 1 more row next week reduces work by 1 row this week (a 1-to-1 trade-off)
- Working 1 more row next week reduces work by 1.5 rows this week (a 1-to-1.5 trade-off)
- Working 1 more row next week reduces work by 0.5 rows this week (a 1-to-0.5 trade-off)

One scenario will be selected to actually matter.

You will get more details and practice in the project's first HIT after you are qualified.

Complete each of 3 days' HITs for a $5 bonus, earning $9.50 total

- On each day in blue below, complete one HIT for $1.50.
- Once you complete all 3 HITs, you will be paid a $5 bonus.
- If you fail to complete a day's HIT, you will not be able to complete further HITs, nor will you receive the bonus.
- You will always be paid for HITs you have already completed. All earnings are paid within 24 hours.
- Each day's HIT will be available *at least* between 12 p.m. and 12 a.m. (midnight), Pacific Time.
- You will be qualified for today's HIT as quickly as possible, but it may take 30 minutes to an hour.
- You will receive a reminder notification through mTurk on each day that you have a HIT to complete.
- Please do not accept more than one HIT on the same day; it will not give you additional work or earnings.
- ***Please only participate if you think you can complete one HIT on each of these 3 days!***

Sun, Oct 27	**Mon, Oct 28 (today)** 1 HIT (today after qualified)	Tue, Oct 29	**Wed, Oct 30** 1 HIT	Thu, Oct 31	Fri, Nov 1	Sat, Nov 2
Sun, Nov 3	Mon, Nov 4	Tue, Nov 5	**Wed, Nov 6** 1 HIT	Thu, Nov 7	Fri, Nov 8	Sat, Nov 9

No deception is used in this experiment

This is an academic research project in the field of Economics, which widely prohibits the deception of experimental subjects.

All information and instructions provided to you in this experiment are truthful.

If you feel anything in this experiment is deceptive, please notify the [IRB] at [IRB email].

Consent

This is an academic research project to study work decisions involving delay and uncertainty.

You may choose to quit at any time. You will still receive earnings for what you have completed. Risks are comparable to typical computer use. There is no direct benefit to you anticipated from your participation in this study. The data we collect will not be linked to your identity in any way.

Figure S.7: Qualification session instructions (3 of 6)

If you have any questions about this research project, please contact [researcher] at [researcher email].

If you have any questions regarding your rights and participation as a research subject, please contact the [IRB contact information].

Participation in research is voluntary. Clicking the button labeled "I Consent" below will indicate that you have decided to participate as a research subject in the study described above.

☐ I CONSENT

Qualification survey

To qualify for the study, complete this one-time survey. Earn $1.50 for a complete submission.

Please enter your worker ID:

Questions about instructions

1. On which of the following days do you plan on completing a HIT for this project?

☐ Sun, Oct 27 ☐ Mon, Oct 28 ☐ Tue, Oct 29 ☐ Wed, Oct 30 ☐ Thu, Oct 31
☐ Fri, Nov 1 ☐ Sat, Nov 2 ☐ Sun, Nov 3 ☐ Mon, Nov 4 ☐ Tue, Nov 5
☐ Wed, Nov 6 ☐ Thu, Nov 7 ☐ Fri, Nov 8 ☐ Sat, Nov 9

2. After you receive your qualification today, will there be another HIT to complete today?

○ Yes. ○ No.

3. What happens if you make counting errors in the task?

○ The HIT might be rejected. ○ I must start the entire HIT over from the beginning.
○ I will be told which rows have errors, then I'll correct the errors.

4. Suppose you complete the first HIT later today and you also complete the HIT on this Wednesday. However, you do not complete the HIT next Wednesday. How much will you earn in total from this study? (Do not include earnings from this qualification HIT.)

5. How many HITs in total must you complete to earn the bonus? (Do not count this qualification HIT.)

Figure S.8: Qualification session instructions (4 of 6)

6. On how many days total (including today) must you complete a HIT to earn the bonus? (Do not count this qualification HIT.)

7. How much will you earn in total by fully participating in this study, including the bonus? (Do not include earnings from this qualification HIT.)

8. Each day's HIT will definitely be available during what times?

Specify a time range using Pacific Time:

Beginning at: Ending at:

Demographic survey

Your responses will kept anonymous and will not affect your participation in this study.

1. What is your gender?

Male Female Other

2. Would you say that in general your health is—

3. During the past month, other than your regular job, did you participate in any physical activities or exercises such as running, calisthenics, golf, gardening, or walking for exercise?

4. On average, how many hours of sleep do you get in a 24-hour period?

5. What is your age?

Figure S.9: Qualification session instructions (5 of 6)

Start today's HIT

- This is the first of the 3 HITs for an academic research project.
- Please review these instructions.

Earnings

- This HIT pays $1.50
- 1 HIT this Wednesday; it pays $1.50
- 1 HIT next Wednesday; it pays $1.50
- Earn a $5 bonus for completing all 3 HITs

This HIT's requirements

This HIT should only take 5-10 minutes to complete; it pays $1.50

1. Work 10 rows of the counting task (like the example below)
2. Decide how to split a workload of 360 rows of counting between Wednesday and next Wednesday

This Wednesday's HIT requirements

This HIT may take 10-20 minutes, depending on how you split the work between the days; it pays $1.50

1. Work 10 rows of the counting task (like the example below)
2. Decide how to split a workload of 360 rows of counting between Wednesday (that same day) and next Wednesday
3. One of your decisions from today or Wednesday is selected to actually split the workload
4. Work the counting task for the selected amount of work for this Wednesday

Next Wednesday's HIT requirements

This HIT may take 10-20 minutes, depending on how you split the work between the days; it pays $1.50

1. Work 10 rows of the counting task (like the example below)
2. Work the counting task for the selected amount of work for next Wednesday

Summary

Earn $9.50 total (in addition to the $1.50 qual HIT) for about 30 to 40 minutes of work total, assuming you complete all 3 HITs.

Figure S.10: Day zero session instructions (1 of 3)

Example: working 10 rows of the counting task

This is a completed example of the counting task that would be in each HIT.

Please count the number of zeros (“0”) on each line and enter it in the box.

Each row will be marked correct or incorrect. You must correct errors before submission.

Row No.	String	Count (“0”)
1	1000110011100011	8
2	1000010100000001	12
3	1110001110000011	8
4	0100110010101111	7
5	0000100010101110	10
6	1101001011001010	8
7	0000111001010001	10
8	1011100110010010	2
9	1110110100011111	5
10	0110001100111001	8

Check responses and save

Decide how to split a workload between this Wednesday and next Wednesday

You start with a workload of 360 rows of counting. You will decide how to split up the workload between this Wednesday and Wednesday of next week.

You decide for different trade-off scenarios. For example:

- Working 1 more row next week reduces work by 1 row this week (a 1-to-1 trade-off)
- Working 1 more row next week reduces work by 1.5 rows this week (a 1-to-1.5 trade-off)
- Working 1 more row next week reduces work by 0.5 rows this week (a 1-to-0.5 trade-off)

One scenario will be selected to actually matter.

You will get more details and practice in this HIT before you make your decisions that matter.

Figure S.11: Day zero session instructions (2 of 3)

Complete each of 3 days' HITs for a $5 bonus, earning $9.50 total

- On each day in blue below, complete one HIT for $1.50.
- Once you complete all 3 HITs, you will be paid a $5 bonus.
- If you fail to complete a day's HIT, you will not be able to complete further HITs, nor will you receive the bonus.
- You will always be paid for HITs you have already completed. All earnings are paid within 24 hours.
- Each day's HIT will be available *at least* between 12 p.m. and 12 a.m. (midnight), Pacific Time.
- You will receive a reminder notification through mTurk on each day that you have a HIT to complete.
- Please do not accept more than one HIT on the same day; it will not give you additional work or earnings.
- ***Please only participate if you think you can complete one HIT on each of these 3 days!***

Sun, Oct 27	**Mon, Oct 28 (today)** 1 HIT	Tue, Oct 29	**Wed, Oct 30** 1 HIT	Thu, Oct 31	Fri, Nov 1	Sat, Nov 2
Sun, Nov 3	Mon, Nov 4	Tue, Nov 5	**Wed, Nov 6** 1 HIT	Thu, Nov 7	Fri, Nov 8	Sat, Nov 9

No deception is used in this experiment

This is an academic research project in the field of Economics, which widely prohibits the deception of experimental subjects.

All information and instructions provided to you in this experiment are truthful.

If you feel anything in this experiment is deceptive, please notify the [IRB] at [IRB email].

If you have any questions about this research project, please contact [researcher] at [researcher email].

Begin practice round

You will complete a practice round before the actual tasks and decisions that matter.

Begin PRACTICE

Figure S.12: Day zero session instructions (3 of 3)

PRACTICE MODE **The correct answers are already filled in to save you time.**

Complete 10 required rows of counting

Please count the number of zeros (“0”) on each line and enter it in the box.

Each row will be marked correct or incorrect. You must correct errors before submission.

Row No.	String	Count (“0”)
1	1000110011100011	8
2	1000010100000001	12
3	1110001110000011	8
4	0100110010101111	7
5	0000100010101110	10
6	1101001011001010	8
7	0000111001010001	10
8	1011100110010010	8
9	1110110100011111	5
10	0110001100111001	8

Check responses and save

Figure S.13: Required tasks: This interface shows the subject examples of the task.

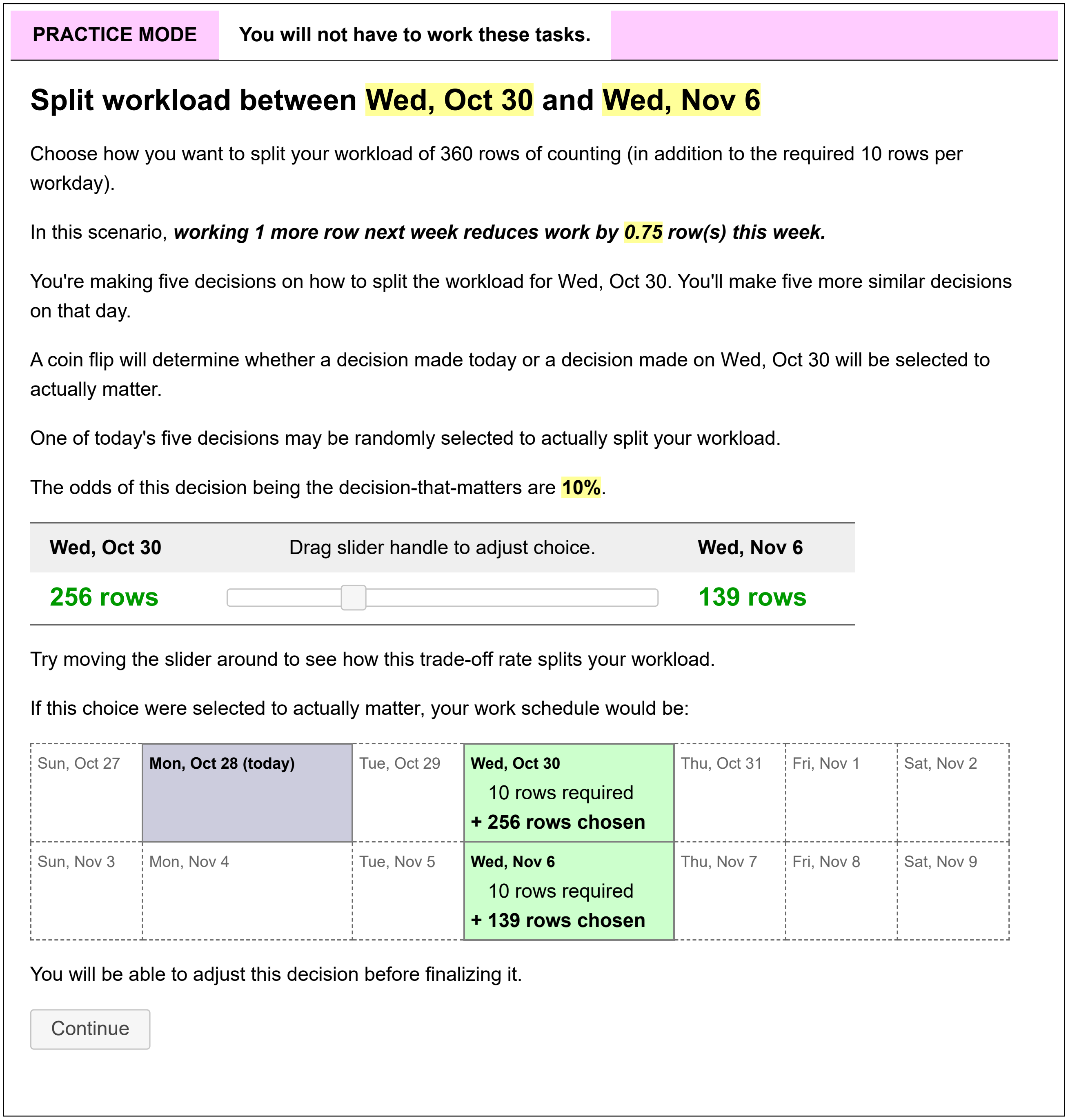

Figure S.14: Task allocation, separate: This interface allows the subject to allocate their workload between days.

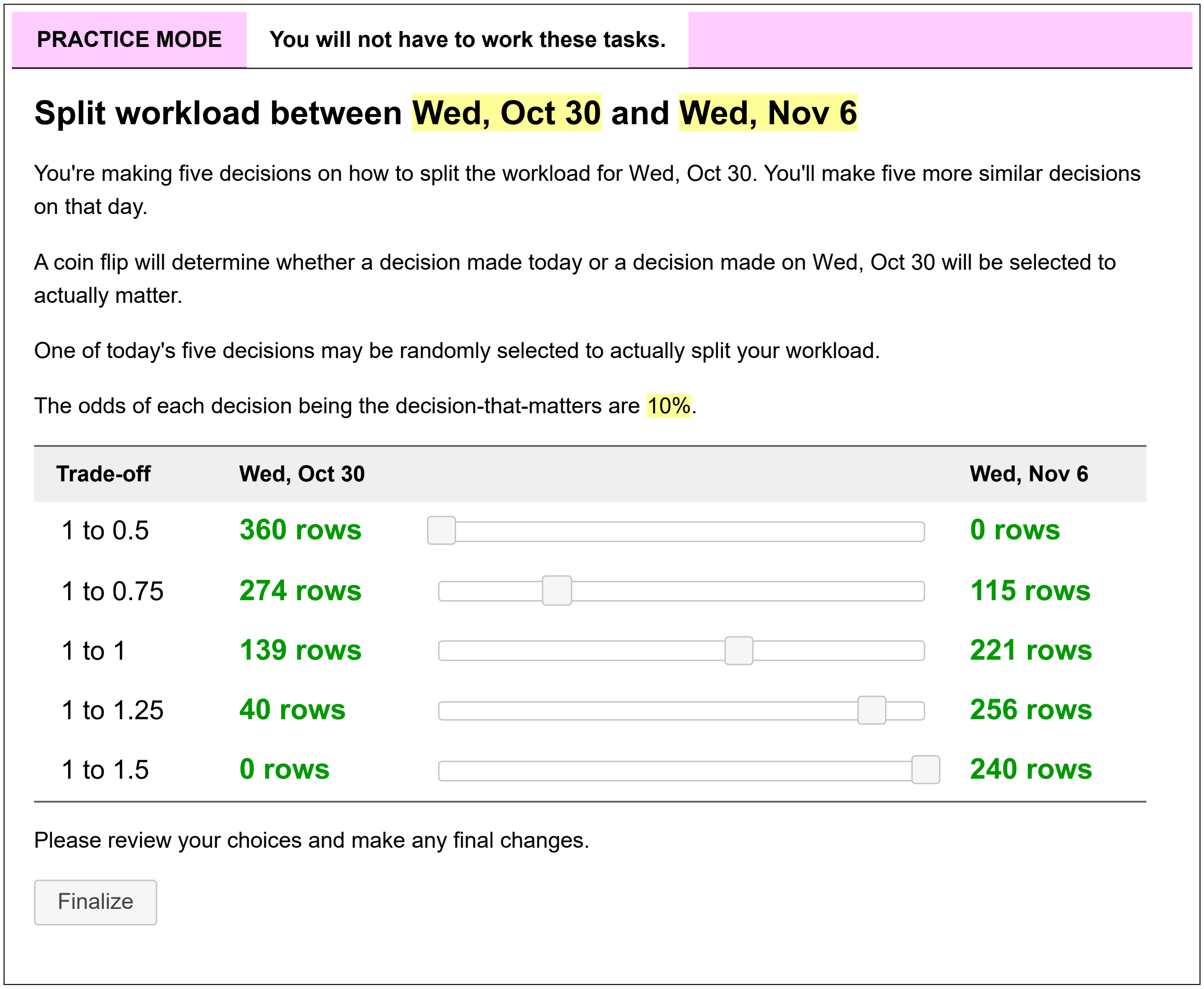

Figure S.15: Task allocation, juxtaposed: This interface allows the subject to allocate their workload between days.

PRACTICE MODE

How today's decisions are used

You made decisions about splitting work between this Wednesday and next Wednesday.

You will make similar decisions again Wednesday. One day will be selected for its decisions to actually matter.

Sun, Oct 27	**Mon, Oct 28 (today)** Decisions made	Tue, Oct 29	**Wed, Oct 30** Decisions made	Thu, Oct 31	Fri, Nov 1	Sat, Nov 2
Sun, Nov 3	Mon, Nov 4	Tue, Nov 5	**Wed, Nov 6**	Thu, Nov 7	Fri, Nov 8	Sat, Nov 9

You just made five decisions about how to split work between these days

Today's choices

Choice No.	Trade-off	Wed, Oct 30	Wed, Nov 6
1	1 to 0.5	360 rows	0 rows
2	1 to 0.75	235 rows	167 rows
3	1 to 1	139 rows	221 rows
4	1 to 1.25	52 rows	247 rows
5	1 to 1.5	0 rows	240 rows

You will make five similar decisions Wednesday

Wednesday's choices

Choice No.	Trade-off	Wed, Oct 30	Wed, Nov 6
1	1 to 0.5	x rows	x rows
2	1 to 0.75	x rows	x rows
3	1 to 1	x rows	x rows
4	1 to 1.25	x rows	x rows
5	1 to 1.5	x rows	x rows

After you make decisions Wednesday, a coin-toss will select which day's decisions are used

Today's choices

Choice No.	Trade-off	Wed, Oct 30	Wed, Nov 6
1	1 to 0.5	360 rows	0 rows
2	1 to 0.75	235 rows	167 rows
3	1 to 1	139 rows	221 rows
4	1 to 1.25	52 rows	247 rows
5	1 to 1.5	0 rows	240 rows

Wednesday's choices

Choice No.	Trade-off	Wed, Oct 30	Wed, Nov 6
1	1 to 0.5	x rows	x rows
2	1 to 0.75	x rows	x rows
3	1 to 1	x rows	x rows
4	1 to 1.25	x rows	x rows
5	1 to 1.5	x rows	x rows

Reveal

Figure S.16: This interface gives the subject intuition regarding the selection procedure between days.

PRACTICE MODE **You will not have to work these tasks.**

Implement workload for Wed, Oct 30 and Wed, Nov 6

For this practice round, the coin-toss selected today's decisions to actually matter.

Accordingly, here are the decisions you made today to split the workload of 360 rows of counting.

One rate and your corresponding choice is randomly selected to actually matter.

These work amounts are in addition to the 10 rows of counting required on each day.

Choice No.	Trade-off	Wed, Oct 30	Wed, Nov 6
1	1 to 0.5	360 rows	0 rows
2	1 to 0.75	235 rows	167 rows
3	1 to 1	139 rows	221 rows
4	1 to 1.25	52 rows	247 rows
5	1 to 1.5	0 rows	240 rows

Reveal

Figure S.17: This interface gives the subject intuition regarding the selection procedure between rates.

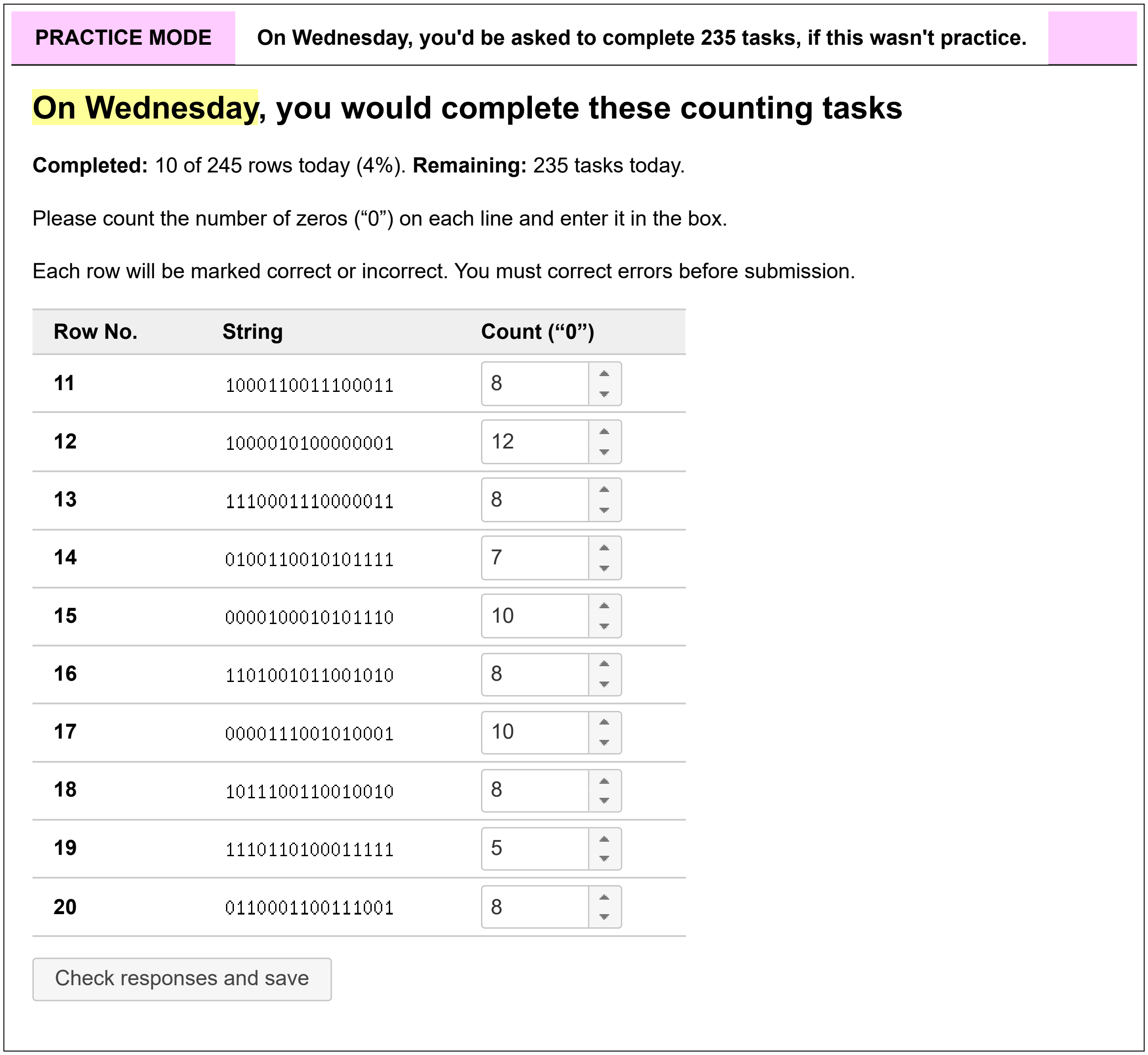

Figure S.18: This interface shows the subject examples of the implemented tasks.