Waiting to Choose: The Role of Deliberation in Intertemporal Choice*

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Abstract

We study the impact of deliberation on intertemporal choices. Using multiple experiments, including a field study in the Democratic Republic of Congo, we show that the introduction of waiting periods—a policy that temporally separate information about choices from choices themselves—cause substantially less myopic decisions. These results cannot be captured by models of exponential discounting nor present bias. Comparing the effects of waiting periods to making planned choices over future time periods, the former has a larger impact on reducing myopia. Our results highlight the role of deliberation in decision-making and have implications for policy and intervention design.

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1 Introduction

Suppose an individual learns of a substantial windfall in her tax refund. Typically, the waiting time to receive a refund is three weeks after filing, during which she can deliberate what portion to save and what to spend. Now imagine that a firm offered to deliver the refund immediately: would removing this waiting period change the individual's decision to spend or save?[1]

Models of time-discounting—both classic exponential and most behavioral "present-focused" models—say no.[2] Despite this, waiting periods are often imposed in practice when myopia and impulsivity are perceived to be particularly harmful. Many U.S. states impose a waiting period of up to 14 days between the purchase and receipt of a gun, and recent work has found them to be effective for reducing homicides by as much as 17%, or 750 gun homicides per year (Luca et al., 2017).[3] Waiting periods are also often imposed for those seeking to get married or divorced, and are prescribed as a strategy for avoiding myopic choices in negotiations (Brooks, 2015) and conflict resolution (Burgess, 2004). These policies are predicated on the idea that inserting a delay between when information about a choice comes into focus and the ability to make a choice can prompt a shift towards more deliberative thinking and lead to less myopic decisions. Despite the frequent use of waiting periods in practice, there has been little economic research to examine whether such deliberation prompts actually affect intertemporal choice.[4]

This paper presents evidence that waiting periods have a significant effect on intertemporal choices. Across three studies, we show people make less myopic, present-focused decisions when information about a choice and the opportunity to make it are separated by a waiting period. We examine and attempt to rule out several potential mechanisms, including present bias and a general tendency to be more prudent when prompted. Together, our findings demonstrate the systematic impact of waiting periods on intertemporal choice across multiple contexts, and provide evidence

[1] This example is pertinent in light of the recent partnership between Walmart, a retail store, and Jackson Hewitt, a tax preparation service. Customers are able to file their taxes at Walmart, with Jackson Hewitt providing the refund immediately on a gift card through a no-cost Refund Anticipation Loan.

[2] Assuming commitment devices are not available.

[3] Additionally, Koenig and Schindler (2016) find that the ten states and the District of Columbia that have waiting periods for the purchase of firearms (ranging from 24 hours in Illinois to 14 days in Hawaii) experienced smaller upticks in firearm purchases than other states following the Sandy Hook massacre. Edwards et al. (2016) find that mandatory waiting periods reduce firearm suicides.

[4] Experimental work on waiting periods in economics pertains almost entirely to cooperation and social preferences. See Andersen et al. (2018) for an example and a review of the literature.

for targeted deliberation as the mechanism.

In the first study, individuals allocated labor and leisure across two hour-long work periods within a single session. We used an online labor market to recruit a population that was experienced in making short-term, do-it-now-or-later style, intertemporal labor-leisure decisions in a similar context. Each participant chose how to allocate real-effort tasks between two work periods, where any time not spent on the effort task could be used to engage in other activities of their choosing. Delaying tasks to a later work period typically resulted in a greater total task requirement, while choosing to allocate tasks to the earliest available period minimized total work time.

In the Immediate treatment, participants were given the information about the allocation decision and had the opportunity to make their choice directly thereafter; in the Waiting Period treatment they were given the same information, but could only make their choice after a one-hour waiting period. Both sets of participants faced the exact same choice set—allocating effort tasks between two work periods—and neither group faced any time pressure to make their choices.[5] The only difference is that the latter group faced a waiting period before the *opportunity* to make a choice, while the former was given this opportunity directly after learning the information.

We find that introducing a waiting period had a significant effect on intertemporal allocations. After the waiting period, participants allocated more effort tasks to the earlier work period, reducing their overall workload as a result. The magnitude of the effect was sizable: 17% of the mean in the treatment without a waiting period, or half of a standard deviation ($p = 0.02$). A model that accounts for the impact of waiting periods on corner solutions (allocating all tasks to either period) shows an effect size of 35% ($p = 0.02$). Another interesting feature of the results is that the waiting period affects the extensive margin of whether all tasks are assigned to the first work period or not: 46% of choices without a waiting period are to do all tasks in WP1, compared to 73% with a waiting period ($p = 0.01$). The overall rate of extensive margin choices implies that intertemporal elasticity of substitution must be relatively high, while the treatment effect suggests that waiting periods led to the realization that it is worth working harder now if it means minimizing the number of tasks overall.[6] This points to the possibility that, rather than changing people's underlying level

[5]Because no participant faced time pressure, our studies are distinct from work examining the effects of *restricting* the time available to make a choice in social dilemmas such as the public goods game. Additionally, because the timing of the opportunity to make a choice was varied exogenously, our work is also distinct from research correlating reaction times with decisions in social dilemmas (see Rand et al., 2012, for examples of both approaches).

[6]Given the short time horizon, we focus on the comparative statics between treatments as identifying differences in

of discounting, waiting periods prompt deliberation which leads to less sub-optimal choices.

To rule out the possibility that this effect is driven by differences in the timing of work periods (e.g. one group getting used to waiting, or exogenous information shocks during the delay), we also implemented a Delay Control treatment in which the first hour did not involve work, as in the waiting period treatment, but information about the allocation decision was only presented after this delay.[7] If our results are driven by timing of the work periods, then participants in the Delay Control treatment would make similar allocation decisions to those in the Waiting Period treatment. In contrast, if waiting periods affect behavior by prompting deliberation over the choice set, allocations in the control treatment would resemble those in the Immediate treatment without a waiting period. Consistent with the deliberation account, we find no significant difference in task allocation between the Delay Control treatment and the Immediate treatment (effect size of 3% $p = 0.606$).

A potential mechanism driving our results could be that individuals have non-constant discounting (e.g. present bias), are sophisticated about it, and can mentally commit to a plan. We test for this explanation explicitly by implementing a Commit treatment in which participants were given the opportunity to make an allocation decision between two future work periods—the same two as those in the waiting period condition—directly after being informed about the choice set. If behavior in the Waiting Period treatment is explained by non-constant discounting, then the allocation decision over two future work periods in the Commit treatment would be similar to choices of those who faced a waiting period. Instead, we find that participants in the Commit treatment still put off significantly more effort tasks to the later work period than those who faced a waiting period before the decision ($p = 0.04$). The Commit treatment recovers only 40% of the effect of waiting periods on myopia compared to the Immediate treatment.

Using these data, we estimate a structural model to make direct comparisons between station-

present-focused choices, i.e. *myopia*, where the decision to delay work and do more total tasks is interpreted as more myopic than a choice that results in fewer total tasks. As discussed further below, this identification assumption follows from the foundational work by McClure et al. (2007) who demonstrate that myopia operates over very short time horizons (10 minutes) for consumption goods. Follow up work by Balakrishnan et al. (2017) shows that the short time horizon is appropriate for identifying myopia over money as well. For this reason, we believe that our task is well-suited to study changes in myopia as a function of waiting periods. This being said, we do not claim that our results—particularly with regards to baseline task allocations—will generalize to discounting between future periods over longer time horizons.

[7]This is similar to the robustness treatment in Halevy (2015).

arity violations, i.e. differences in allocations made over sooner versus later periods, and violations of time invariance, i.e. differences in allocations after a waiting period, given the same choice set.[8] Violations of stationarity (invariance) involve comparisons of allocations in the Immediate treatment to those in the Commit (Waiting Period) treatment. The results, which are presented in the Appendix, suggest that waiting periods are twice as effective in reducing myopia as the ability to plan and commit for the future.

One concern with the first study is that participants' behavior during the waiting periods was unobservable; perhaps social interactions or access to other informational sources were responsible for the impact of waiting periods. We ran the second study to determine if the initial findings were robust to eliminating these possibilities and to further explore the mechanism driving the observed effects. The experiment was run in the laboratory using a similar protocol as the first study. Participants were randomized into either a treatment with a waiting period or one without. We enforced tight control over the environment and restricted any participant communication with others. The effect of waiting periods is replicated in this environment both in sign and magnitude: compared to the 17% effect in the online experiment, waiting periods led to a 14% increase in tasks allocated to the earlier work period ($p = 0.012$).

We also use the laboratory study to test whether waiting periods cue a general shift from 'fast' automatic to more deliberative decision-making (Kahneman, 2011), or whether they prompt targeted deliberation with respect to the choice at hand. The latter would be consistent with models of 'prospection', whereby people facing an intertemporal choice engage in targeted deliberation by mentally simulating the utility consequences of the decision's potential outcomes (Wheeler et al., 1997; Schacter et al., 2007; Gilbert and Wilson, 2007; Gabaix and Laibson, 2017). Richer mental simulations are predicted to decrease myopia by reducing noise around future utility forecasts.[9] In our setting, if waiting periods affect choices across domains, then this would suggest a general shift in decision-making; if the effects are specific to the domain associated with the provided information, then this would provide evidence for targeted deliberation. To test for this, participants

[8]Halevy (2015) presents the three properties of the standard exponential discounting model of time preferences and describes how violations can be identified. In a three period model, $t = 1, 2, 3$, violations of stationarity imply a difference in allocations made in $t = 1$ between periods $t = 1, 2$ versus $t = 2, 3$ (Immediate versus Commit treatments, respectively). Violations of invariance imply a difference in allocations made in $t = 1$ over $t = 1, 2$ versus allocations made in $t = 2$ over $t = 2, 3$ (Immediate versus Waiting Period treatments, respectively.

[9]These predictions are outlined more formally in Section 2.1.2.

in both treatments saw questions from the Cognitive Reflection Test—which was specifically designed to measure 'fast' versus deliberative decision-making (Frederick, 2005)—immediately after making their task allocation choices. Importantly, they were not provided with information about these questions beforehand so that participants with a waiting period could not deliberate about them. Consistent with the targeted deliberation account, there were no differences in CRT scores between the waiting period conditions ($p = 0.92$).[10]

Our third study was run in the field as a proof-of-concept, illustrating how delays between information and choice can be used as a policy tool to reduce myopia in practice, and demonstrating the generalizability of our results across contexts. We worked with a small neighborhood grocery store in Bukavu, Democratic Republic of Congo (DRC). Upon arriving at the grocery store, customers received a coupon that could be exchanged for one bag of flour on a pre-specified redemption date. For each day the coupon was saved after this date, its value increased by an additional bag of flour (up to five bags total). In one treatment, customers had the opportunity to redeem the coupon on the same day it was received; in the other, they had to wait one day before being able to redeem the coupon (the value-accrual schedule was thus delayed by a day as well). Thus, the treatment variation enforced a waiting period between coupon receipt and the ability to use it. We find that the introduction of a waiting period led to a significant and meaningful reduction in the fraction of individuals redeeming their coupon on the earliest possible date—for the smallest amount of flour—from 25% without a waiting period to 9% with one ($p = 0.001$). Despite attempts to minimize potential confounds—which we discuss more fully in Section 3.1—the field setting prevents us from completely ruling them out. In this sense, the findings from this study are complementary to our lab and online studies which demonstrate the waiting period effect in a more controlled setting.

Our findings highlight the important role of deliberation in intertemporal choices. More specifically, the results point to the potential role of waiting periods in mitigating myopia. As demonstrated in McClure et al. (2007), the psychological wedge between "now" and "later" operates over very short time periods—substantially shorter than the time frames used in our studies. In turn,

[10]Comparing allocations between the Waiting Period and Delay Control conditions in the online study offers a further test of this hypothesis. Namely, if waiting generates a general shift in more deliberative decision-making, then decisions in the Delay Control condition should be similar to those in the Waiting Period condition. Since choices were more myopic in the former than the latter, this points to targeted deliberation as the mechanism.

we believe that our paradigms are well-suited to identify changes in myopia. Additionally, since the waiting period does not change when decisions are made, the shift in behavior constitutes a violation of time invariance—an under-explored property of intertemporal choice models (Halevy, 2015). That being said, allocations over short time horizons are likely subject to factors that may not play a significant role over longer horizons, such as anticipatory utility, perceived fatigue (as our structural model will suggest), and narrow bracketing. This will likely preclude generalization of any estimated discount rates (i.e. δ in models of time preferences) to discounting over longer time horizons.

In Section 2.1.2, we discuss how our results are consistent with theories of 'prospection', where deliberation reduces myopia by decreasing uncertainty over future utility outcomes (Gilbert and Wilson, 2007). Gabaix and Laibson (2017) present a formal framework for this process. In their model, myopia is at least partly due to greater cognitive noise and uncertainty surrounding the utility consequences of future events compared to similar events in the present. If that noise can be reduced through prompts which cue the individual to stop and deliberate about the utility consequences of choices, this implies a clear mechanism through which waiting periods will mitigate myopic choices by reducing 'suboptimal' decisions.[11] In this way, our paper contributes to the recent literature examining the role of cognitive noise and information processing in generating biases in judgment and decisions (Woodford, 2020; Khaw et al., 2018; Frydman and Jin, 2020; Enke and Graeber, 2019; Bordalo et al., 2020; Caplin and Dean, 2015).

Our work also contributes to the related literature in economics that distinguishes between automatic, heuristic thinking and deliberative processing (Thaler and Shefrin, 1981; Kahneman, 2003). In these models, automatic decisions are characterized by the use of simple heuristics that reduce the complexity of the choice but may result in systematic biases, whereas deliberation leads to decisions that are closer to the predictions of the normative model. Myopic, nearsighted behavior has been attributed to 'fast' choices and the use of heuristics (Rubinstein, 2003; Read et al., 2013), while more farsighted behavior has been attributed to deliberative processing (Metcalfe and Mischel, 1999). Researchers have used such models to explain anomalies in the cross-section of stock returns (Barberis et al., 2013) and excessive focus on the leftmost odometer digits in used-car sales (Lacetera et al., 2012). Rees-Jones and Taubinsky (2016) argue that reliance on heuristics

[11] Here, 'suboptimal' refers to the comparison between choices made with cognitive noise to those without it.

leads to significant misperceptions of the US Federal Income Tax code, while Kessler et al. (2017) show that deliberative processing leads people to choose more efficient allocations in social dilemmas. Dai and Fishbach (2013) show that, similar to our findings, informing participants about a decision one month in the future leads to more patient choices over lotteries involving money or durable goods. However, given the length of the delay, one cannot separately identify the effects of deliberation from the arrival of decision-relevant information during this period (e.g. learning through communication with others), potential opportunities for arbitrage, or changes in preferences orthogonal to the effects of deliberation (e.g. getting acclimated to process of waiting).[12]

The rest of the paper is organized as follows. Section 2 describes both effort allocation experiments, outlines the hypotheses and presents the results. Section 3 presents the application of waiting periods in the field and the results. Section 4 discusses the findings and concludes.

2 Waiting Periods and Effort Allocation

2.1 Online Labor Market

2.1.1 Design and Implementation

Participants performed a series of real-effort tasks over a span of approximately three hours for a \$20 payment.[13] For our first study, we used an online labor market run by Amazon's Mechanical Turk to recruit participants (N = 122) who were experienced in making intertemporal tradeoffs between labor and leisure in a context similar to our experiment.[14] We adopted the approach of Augenblick et al. (2015) in allowing participants to allocate the effort tasks between two work periods using a series of discretized Convex Time Budgets (Andreoni and Sprenger, 2012; Andreoni et al., 2015). Participants were informed that in order to complete the study and earn their payment, they must finish a number of tasks over the course of two one-hour work periods — WP1 and WP2, respectively. We used a single session design to circumvent issues of differential transaction costs

[12] Another line of work looks at the *choice* to wait in dynamic games (Kang et al., 2010). In these settings, however, the incentive to wait has to do with the potential exogenous arrival of decision-relevant information, e.g. signals about the value of an asset.

[13] While the advertised duration was three hours, we enforced a five-hour time limit to allow for variation in task speed.

[14] Data collected from participants on Mechanical Turk compares favorably to other participant pools (Paolacci and Chandler, 2014). The platform has been increasingly used by economists to study a variety of questions such as incentive effects (DellaVigna and Pope, 2018) and probabilistic reasoning (Martinez-Marquina et al., 2018).

and future uncertainty that may affect intertemporal decisions (Cohen et al., 2016). This captures intertemporal choice over a short horizon, comparable to other studies that have used similar or shorter time horizons to examine present bias and myopia (McClure et al., 2007; Barton, 2015).

The span between finishing the effort tasks and the start of the next phase of the study was explicitly labeled as free time, during which the participants could engage in any activity of their choice.[15] In turn, we conceptualize the decision to allocate tasks between work periods as a now-vs.-later intertemporal tradeoff between work and free time, which we refer to as leisure from now on.

The tasks were designed to be effortful in the sense that barring compensation, the participant should prefer leisure over labor. Each effort task consisting of counting the numbers of zeros in large, randomly generated table of zeros and ones (Falk et al., 2006; Abeler et al., 2011). Pre-tests revealed that each 10x15 table took roughly one minute to complete. Participants encountered the tables one at a time and could not advance until they entered the correct answer. Before they were presented with information about the effort allocation budgets and decisions, participants had to successfully complete two sample tasks in order to become familiar with it.

After completing the sample tasks, participants were informed that they would face a series of choices to allocate effort tasks between WP1 and WP2. One choice was drawn at random and implemented as the actual work requirement. Each participant made allocation decisions using four convex time budgets that varied in the implied interest rate for putting tasks off to the later work period. Every budget allowed for the possibility of doing 40 tasks in WP1. For example, Budget 1 offered the the possibility of 40 tasks in WP1 and no tasks in WP2, no tasks in WP1 and 60 tasks in WP2, or any of nine evenly-spaced convex combinations of those extremes. Implied interest rates varied by budget, from 50% for Budget 1 to 0% for Budget 4. Table 1 presents the convex budgets.[16]

Participants were also given two binary choices that served as manipulation checks for sensitivity to the interest rate. The first offered a choice between 40 tasks in WP1 (and zero tasks in WP2)

[15] We did not want to restrict participants' ability to choose their preferred activity, and recommended that they could spend this time watching streaming movies, reading a book, etc. Exit survey data suggest participants indeed spent the time on leisure activities. Responses included "I cooked and watched television," "I read a book on my Nook. I've had a very stressful day and it was nice to have some free time to do that," "I mostly just listened to music and read some articles. All responses available upon request.

[16] See Section A.4 for examples of the tasks and choice sets.

Table 1: Choice Sets in the Online Study

Budget	Max. WP1 Tasks	Max. WP2 Tasks	# of Options	Interest Rate
1	40	60	11	50%
2	40	50	11	25%
3	40	45	6	12.5%
4	40	40	11	0%

WP1 and WP2 refer to Work Period 1 and 2, respectively. Maximum tasks allocated to one work period imply that zero tasks would be allocated to the other work period. The last column lists implied one-hour interest rates.

or 35 tasks in WP2 (and zero tasks in WP1)—a negative interest rate; the second offered a choice between 40 tasks in WP1 (and zero tasks in WP2) or 41 tasks in WP2 (and zero tasks in WP1)—a very small positive interest rate close to zero. The goal of these questions was to determine the degree of attention paid to small variations in the interest rate around a critical price point, and whether there was a general desire to get the tasks done now rather than later.

Participants could not advance from a work period to the next phase of the study until the full time allocated to that work period elapsed, even if all the allocated effort tasks were completed. This is a crucial design element that ensures allocating tasks to WP1 did not result in an earlier end to the experiment. This was accomplished by setting the work periods to last 60 minutes minus the number of tasks successfully completed. For example, if the participant was required to complete 60 tasks in a period, that period would consist only of those 60 tasks; if she was required to do 40 tasks, the participant would have 20 minutes left over in the work period to engage in a leisure activity of her choice before advancing. This design ensures that participants' decisions only reflected their preferences for allocating effort within the fixed duration of the study—allocation decisions did not change the total length of the study. It also ensures that working slowly to run out the clock on a work period was not an option. After the period elapsed, participants had to actively click a button to continue, either to the next set of tasks or to the exit survey, within a reasonable amount of time; otherwise the study would expire and they would not be paid. This means, for example, that even for a participant with zero tasks to complete in WP2, they had to be present at the end of WP2 to advance the protocol.[17]

[17] While the exact proximity to the computer cannot be verified, these prompts ensured that the participant had to be at least relatively close to the computer in order to complete the study and be paid.

We implemented four different treatments. To identify treatment effects with minimal confounds, we used a between-subjects experimental design (Charness et al., 2012). This differentiates our studies from within-subjects experiments that would more precisely identify inconsistent behavior (e.g. Sayman and Öncüler (2009)). All treatments presented participants with the same budgets as described above and were divided into three one-hour periods. The main differences between treatments are whether WP1 and WP2 were the first two periods or the last two periods. Figure 1 outlines all four treatments.

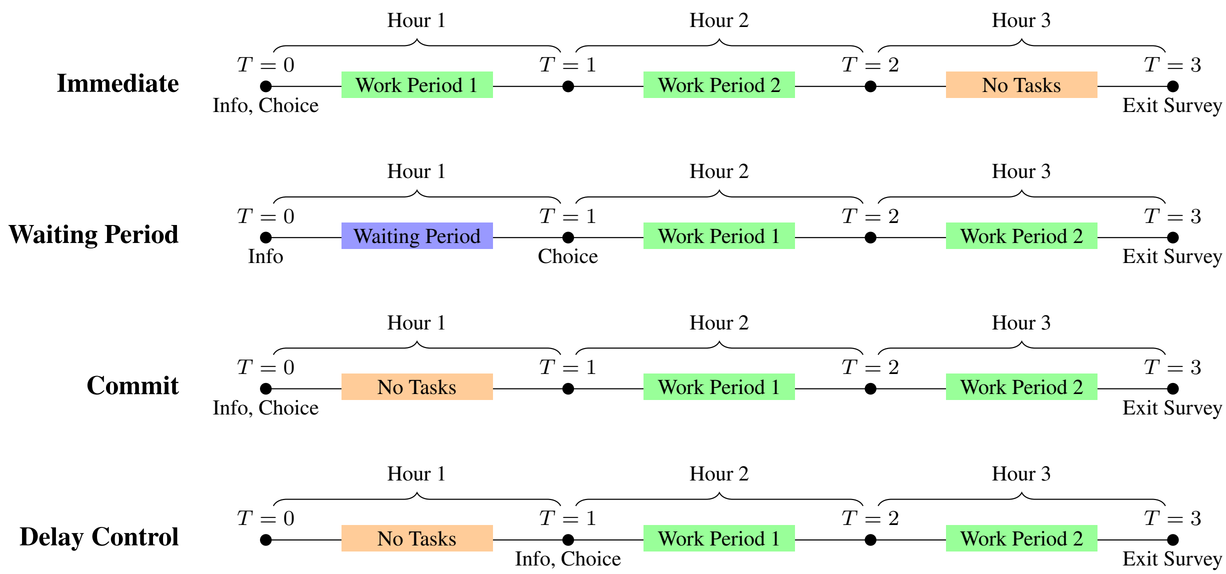

Figure 1: Outline of Experimental Conditions

In the Immediate treatment, participants were presented with information about the budgets and had the opportunity to make their allocation decisions over WP1 and WP2, which began directly after their choice. After the second WP ended, participants had a one-hour period where no work was required before filling out a questionnaire—the final hurdle that all participants had to clear before receiving payment. In the Waiting Period treatment, participants were presented with information about the budgets but had a one-hour waiting period before having the opportunity to make the allocation choice. As in the Immediate treatment, WP1 and WP2 followed directly after the decision. The Commit treatment features the same timing of the work periods as in the Waiting Period treatment, with the key difference that participants made allocation decisions *before* the waiting period. In other words, they made fully committed choices over outcomes that were shifted to the future.

Lastly, we designed the Delay Control treatment as a robustness check to ensure that any variation in behavior between the Immediate, Waiting Period, and Commit treatments was due to the waiting period rather than differences in the general timing of work periods. In the Delay Control treatment, participants learned that they would make a decision regarding the allocation of effort tasks but were not presented with the budgets until after a one hour period with no work. The Delay Control treatment has the same timing of work periods as the Waiting Period treatment, but the same information-choice timing as the Immediate treatment. This treatment allows us to rule out alternate explanations such as the delay getting participants used to waiting in general, exogenous information shocks, as well as basic differences in preferences over the timing of outcomes.

In no treatment were participants time constrained when making their allocation decisions. Instead, the crucial difference across treatments was whether the *opportunity* to make a choice was preceded by a waiting period. There were also no differences in the amount of information participants received prior to making their choices.

It is important to note that, as discussed in Cohen et al. (2016), intertemporal decisions over non-monetary consumption events such as food and effort involve potential factors other than the individual's level of time discounting. In the case of effort, factors such as the shape of the effort cost function, complementarities between effort and leisure, or negative anticipatory utility from future effort costs (Loewenstein, 1987) may affect the baseline allocation of tasks between the two periods.[18] The shorter time horizon may also introduce factors such as narrow bracketing (Read and van Leeuwen, 1998)—where people consider tasks over work periods jointly rather than in isolation—that would generate a motive to minimize the total number of tasks performed.[19] It is therefore difficult to interpret the allocation in any one treatment as a general measure of time discounting between future periods (i.e. the δ parameter in models of time preferences). That being said, prior work has shown that both short time horizons and the use of consumption events can identify myopia (McClure et al., 2007). Since all treatments in our experiment involved the same intertemporal allocation decisions between two periods of the same length, background factors

[18] For example, in reviewing the literature on measuring time preferences, Cohen et al. (2016) show that estimated discount rates for consumption rewards are consistently higher than for monetary rewards.

[19] Research has demonstrated that context and framing effects have a significant impact on the implied level of time discounting (Ainslie and Haendel, 1983; Sayman and Öncüler, 2009). Factors such as the question format (Read et al., 2012), current affective state (Loewenstein et al., 2003), and the ordinal ranking of outcomes (Loewenstein and Prelec, 1993) have all been shown to impact not only the quantitative measures of discounting, but the qualitative conclusions as well.

such as fatigue and bracketing are held constant. As such, we focus on the differences in choices across treatments as identifying changes in myopia.

2.1.2 Hypotheses

In this section, we outline the hypotheses that different models of time preferences make in our setting. Consider a decision-maker (DM) who chooses to allocate tasks, x_t, between work periods which can occur in $t = 0, 1, 2$. The DM evaluates utility as $U_k(x_0, x_1, x_2) = \sum_{t=k}^{2} D(t-k)u(x_t)$, where $D(\cdot)$ is the DM's discounting function, and k represents the time period in which the evaluation is made. Assuming that the tasks are not enjoyable to perform, normalizing the instantaneous disutility of effort function $u(0) = 0$ and the discounting function $D(0) = 1$, the DM in our Immediate treatment solves the following decision problem

$$\min_{x_0,x_1} U_0(x_0, x_1) = u(x_0) + D(1)u(x_1) \quad \text{s.t.} \quad x_0 + \frac{x_1}{1+r} = 40 \quad , \tag{1}$$

where r is the interest rate by which tasks avoided in the earlier period grow. In the Waiting Period treatment the allocation choice is shifted by one period, so the DM solves

$$\min_{x_1,x_2} U_1(x_1, x_2) = u(x_1) + D(1)u(x_2) \quad \text{s.t.} \quad x_1 + \frac{x_2}{1+r} = 40 \quad . \tag{2}$$

We first outline the predictions of the standard exponential discounting model and the commonly used behavioral model of present bias with quasi-hyperbolic discounting, which relaxes the assumption of constant discounting. Under constant, exponential discounting, $D(t-k) = \delta^{t-k}$, with δ typically $\in [0, 1]$. The DM in the Waiting Period treatment solves the same decision problem subject to the same constraint as in the Immediate treatment, with the labels shifted by one period. In turn, under exponential discounting the allocations should be the same in both treatments.

Under quasi-hyperbolic discounting, $D(t-k) = \beta^{1(t>k)}\delta^{t-k}$. The β parameter serves to further discount any utility or disutility not received immediately. $\beta \in [0, 1)$ corresponds to a violation of constant discounting and is used to model impulsivity and procrastination (Laibson, 1997). When the DM first receives information about the decision in $t = 0$ of the Waiting Period treatment, she evaluates the future allocation as

$$\min_{x_1,x_2} U_0(x_1,x_2) = D(1)u(x_1) + D(2)u(x_2) \quad \text{s.t.} \quad x_1 + \frac{x_2}{1+r} = 40 \quad , \tag{3}$$

where $D(1) = \beta\delta$ and $D(2) = \beta\delta^2$. When $k = 0$, her preferred allocation may indeed be different than her choice in the Immediate treatment. However, absent an ability to commit to that preferred allocation, the DM again faces the decision problem represented in (2) after the waiting period elapses—that is, the decision faced in the Immediate treatment shifted by a period. Note that the same logic holds for the case of true hyperbolic discounting, where the discount factor is non-constant between any two periods. In turn, absent the ability to commit, both exponential and quasi-hyperbolic discounting models predict that a waiting period should not affect the allocation decision. We refer to x_t^T as the allocation of tasks to period t that solves the disutility minimization problem in treatment $T \in \{I, WP, C, DC\}$ corresponding to the Immediate, Waiting Period, Commit, and Delay Control treatments, respectively.

Hypothesis 1. *Exponential/Quasi-hyperbolic/Hyperbolic discounting: absent the ability to commit,* $x_1^{WP} = x_0^I$.

If the DM in the Waiting Period treatment could commit to an allocation in $k = 0$, then choices in that treatment may differ from those in the Immediate treatment. Compare the problems solved by a quasi-hyperbolic DM in equations (1) and (3). When $\beta \in [0, 1)$, $D(1) < \frac{D(2)}{D(1)}$ and the DM displays present bias and prefers a more patient allocation in equation (3) than (1). O'Donoghue and Rabin (1999) show that a DM who is sophisticated about her present bias may take steps to commit herself to following through on the allocation preferred in $k = 0$. Though our experiments were structured to minimize the availability of external commitment devices, participants in the Waiting Period treatment may have been able to mentally commit to a choice in $k = 0$ and follow through on this initial plan in $k = 1$. The Commit treatment makes this commitment opportunity explicit—it features exactly the choice in equation (3)—to test whether the effects of waiting periods are attributable to this commitment mechanism.[20] Any lapse in mental commitment would

[20]Identification of present-bias depends critically on how the 'present' is defined. McClure et al. (2007) demonstrate that delaying the earliest reward by a ten-minute window is sufficient shift it to the future: participants exhibited significant present bias when choosing between rewards at 0,10 and 20 minutes, but exhibited no detectable present bias when all rewards were shifted by ten minutes (with the earliest reward available ten minutes later). Based on

lead the DM in the Waiting Period treatment to make a less patient choice than in the Commit treatment. In turn, if non-constant discounting such as present bias was driving the difference between the Waiting Period and Immediate treatments, then task allocations to the earlier work period in the Commit treatment should be greater than or equal to those in the Waiting Period treatment.

Hypothesis 2. *Quasi-hyperbolic/Hyperbolic discounting: with a perfect ability to commit in the Waiting Period treatment, $x_1^{WP} = x_1^C$. With imperfect ability to commit, $x_1^{WP} < x_1^C$.*

Work on the role of cognitive noise and information processing offers a potential mechanism for why waiting periods may lead to a greater allocation of tasks to the earlier work period. Research in psychology (Gilbert and Wilson, 2007; Wheeler et al., 1997) and neuroscience (Schacter et al., 2007) posits that people engage in the process of 'prospection' when making intertemporal choices, whereby they mentally simulates future outcomes to get a better sense of the pain or pleasure to be expected from each. Richer simulations are argued to result in less myopic choices. Gabaix and Laibson (2017) develop a formal model that builds on this work, where the DM is uncertain about the true realization of future utility and generates imperfect forecasts of the relevant outcomes. The DM draws a noisy signal about utility consequences through the process of mentally simulating future outcomes and events. In this way, intertemporal choice can be viewed as a signal extraction problem. When first presented with potential choices, the DM's decision *without* deliberation relies on a noisy prior belief about future utility. Under the assumption that the variance of cognitive noise increases with the horizon—intuitively, events that are further away are more difficult to represent mentally—the DM behaves myopically. We argue that waiting periods prompt deliberation and mental simulations of the decision problem. As a result, utility forecasts will be more accurate and myopia will be mitigated.[21]

Hypothesis 3. *Deliberation: $x_1^{WP} > x_0^I$.*

The result depends on the existence of a tradeoff in allocating tasks to the second work period, such that in the absence of cognitive noise, allocating tasks to the sooner period is preferred to

this evidence, we believe the one hour period is sufficient to identify present bias (also see also see Balakrishnan et al. (2017) for recent discussion of this topic.

[21] In Section A.1, we outline the exact conditions under which the DM will appear more myopic when making 'fast' choices than after deliberation and successive simulations of the decision problem.

allocating tasks to the later period. Absent such a tradeoff—when the interest rate r is zero—there should be no differences between treatments with and without a waiting period.

Additionally, if waiting periods indeed prompt deliberation with respect to the choice set being considered—what we refer to as 'targeted deliberation'—then there should be no effect on choices that are not being considered or on decisions in different domains. This prediction contrasts with one where waiting periods prompt a general shift towards more deliberative decision-making across domains. Our Delay Control treatment, which did not present participants with information about the choice set before the delay, and results from the laboratory study can be used to test between these predictions.

Hypothesis 4. *Domain-specificity:* $x_1^{WP} > x_1^{DC}$ *and* $x_0^I = x_1^{DC}$.

Importantly, in the terminology of Halevy (2015), a difference between x_1^{WP} and x_1^{DC} represents a violation of time invariance, which stipulates that temporal allocations should be evaluated relative to "stopwatch time."

In the following section, we present results from the online labor market study testing the hypotheses. We then describe the setup of the laboratory experiment which provides a further test of the predictions.

2.1.3 Results

Participants' allocation decisions were responsive to the interest rates. Examining decisions on the convex budgets, pooled across treatments, participants allocated significantly more tasks to WP1 as the interest rate increased. When we regress tasks allocated to WP1 on the log interest rate (plus one), we find that a one-percent increase in the interest rate leads to roughly 0.14 additional tasks allocated to WP1 ($p < 0.001$), from an average of 28 tasks when the rate is zero.[22] This result also shows that subjects preferred to delay gratification in our study. We can reject that the 0% interest-rate allocation is a 50-50 split of tasks between WP1 and WP2 ($p < 0.001$). Considering the binary choices, we find more evidence of delayed gratification; while the majority of subjects allocated all tasks to WP2 when the interest rate was negative, 45% chose to do an extra five tasks in order to get them all done in WP1. When the interest rate was marginally positive, 91% allocated all tasks

[22]Estimate is from an OLS regression with standard errors clustered at the individual level.

to WP1. Together, these results confirm that participants were attentive to our manipulation of the interest rate, and show a general tendency to want to get the tasks done sooner rather than later. Only one of 122 subjects switched in the opposite-to-expected direction on the binary choices.

Turning to treatment differences, we examine allocation decisions on the convex budgets. Comparing the Immediate and Waiting Period treatments allows us to test Hypothesis 1 versus Hypothesis 3. As illustrated in Figure 2, Panel A, the results support Hypothesis 3. Participants allocated more tasks to WP1 in the Waiting Period treatment than in the Immediate treatment. Table 2 formalizes this finding with regressions of tasks allocated to WP1 on treatment dummy variables. Participants select corner solutions 56% of the time, with the vast majority of those being all tasks in WP1.[23] To handle this potential censoring issue, we follow the prior literature (e.g. Andreoni and Sprenger (2012)) in employing a two-limit Tobit regression. We also include results from OLS regressions to maintain simplicity and ease of interpretation. Furthermore, Appendix Table A.1 presents treatment effect estimates from Probit models of allocating all tasks to WP1.[24] The frequency of corner solutions in our data suggests a very high intertemporal elasticity of substitution between WP1 and WP2, which in turn indicates that the marginal disutility of work does not increase rapidly. It also means that there is no sense in which the marginal disutility of tasks in WP2 falls to zero when there are zero tasks allocated to WP2. Both of these facts are reflected in our structural estimates in the Appendix.

Columns (5) and (10) show the pooled estimates across budgets with different interest rates (with standard errors clustered at the individual level). The magnitude of the pooled Waiting Period effect is large and statistically significant: about 17% of the Immediate mean—35% according to the Tobit model—or half of a standard deviation.[25] The Tobit model produces a larger estimated effect because the Waiting Period treatment encouraged significantly more participants—27 percentage points from a baseline of 46% in Immediate—to allocate all tasks to WP1. Therefore, the effect on the latent allocation is larger than the effect on the censored allocation.

[23] This rate of corner choices is well within the range of prior studies using the convex time budget method. For example, Grijalva et al. (2018) find that 68% of participants select corners, while 70% of participants do so in Andreoni and Sprenger (2012)

[24] Very rarely ($< 4\%$ of the time) did participants allocate no tasks to WP1, so we do not analyze that margin.

[25] Given an $\alpha = 0.05$, the pooled effect has statistical power of 97%, assuming independence across tasks within an individual ($S.D.$ of all convex-budget tasks allocated to WP1 = 10.160).

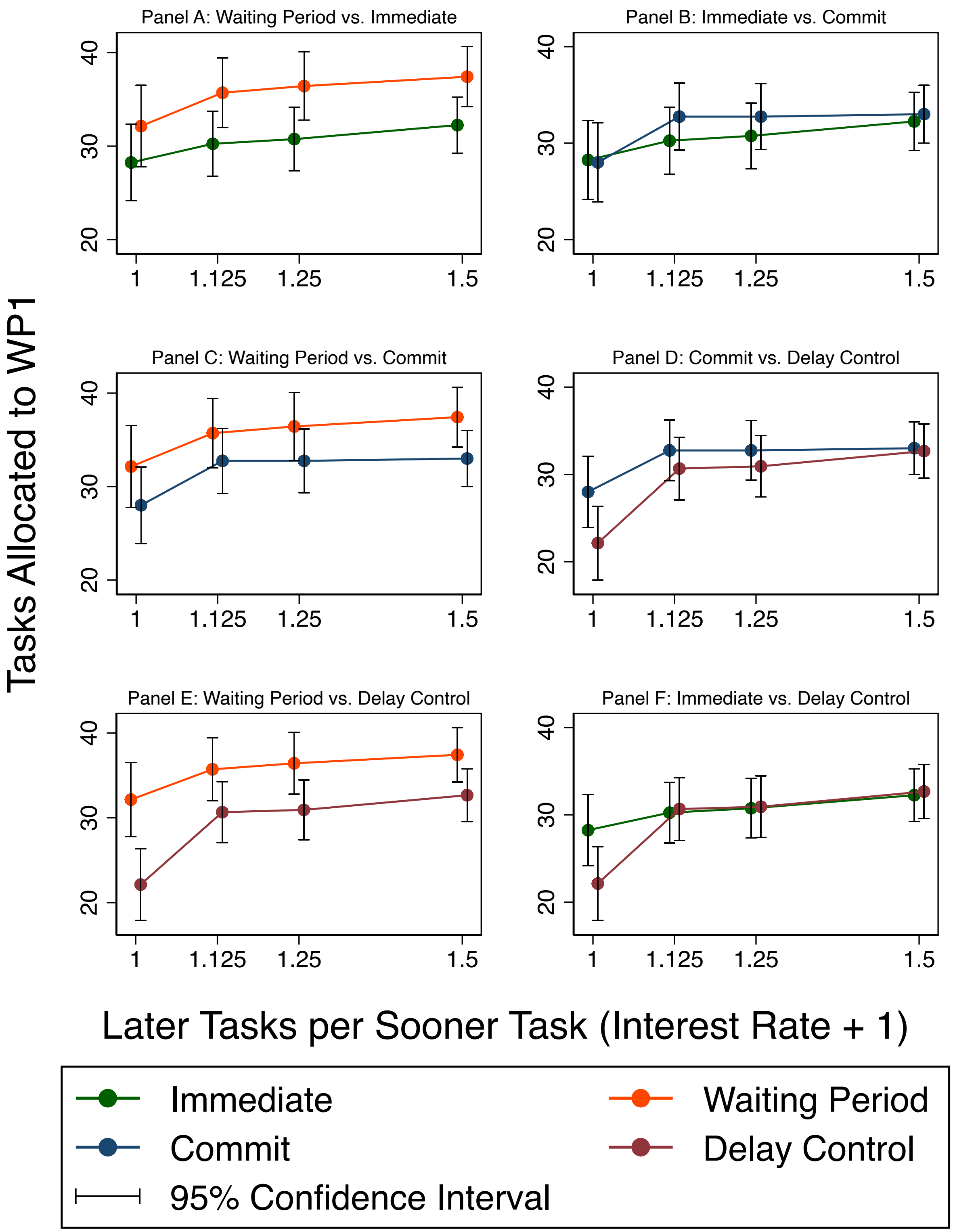

Figure 2: Tasks Allocated to Work Period 1, by Treatment, Online Study

Table 2: Effect of Treatment on Convex Task Allocations to Work Period 1, Online Study

Model:	OLS					Tobit				
Interest rate:	50%	25%	12.5%	0%	All	50%	25%	12.5%	0%	All
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Waiting Period	5.179**	5.679**	5.464**	3.893	5.054**	14.049**	15.914*	15.814*	7.351	13.002**
	(2.029)	(2.446)	(2.562)	(2.766)	(2.190)	(7.031)	(8.206)	(8.471)	(5.654)	(5.463)
Commit	0.750	2.000	2.500	-0.250	1.250	1.180	3.308	4.652	-0.282	2.168
	(2.234)	(2.340)	(2.350)	(2.702)	(2.257)	(5.060)	(5.318)	(5.567)	(4.924)	(4.831)
Delay Control	0.417	0.183	0.417	-6.117*	-1.275	0.540	0.465	0.750	-9.936*	-2.298
	(2.467)	(2.813)	(2.850)	(3.193)	(2.465)	(5.431)	(5.931)	(6.164)	(5.618)	(4.950)
Constant	32.250	30.750	30.250	28.250	30.375	39.686	37.637	37.450	33.046	37.238
	(1.757)	(1.869)	(1.899)	(1.927)	(1.793)	(4.026)	(4.278)	(4.460)	(3.707)	(3.842)
$\chi^2_1(H_0 : \text{WP} = \text{C})$	6.73***	3.04*	1.74	2.20	4.18**	3.81*	2.68	1.86	1.80	4.57**
$\chi^2_1(H_0 : \text{WP} = \text{DC})$	5.59**	4.30**	3.41*	9.47***	9.01***	3.69*	3.26*	2.94*	7.38***	8.10***
$\chi^2_1(H_0 : \text{C} = \text{DC})$	0.02	0.54	0.70	3.74*	1.34	0.02	0.26	0.44	3.30*	0.91
N	122	122	122	122	488	122	122	122	122	488

$^{***}: p < 0.01$, $^{**}: p < 0.05$, $^{*}: p < 0.1$. In columns (1)-(4) and (6)-(9), bootstrapped standard errors from 1000 replications are reported in parentheses below each estimate, to adjust for non-normality of the error distribution. Output is reproducible with a seed of 1. In columns (5) and (10), standard errors clustered at the individual level are reported in parentheses below each estimate. Tobit models adjust for the fact that we observe frequent corner solutions in which a subjects allocates a maximum of 40 or a minimum of zero tasks to WP1. The hypothesis tests report the chi-square statistics associated with tests of equality between the treatment effects, where WP stands for Waiting Period, C stands for Commit and DC stands for Delay Control.

We now examine the effect of the waiting period by budget. Budgets 1-3 offer a tradeoff between completing fewer tasks sooner versus a larger amount later, with implied interest rates of 50%, 25% and 12.5%, respectively. Decisions on Budget 4, which had a 0% implied interest rate, did not involve a tradeoff between fewer tasks now versus more tasks later. Regression estimates of the treatment effect on tasks allocated to WP1, separately by budget, are presented in columns (1)-(4) and (6)-(10) of Table 2. Participants in the Waiting Period treatment allocated significantly more tasks to WP1 than those in the Immediate treatment across all three budgets with positive interest rates. The effect sizes are about half of a standard deviation in each instance.[26]

Consistent with Hypothesis 3, waiting periods only led to significantly earlier allocations if this resulted in fewer tasks to complete overall, i.e. on budgets with positive interest rates. The treatment effect on the 0% interest rate budget shrinks by 28% and is not statistically different from zero. It is not statistically different from the estimates of the waiting period effect on the other budgets, however. Considering the binary-choice budget with a negative interest rate—meaning that subjects could do fewer tasks by waiting until WP2—we find that waiting periods made subjects 13% *more* likely to allocate all tasks to WP2, though the effect is not statistically significant (see Table A.1). When the interest rate is positive, waiting periods lead to more tasks being allocated to the sooner period; when the interest rate is negative, tasks are (directionally) more likely to be allocated to the later period. Together, these results offer suggestive evidence for individuals becoming better calibrated and less prone to making sub-optimal choices after waiting periods, rather than just shifting tasks to the later period in general.

Comparing decisions in the Commit treatment to those in the Waiting Period treatment allows us to test Hypothesis 2: whether the effects of waiting periods operate via sophisticated present bias and the ability to mentally commit to a plan. As shown in Figure 2, Panel C, participants in the Waiting Period treatment allocate more tasks to WP1 than those in the Commit treatment across the budgets. This suggests that the effect of the waiting period cannot be explained solely by models that relax the assumption of constant discounting such as present bias. Table 2 shows the effects of the Commit and Waiting Period treatment relative to the Immediate treatment. Pooling across budgets, the Waiting Period coefficient is four times the size of the Commit coefficient, and

[26]Looking at the positive interest-rate budgets separately, we have 64%, 59%, and 52% power to detect an effect size at the 5% as the interest rate decreases.

six times larger according to the Tobit model. Both differences are statistically significant. On each positive-interest rate budget, the Waiting Period coefficient is at least twice as large as the Commit coefficient. We can reject equality of the coefficients on budgets with $r = 0.5$ and $r = 0.25$ (only $r = 0.5$ for the Tobit models).[27]

As a robustness check and test of Hypothesis 4, we compare the Waiting Period treatment to choices in Delay Control. Figure 2, Panel E shows that the comparison between Waiting Period and Delay Control is very similar to that between Waiting Period and Immediate. In Table 2, the coefficient on the Waiting Period treatment is significantly different from the Delay Control treatment across all budgets. For the positive-interest rate budgets, allocation decisions in the Delay Control treatment were not significantly different from those in the Immediate treatment, as shown in Figure 2, Panel F. Participants in the Delay Control treatment allocated fewer effort tasks to WP1 when $r = 0$ compared to those in the Immediate treatment. In line with Hypothesis 4, waiting periods appear to only affect decisions that can be considered based on information available beforehand, which is not consistent with a general shift to more deliberative decision-making.

In Section A.2, we estimate the parameters of an intertemporal utility function that permits both present bias and an as-if discounting simulation parameter. We allow for spillover of effort across work periods, and background work requirements in our model. Structural estimation of such parameters is common in the experimental literature on time discounting. This allows us to compare our estimate of present bias over effort to other estimates in order to gauge the comparability of our procedure. As discussed earlier, the short-horizon nature of the study and factors orthogonal to time preferences in participants' effort allocation decisions complicate inference of long-horizon discount rates, δ. In turn, our estimation procedure focuses on parameters related to myopia that can be identified directly through treatment variation. We identify present bias of similar magnitude to estimates in prior work on effort: $\beta = 0.912$ (S.E. = 0.042, $p = 0.037$, tested against $\beta = 1$).[28] We also estimate a "simulation parameter," S, that can be interpreted as a multiplicative factor that scales the discount factor when there is no time for deliberation. Like

[27] For Budgets 1-3, the coefficients of the Commit treatment are all positive though not significant, providing suggestive evidence of present bias. This is also shown in Figure 2, Panel B. In Section A.2 we estimate a structural model that identifies a present bias parameter $\beta < 1$ in a model of quasi-hyperbolic discounting. However, consistent with the reduced form results, it does not explain the full impact of waiting periods on choices.

[28] Augenblick et al. (2015) estimate $\beta = 0.90$.

β, S is significantly less than one: $S = 0.841$ (S.E. = 0.046, $p = 0.001$, tested against $S = 1$). S is also significantly different from β ($p = 0.045$). In sum, while our design captures significant present-bias, the independent effect of waiting periods on intertemporal decisions is significantly larger both in a statistical and economic sense. Details on the utility maximization problem, its solution, the censored-Tobit maximum likelihood estimations procedure, and the full set of estimates are presented in the Section A.2.

2.2 Laboratory Study

2.2.1 Design and Implementation

In the online labor market study, we lacked precise information on what participants did during the waiting periods. Though exit survey responses suggest that participants largely took this time to engage in unrelated activities like reading a book or cooking a meal, the setup does not allow us to completely rule out alternative mechanisms. For example, participants may have used the waiting period to consult with others.

To investigate whether such alternative explanations could be driving the waiting period effect—and to increase the statistical power of our waiting period test—we replicated the effort study in the laboratory where activities available to the participants were limited and observable. We recruited participants (N = 72) for a study at Carnegie Mellon University's Center for Behavioral Decision Research. The advertised study duration was 3.5 hours, with a reward of $50 for completion of the study.

Participants in the study faced the same choices and the same interface as those in the online study, which allows us to analyze the data both separately and pooled with the online study. Upon arriving to the lab, participants were randomized into either the Immediate or the Waiting Period treatment. One major difference in protocol for the lab study was that we were able to explicitly prohibit communication and limit the types of activities participants could engage in during the waiting period and other intervals of free time. No cell phone usage was allowed, and internet use was limited to watching media (access to an online streaming site and headphones were provided). Subjects were also encouraged to read material they had brought to the lab. Research assistants were instructed to monitor activity during waiting periods and free time, and responses to exit surveys are consistent with participants engaging in the suggested activities.[29]

We collected additional data in this study to allow us to test Hypothesis 4. Immediately following the task allocation choices, we embedded questions designed to test the propensity for automatic decisions versus deliberative processing. The question set consisted of seven Cognitive Reflection Test questions (CRT from Frederick (2005), CRT2 from Thomson and Oppenheimer (2016)).[30] Participants were not given information about these questions at the onset of the experiment; unlike the task allocation decisions, participants in the Waiting Period treatment could not deliberate about them during the waiting period. If waiting periods lead to a general shift towards more deliberative processing, then we should observe a treatment effect on the number of questions answered correctly. Hypothesis 4 predicts that there should be no such effect because these questions are in a different domain than the information presented before the waiting period.

2.2.2 Results

Results from the laboratory are consistent with those from the online labor market. The average number of tasks allocated to WP1 across all convex budgets is 31.6 ($S.D. = 8.7$) in the lab study and 31.5 ($S.D. = 10.6$) in the online labor market study. Participants were slightly less responsive to changes in the interest rate. When we regress tasks allocated to WP1 on the log interest rate (plus one), we find that a one-percent increase in the interest rate leads to roughly 0.08 additional tasks allocated to WP1 ($p < 0.01$), from an average of 29 tasks when the rate is one.[31] The general preference for delayed gratification and the frequency of corner solutions is almost identical in the laboratory and online settings.

Consistent with the third hypothesis, participants allocated more tasks to WP1 in the Waiting Period treatment than in the Immediate treatment. This is shown in Figure 3, and estimated via OLS and Tobit regression in Table 3 (Panels A and B, respectively). The pooled OLS estimate in column (5) of Panel A shows a statistically significant Waiting Period effect size of 14%, similar

[29]One participant was observed trying to use a cell phone, and is excluded from the analysis. Inclusion of their data does not affect our results.

[30]For example, participants were asked "A bat and a ball cost $1.10 in total. The bat costs $1.00 more than the ball. How much does the ball cost?" Kahneman and Frederick (2002) and Frederick (2005) argue that the 'fast' automatic response is $0.10; overriding this heuristic response through deliberative processing leads to the correct answer, $0.05.

[31]Estimate is from an OLS regression with standard errors clustered at the individual level.

to our OLS estimate of 17% in the online labor market study.[32] In both studies, this is roughly half a standard deviation. As with the online study, the Tobit estimate of 25% is larger than the OLS estimate. Probit estimates in Table A.2 show that the waiting period similarly increased the frequency of doing all the tasks in WP1.

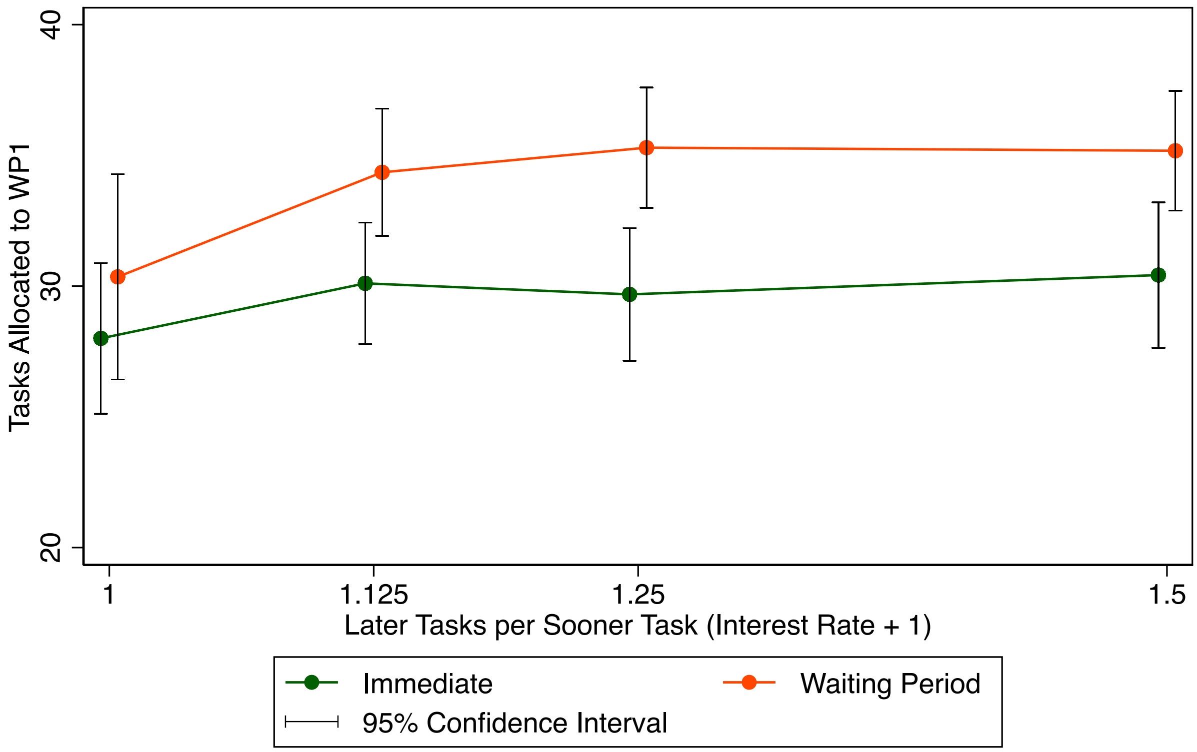

Figure 3: Tasks Allocated to Work Period 1, by Treatment, Lab Study

Table 3, columns (1)-(4) presents results separately for each convex budget. Again, participants in the Waiting Period treatment allocated significantly more tasks to WP1 than did those in the Immediate treatment across all three convex budgets with a positive interest rate.[33] The size of the effect shrinks by 51%, moving from the positive-interest budgets to the zero-interest budget, and is no longer significantly different from zero. Results are qualitatively similar for the Tobit estimates shown in Panel B, although slightly large in magnitude. Overall, these results are consistent with those from the online labor market study, indicating that the observed effects were not driven by access to communication or other activities pursued during the waiting period.

[32] Given an $\alpha = 0.05$, this pooled effect has statistical power of 99%, assuming independence across tasks within an individual ($S.D.$ of all convex-budget tasks allocated to WP1 = 8.657).

[33] Looking at the positive interest-rate budgets separately, we have 69%, 85% and 67% power to detect an effect size at the 5% level as the interest rate decreases.

Table 3: Effect of Treatment on Convex
Task Allocations to Work Period 1, Lab Study

Interest rate:	50%	25%	12.5%	0%	All
	(1)	(2)	(3)	(4)	(5)
Panel A: OLS Models					
Waiting Period	4.755***	5.610***	4.248**	2.353	4.241***
	(1.791)	(1.721)	(1.712)	(2.416)	(1.635)
Constant	30.421	29.684	30.105	28.000	29.553
	(1.406)	(1.288)	(1.204)	(1.429)	(1.105)
Panel B: Tobit Models					
Waiting Period	9.137**	10.453***	7.528**	4.627	8.148**
	(3.655)	(3.135)	(3.083)	(4.119)	(3.130)
Constant	33.273	31.282	32.367	31.350	32.121
	(2.427)	(1.893)	(2.154)	(2.767)	(1.954)
N	72	72	72	72	288

*** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.10$. In columns (1)-(4), bootstrapped standard errors from 1000 replications are reported in parentheses below each estimate, to adjust for non-normality of the error distribution. Output is reproducible with a seed of 1. In column (5), standard errors clustered at the individual level are reported in parentheses below each estimate. Tobit models adjust for the fact that we observe frequent corner solutions in which a subjects allocates a maximum of 40 or a minimum of zero tasks to WP1.

Next, we consider the impact of the waiting period on choices unrelated to the intertemporal task budgets. If waiting periods lead to a general shift towards more deliberative decision-making, then we expect to see higher scores on the Cognitive Reflection Test questions in that treatment. This is not what we find. In the Immediate treatment, the average CRT score is 1.97 correct answers (out of three), and in the Waiting Period treatment, the average score is 2.00 ($p = 0.92$). The average CRT2 score is 2.53 (out of four) in Immediate and 2.50 in Waiting Period ($p = 0.93$). Consistent with Hypothesis 4 and the 'targeted deliberation' account, the waiting period appears to have little to no impact on decision-making outside the domain of the task being considered.[34]

Given the similarities between the laboratory and online data, we consider the statistical power of our waiting period effect given the pooled samples. We have a total of 528 convex-budget

[34] Additionally, we can use the CRT scores to examine whether waiting periods differentially impact people with different cognitive dispositions. We find no significant interaction effects between CRT scores and waiting periods on the task allocations, suggesting that waiting periods are equally effective in mitigating myopia along the dimension captured by the CRT measure.

choices from 132 participants in Immediate and Waiting Period across the two studies. Table A.3 reproduces columns (1)-(5) of Table 2 and Table 3, Panel A for the pooled study sample. The effects of the waiting period on the 50% and 25% interest-rate budgets are significant ($p < 0.01$), and our statistical power of the effect sizes is 92% and 95% respectively. The effect of the waiting period on the 12.5% interest-rate budget is also precisely estimated ($p < 0.01$), with a power of 87%. The pooled estimate across all of the convex budgets is very precise ($p < 0.01$), and assuming statistical independence, power of our study to detect this effect at the 5% level approaches 100%.

3 Waiting Periods for Consumption Goods

3.1 Design and Implementation

Our third study demonstrates an application of waiting periods in a field context, with a different good, and over a longer decision horizon. We partnered with a small grocery store in a residential area in Bukavu, a city on the Eastern border of the Democratic Republic Congo (DRC). The store sells everyday goods and simple foodstuffs like rice, water, and milk. It also has access to electricity and refrigeration that is lacking in most homes, and the vast majority of the people in our sample visited the store every day to pick up groceries. The store ran as usual during the study and was staffed by the family that has owned and operated it for the past decade in order to avoid disrupting customers' familiarity with the store and to reduce uncertainty related to the experiment taking place. One of the authors supervised all aspects of the procedures for the entire length of the experiment.

A total of 258 store customers participated in the study. Each made a decision of when to redeem a coupon for a set amount of flour. Cassava flour is a staple crop and consumption good in the Eastern DRC, particularly for making *fufu* and *chikwangue*, both dough preparations. Cassava products, in general, contribute to about 65% of daily calories consumed in the DRC, and are the main food crop for 80% of the population, which translates to about 0.4kg per-capita daily—mostly from fufu and chikwangue (Harvest Plus, 2010). The coupons were redeemable for flour that the store typically sold, not a new unfamiliar product.

Upon arriving at the store and agreeing to participate, all customers completed a demographic survey.[35] Participants who were illiterate or had difficulty completing the survey on their own were helped by a research assistant who was blind to the hypothesis. The survey was in both Swahili and French and the participants chose which was more convenient for them. On average the survey took 30 minutes to complete.[36]

Participants were then randomly assigned to one of two treatments—Immediate or Waiting Period. In both, they received a coupon that could be exchanged for varying amounts of flour depending on the day it was redeemed.[37] In the Immediate treatment, the participant could redeem the coupon on the same day for 1 bag of flour (approximately 1kg). If she chose not to redeem it on that day, she could come back the next day for 2 bags, and so on, up until 5 bags of flour. The Waiting Period treatment shifted the redemption schedule by one day: the participant had to wait a day before deciding whether to redeem the coupon for 1 bag of flour. As in the Immediate treatment, if she chose not to redeem the coupon on the day after the waiting period, she could come back the next day for two bags of flour, and so on, up until 5 bags of flour. Table 4 presents the coupon value schedule. The study was run over a number of days, meaning that treatment status and the calendar date of redemption options are not co-linear. Due to the material incentives and participants' daily visits to the store, only one participant did not redeem their coupon by the last possible day (this individual was in the Waiting Period treatment).

Table 4: Coupon Value over Time - kg of Flour

Treatment:	Immediate	Waiting Period
Day of Receipt	1	0
1 Day after Receipt	2	1
2 Days after Receipt	3	2
3 Days after Receipt	4	3
4 Days after Receipt	5	4
5 Days after Receipt	0	5
6 Days after Receipt	0	0

Our key dependent measure is the likelihood that an individual redeemed her coupon for its

[35]The survey was presented in the beginning of the experiment in order to collect demographic information and other variables of interest in case of differential attrition. As discussed further below, attrition turned out to be minimal.
[36]Full questionnaire available in the AEA Data and Code Repository.
[37]Each coupon had an ID matching it with a questionnaire, a date of issue, and a code signifying the treatment.

minimum value of one bag of flour. This measure is used for the following reasons. Once an individual in the Immediate treatment chooses not to redeem their coupon on the day it is received, she experiences an overnight waiting period. Additionally, given the expected difference in minimum-value redemption rates, the individuals who chose not to redeem on the earliest possible date in the Immediate treatment will be more selected relative to those who chose not to redeem in the Waiting Period treatment. Particularly, those who resist the urge to redeem in the Immediate treatment may be more patient on average than those who resist after a waiting period. This makes comparisons of choices after the earliest possible redemption date subject to selection issues. Looking at minimum-value redemption as the main dependent variable is the closest analog to our online and lab studies.

Several aspects of our study may limit causal inference. First, there may be transaction costs associated with returning to the store, which are constant across all redemption dates in the Waiting Period treatment, but not in the Immediate treatment because participants were recruited at the store. There may also be trust issues associated with redemption of coupons in the future which matter in Immediate treatment but are held constant in the Waiting Period condition. We note that the store was a common daily destination for the participants, and that they presumably had means to purchase food without the existence of our study (as they were recruited in the store). In addition, we collected data on both food access and the distance participants lived from the store. We use this data as well as measures of risk and trust attitudes as controls in our analyses. However, these steps may not fully rule out the highlighted issues. In turn, we view the results from our field study as complimentary to the better-identified online and laboratory studies, which demonstrate similar results in a more controlled environment.

3.2 Results

Responses on the questionnaire are used to verify that key demographic and preference variables were uncorrelated with treatment assignment. The frequency of significant differences is consistent with random assignment (see Table A.4). Most importantly, neither measures of trust of others, stated preference for risk, nor food access were correlated with treatment assignment.

Consistent with Hypothesis 3, the introduction of a waiting period has a substantial effect on minimum-value redemption rates: 34 individuals (25%) in the Immediate treatment redeemed the coupon on the earliest possible date, compared to 11 (9%) in the Waiting Period treatment. The 16 percentage-point difference is statistically significant ($p < 0.01$).[38] Results are presented in Table 5. This estimate is robust to the addition of control variables and their interactions with the treatment variable; including food access, distance from the store, trust in other and risk tolerance does not diminish the size of the treatment effect, and none of the variables interact significantly with treatment.[39] Probit model marginal effects show a slightly larger waiting period effect.

Examining choices after the earliest redemption date suggests that indeed, the sample of individuals who resisted the urge to redeem the coupon in the Immediate treatment were more patient after an overnight waiting period than those who resisted in the Waiting Period treatment. Conditional on not redeeming earliest possible date, those in the Waiting Period treatment redeemed their coupon 0.73 days sooner than those in the Immediate treatment ($p < 0.01$).[40]

Table 5: Impact of Waiting Period on
Likelihood of Minimum-value Coupon Redemption

Model:	OLS		Probit	
	(1)	(2)	(3)	(4)
Waiting Period	-0.160***	-0.166***	-0.211***	-0.243***
	(0.045)	(0.047)	(0.074)	(0.082)
Constant	0.250	0.253	0.250	0.251
	(0.037)	(0.039)	(0.037)	(0.038)
Food, Distance, Trust, Risk controls	N	Y	N	Y
All controls interacted with WP	N	Y	N	Y
N	258	252	258	252

*** : $p < 0.01$. Coefficients from Probit models are the marginal effects associated with switching from the Immediate to Waiting Period treatment. Robust standard errors are reported in parentheses below each estimate. Controls in columns (2) and (4) include Food access, trust in others and risk tolerance (all measured on 1-4 scales), and distance from the store (measured on a 1-3 scale). All control variables are de-meaned. We lose six observations with the addition of control variables due to incomplete survey responses.

[38] We have 94% statistical power of detecting this effect at the 5% level.
[39] For space, we report only the coefficient on the treatment variable here. Full estimates are in Table A.5.
[40] This is driven by a higher likelihood of maximum redemption in the Immediate treatment, whereas most individuals in the Waiting Period treatment redeem their coupon for four bags of flour.

4 Discussion

Three studies demonstrate the significant effect of waiting periods on myopia. When an intertemporal choice is preceded by a waiting period, participants in an effort allocation task choose to complete more tasks earlier, thereby minimizing overall work time and unpleasant effort. This effect cannot be explained by non-constant discounting such as present-biased time preferences. In our first study, the effect of temporally separating the receipt of information about a choice and the choice itself is stronger than the effect of temporally separating the choice and its consequences, i.e. the ability to commit to a future allocation. Lastly, we demonstrate an application of waiting periods in a field setting, showing that grocery store customers are less likely to redeem a coupon that grows in value over time for its minimum value when the initial redemption choice is preceded by a waiting period.

A noteworthy aspect of our results is that we observed similar directional effects of waiting periods on intertemporal choices across contexts with different baseline behaviors. Our effort studies featured intertemporal choices over a short horizon: the decision of when to complete a series of work task over two consecutive periods. On the other hand, our consumption study featured intertemporal choices over a longer horizon: the decision of when to cash-in an asset that could grow for up to four days. While this is again a relatively short horizon, it features very different baseline preferences: a non-trivial fraction of subjects make the seemingly very myopic decision to redeem the coupon for its minimum value, and the mean redemption time is just over two days. Despite this difference in the type of choice and study populations, the causal impact of waiting periods is sizable and in the same direction: towards choices that decrease costs (effort studies) or increase benefits (consumption study) over the respective time horizon.

Additional work is needed to determine if these results extend to longer horizons and choices with higher stakes. In particular, examining the impact of waiting periods on intertemporal money allocations like saving and borrowing would be particularly valuable, given both the wide set of applications, and the literature contrasting consumption and money discounting. Another important issue that we cannot speak to with these studies is what makes an effective waiting period. How long does it need to be, should there be an explicit suggestion of deliberation, and does their effectiveness diminish with repeat use? We leave this for future work.

Our results highlight the potential use of introducing delays between information and choice in the design of policy and choice architecture. Economists have noted the lack of demand for commitment devices, which contrasts with the predictions of some discounting models designed to capture myopic behavior (Laibson, 2015).[41] Since waiting periods do not restrict individuals' choice or information sets, they may represent a more feasible policy tool for mitigating myopia than other interventions. Consider the tax refund example from the introduction. Our results suggest that eliminating the waiting period between being informed of the refund and the ability to use the windfall could have a significant impact on the choice of whether to spend or save the money. A firm offering to deliver the refund immediately through an Anticipation Loan, for example, may create substantial negative downstream consequences for the consumer even if the loan were interest-free.

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A Appendix for Online Publication

Waiting to Choose

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A.1 Relationship between Deliberation Time and Intertemporal Choice

In this section, we derive predictions of a simplified version of the imperfect foresight model of Gabaix and Laibson (2017) for our setting.

Hypothesis 3: Consider a decision-maker (DM) chooses between (u_0^E, u_1^L) and (u_0^L, u_1^E). For all $i \in \{E, L\}$, u_0^i, is received immediately and u_1^i is received in the following period. The DM knows the value of u_0^i with certainty, but lacks perfect information on the 'true' value of u_1^i and must generate simulations to forecast it. In the context of the first two studies, let (u_0^L, u_1^E) represent the utility from choosing to have only leisure time in WP1 such that all effort tasks are allocated to WP2, and (u_0^E, u_1^L) represent the utility of having only leisure in WP2 such that all effort tasks are allocated to WP1. We consider the case where the DM faces a tradeoff of allocating tasks to WP2, such that she has to do more total tasks when she chooses (u_0^L, u_1^E) than (u_0^E, u_1^L). Thus, $u_0^L = u_1^L > u_0^E > u_1^E$. We refer to the (u_0^E, u_1^L) as the patient choice and (u_0^L, u_1^E) as the impatient choice.

Following Gabaix and Laibson (2017), normalize the DM's prior on u_1^i to zero such that $u \sim N(0, \sigma_u^2)$. This can be interpreted as the average utility that could be realized in WP2 given the choice set available to the DM. We consider the case where waiting periods prompt additional simulations relative to when no waiting periods. When the DM performs her first simulation of u_1^i, she draws an unbiased signal of its value $s_{1,1}^i = u_1^i + \epsilon_{1,1}$, where the first term in the subscript $(1, 1)$ corresponds to the time horizon and the second to the order of the signal drawn. The simulation noise $\epsilon_{1,1}$ is drawn from $\epsilon_1 \sim N(0, \sigma_{\epsilon_1}^2)$. Since we only consider a one-period time horizon, $\sigma_{\epsilon_1}^2 = \sigma_\epsilon^2$.[42] As a Bayesian, she integrates this signal with her prior. The DM's posterior forecast of u_1^i can be represented as $Ds_{1,1}^i$, where $D = \frac{1}{1+\sigma_\epsilon^2/\sigma_u^2}$. Integrating over the distribution of signals, we get $E_1(u_1^i) = Du_1^i$.

[42]This is consistent with a one-period simulation under the proportional variance assumption of Gabaix and Laibson (2017), $\sigma_{\epsilon_t} = t \cdot \sigma_\epsilon$.

Before the initial simulation, the DM values the patient choice as u_0^E and the impatient choice as u_0^L, and thus prefers the impatient choice. After the first simulation, the average DM values the patient choice as $u_0^E + Du_1^L$ and the impatient choice as $u_0^L + Du_1^E$. Because $u_1^L > u_1^E$, it is straightforward to show that the DM's valuation of the patient choice increases after the initial simulation.

To illustrate how successive simulations increase the valuation of the patient choice relative to the impatient choice, let the DM draw a second signal $s_{1,2}^i = u_1^i + \epsilon_{1,2}$. She again updates her beliefs and obtains the posterior

$$Ds_{1,1}^i + D(s_{1,2}^i - Ds_{1,1}^i) = D(1-D)s_{1,1}^i + Ds_{1,2}^i \tag{4}$$

from Proposition 1 of Gabaix and Laibson (2017). Integrating over the distribution of signals, we get

$$E_2(u_1^i) = D(1-D)u_1^i + Du_1^i = D(2-D)u_1^i \quad . \tag{5}$$

To illustrate the result, take a DM who is indifferent between the two choices after an initial simulation, such that her forecasted utility in expectation can be represented as

$$u_0^E + E_1(u_1^L) = u_0^L + E_1(u_1^E) \tag{6}$$

After the second simulation, the left hand side becomes $u_0^E + E_2(u_1^L)$ and the right hand side becomes $u_0^L + E_2(u_1^E)$. The change in valuation of the patient choice is thus the change in the expectation of u_1^L: $D(2-D)u_1^L - Du_1^L = D(1-D)u_1^L$. Correspondingly, the change in the value of the impatient choice is $D(1-D)u_1^E$. The difference in changes between the patient and impatient choices is $D(1-D)(u_1^L - u_1^E)$. Because $D \in (0,1)$, and $u_1^L > u_1^E$, the expression is positive, meaning that relative preference for the patient choice has increased. Therefore, the DM who was indifferent after one simulation – and thus preferred the impatient choice before any simulations – selects the patient option after two simulations.

More generally, define $\gamma(N) \in [0,1]$ as the relationship between deliberation time, N, and simulation noise γ, with $\gamma'(N) < 0$. The Bayesian updating factor becomes an "as-if" discount factor $D(N) = \frac{1}{1+\gamma(N)\alpha}$ where $\alpha = \frac{\sigma_\epsilon^2}{\sigma_u^2}$. Because $\gamma(N)$ is decreasing in N, $D(N)$ is decreasing in

N, and additional simulations lead the decision maker closer to forecasting $u_1^L = u_0^L > u_0^E > u_1^E$ without noise, implying $(u_0^E, u_1^L) \succ (u_0^L, u_1^E)$.[43]

A.2 Structural Estimation in the Online Effort Allocation Study

In this section, we discuss estimates of the utility parameters from equations (1), (2) and (3). Since participants make allocation decisions between two periods, each treatment on its own only reveals their one-hour discount factor for task effort. Because the timing of the work periods and the allocation decision differs by treatment, the variation in the theoretical interpretation of that discount factor allows us to identify the parameters of interest. Specifically, the treatments were designed to separately identify aggregate estimates of the exponential discount factor δ, the present bias parameter β, and the simulation parameter $S_k(t)$. The parameter $S_k(t)$ is meant to capture the effect of additional simulations of the decision problem prompted by the waiting period. In the application of the Gabaix and Laibson (2017) framework outlined in Section 2.1.2, the parameter can be represented as $S_k(t) = \frac{D_k(t)}{D_{k+1}(t)}$. Given the short horizon and the fact that our experiment manipulates the waiting period over only one interval, we drop the subscripts for the analysis, setting $S_k(t) = S$.

Our identification strategy is as follows. Participants in the Waiting Period treatment solve the optimization problem in equation (2), as laid out in Section 2.1.2. We allow for present bias, such that the discount factor between periods is equal to $\frac{D_1(1)}{D_1(0)} = \beta\delta$. Participants in the Immediate treatment solve a similar problem, shifted back by one period as in equation (1). The parameter S identifies any additional discounting that occurs in the Immediate treatment that does not occur in the Waiting Period treatment. Therefore, the discount factor in the Immediate treatment can be represented as $\frac{D_0(1)}{D_0(0)} = S\beta\delta$. We obtain an estimate of S as the ratio of the Immediate discount factor to the Waiting Period discount factor.

Participants in the Commit treatment maximize equation (3). At $t = 0$, subjects allocate tasks between $t = 1$ and $t = 2$. Because choices are made in the absence of a waiting period, the discount factor can be represented as $\frac{D_0(2)}{D_0(1)} \approx S\delta$.[44] In turn, we obtain an estimate of β as the ratio

[43] Sincere thanks to an anonymous referee for helpful, detailed comments on this section.

[44] This is an approximation. Since the variance in forecasts of future utility is increasing in their time horizon, the as-if discounting that occurs in the Commit treatment is between one and two periods in the future, whereas in the Immediate treatment, it is between one period in the future and the present, which is subject to no uncertainty.

of the Immediate discount factor to the Commit discount factor.

Call z_1 tasks allocated to Work Period 1 and z_2 tasks allocated to Work Period 2 and r the the interest rate by which undone tasks grow. The general convex intertemporal allocation decision in our study is

$$\min_{z_1,z_2} U(z_1, z_2) = z_1^\gamma + \delta_T z_2^\gamma \quad \text{s.t.} \quad z_1 + \frac{z_2}{1+r} = 40 \quad . \tag{7}$$

γ is the instantaneous disutility of effort parameter, and δ_T is a treatment-specific discount factor, which we map to the parameters of interest with the across-treatment comparisons mentioned above.

We make two additional adjustments to allow for more flexibility in our model of effort cost. First, we add background parameters ω_1 and ω_2 to the tasks required in each period to represent other effort that might need to be expended during those time periods. Second, we allow for the possibility of less-than complete recovery after Work Period 1 with another background effort parameter, ω_3, that enters as a coefficient on z_1 in the Work Period 2 effort level. The utility function is thus

$$U(z_1, z_2) = (z_1 + \omega_1)^\gamma + \delta_T(z_2 + \omega_2 + \omega_3 z_1)^\gamma \quad . \tag{8}$$

We use the solution to the utility maximization problem to set up a maximum-likelihood estimation. The supply of tasks in Work Period 1 is

$$z_1^* = \frac{40A(1+r) + \omega_2 A - \omega_1}{1 + A(1+r) - \omega_3 A} \quad , \tag{9}$$

where $A = \left(\delta_T(1 + r - \omega_3)\right)^{\frac{1}{\gamma-1}}$. Individuals, i, solve this problem for each choice, j, and select the nearest available option subject to a standard normal error term, $\epsilon_{i,j}$, such that

$$z_{1,(i,j)} - \frac{40A_t(1+r_j) + \omega_2 A_j - \omega_1}{1 + A_j(1+r_j) - \omega_3 A_j} + \epsilon_{i,j} = 0 \quad , \tag{10}$$

where $z_{1,(i,j)}$ is our observed choice for period 1 tasks by person i on task j. The likelihood

Assuming a linear increase in simulation variance and time period, which leads to a hyperbolic as-if discount factor, the S in Commit is slightly closer to one than the S in Immediate. Our estimate of β is thus a lower bound on the quasi-hyperbolic discount factor.

associated with that observation is

$$\phi\left(z_{1,(i,j)} - \frac{40A_j(1+r_j)+\omega_2 A_j - \omega_1}{1+A_j(1+r_j)-\omega_3 A_j}\right) \tag{11}$$

When subjects select corner solutions from the convex choice sets, the convex first order conditions may poorly approximate choices. Therefore, we assume censoring at each corner as in a Tobit model. If $z_{1,(i,j)} = 0$, then we assume that

$$\epsilon_{i,j} > \frac{40A_j(1+r_j)+\omega_2 A_j - \omega_1}{1+A_j(1+r_j)-\omega_3 A_j} \quad , \tag{12}$$

and the likelihood contribution is

$$\Phi\left(-\frac{40A_j(1+r_j)+\omega_2 A_j - \omega_1}{1+A_j(1+r_j)-\omega_3 A_j}\right) \quad . \tag{13}$$

If $z_{1,(i,j)} = 40$, then we assume that

$$\epsilon_{i,j} < \frac{40A_j(1+r_j)+\omega_2 A_j - \omega_1}{1+A_j(1+r_j)-\omega_3 A_j} - 40 \quad , \tag{14}$$

and the likelihood contribution is

$$\Phi\left(\frac{40A_j(1+r_j)+\omega_2 A_j - \omega 1}{1+A_j(1+r_j)-\omega_3 A_j} - 40\right) \quad . \tag{15}$$

In our two binary choice tasks, subjects simply select the smaller value between $(40+\omega_1)^\gamma$ and $\delta_T(40(1+r)+\omega_2)^\gamma$. We make the standard Probit model assumption that the difference between the two utilities is subject to a normal distribution. Thus the probability of observing all work in the first period is

$$Pr(z_{1,(i,j)} = 40) = Pr((40+\omega_1)^\gamma - \delta_T(40(1+r_j)+\omega_2)^\gamma + \epsilon_{i,j} < 0) = \\ \Phi(\delta_T(40(1+r_j)+\omega_2)^\gamma - (40+\omega_1)^\gamma) \tag{16}$$

and the probability of observing all work in the second period is

$$Pr(z_1 = 0) = Pr((40 + \omega_1)^\gamma - \delta_T(40(1 + r_t) + \omega_2)^\gamma + \epsilon_{i,j} > 0) = \\ \Phi((40 + \omega_1)^\gamma - \delta_T(40(1 + r_j) + \omega_2)^\gamma). \quad (17)$$

These probabilities are used to construct the likelihood function. In the estimation, we impose the restrictions that $\gamma > 0$ and that $\omega_1, \omega_2, \omega_3 > 0$ to prevent degenerate results.

We estimate $\gamma = 1.255$ (S.E. = 0.047), indicating increasing marginal disutility of performing the counting task. There is no evidence on any background effort level in Work Period 1 ($\omega_1 = 0$), but there is evidence of background effort in Work Period 2 ($\omega_2 = 4.711$, S.E. = 2.455). Additionally there is some evidence of effort spillover across period ($\omega_3 = 0.253$, S.E. = 0.017). We estimate treatment-specific discount factors of $D_I = 0.968$ (S.E. = 0.071), $D_{WP} = 1.151$ (S.E. = 0.113), $D_C = 1.062$ (S.E. = 0.087), and $D_{DC} = 0.878$ (S.E. = 0.061). The very short time horizon means that we should expect very little discounting. Indeed, discount factors D_I, D_W, D_C do not significantly differ from one ($p = 0.65, 0.18$ and 0.48, respectively); only the Delay Control estimate D_{DC} does ($p = 0.05$). Estimates of S and β are discussed in the text.

A.3 Tables

Table A.1: Effects of Treatment on Doing All Tasks in Work Period 1 on Convex Task Allocations, Probit Models, Online Study

Sample:	Convex Choices					Binary Choices	
Interest rate:	50%	25%	12.5%	0%	All	-12.5%	2.5%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Waiting Period	0.284**	0.346**	0.346**	0.155	0.274**	-0.133	-0.008
	(0.142)	(0.134)	(0.134)	(0.117)	(0.111)	(0.126)	(0.058)
Commit	-0.000	0.031	0.062	0.000	0.023	-0.062	0.040
	(0.125)	(0.125)	(0.125)	(0.122)	(0.114)	(0.123)	(0.084)
Delay Control	0.002	0.031	0.031	-0.115	-0.011	-0.269***	-0.085***
	(0.127)	(0.127)	(0.127)	(0.133)	(0.112)	(0.123)	(0.032)
Constant	0.531	0.469	0.469	0.375	0.461	0.563	0.938
	(0.089)	(0.089)	(0.089)	(0.086)	(0.082)	(0.088)	(0.043)
$\chi^2_1(H_0 : \text{WP} = \text{C})$	4.20**	5.18**	4.20**	1.53	4.84**	0.30	0.43
$\chi^2_1(H_0 : \text{WP} = \text{DC})$	4.03**	5.04**	5.04**	4.22**	6.50**	1.02	1.26
$\chi^2_1(H_0 : \text{C} = \text{DC})$	0.00	0.00	0.06	0.82	0.10	2.54	1.86
N	122	122	122	122	488	122	122

$^{***}: p < 0.01$, $^{**}: p < 0.05$, $^{*}: p < 0.10$. Coefficients are the marginal effects of each treatment on the probability a subject allocates all tasks to WP1. In columns (1)-(4) and (6)-(7), robust standard errors are reported in parentheses below each estimate. In column (5), standard errors clustered at the individual level are reported in parentheses below each estimate. The hypothesis tests report the chi-square statistics associated with tests of equality between the treatment effects, where WP stands for Waiting Period, C stands for Commit and DC stands for Delay Control.

Table A.2: Effects of Treatment on Doing All Tasks in Work Period 1 on Convex Task Allocations, Probit Models, Lab Study

Sample:	Convex Choices					Binary Choices	
Interest rate:	50%	25%	12.5%	0%	All	-12.5%	2.5%
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Waiting Period	0.259***	0.338***	0.250***	0.171*	0.258***	-0.105	-0.029†
	(0.098)	(0.070)	(0.093)	(0.097)	(0.044)	(0.086)	
Constant	0.342	0.237	0.316	0.316	0.303	0.763	1.000†
	(0.078)	(0.069)	(0.076)	(0.076)	(0.037)	(0.069)	
N	72	72	72	72	288	72	72

† : All subjects in the Immediate treatment allocated 40 tasks to WP1 on this budget, versus 97.1% in Waiting Period.

*** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.10$. Coefficients are the marginal effects of each treatment on the probability a subject allocates all tasks to WP1. In columns (1)-(4) and (6)-(7), robust standard errors are reported in parentheses below each estimate. In column (5), standard errors clustered at the individual level are reported in parentheses below each estimate.

Table A.3: Effect of Treatment on Convex Task Allocations to Work Period 1, Lab & Field Studies

Interest rate:	50%	25%	12.5%	0%	All
	(1)	(2)	(3)	(4)	(5)
Waiting Period	4.936***	5.635***	4.796***	3.047*	4.604***
	(1.362)	(1.442)	(1.464)	(1.824)	(1.339)
Constant	31.257	30.171	30.171	28.114	29.929
	(1.124)	(1.100)	(1.084)	(1.217)	(1.014)
N	132	132	132	132	528

*** : $p < 0.01$, ** : $p < 0.05$, * : $p < 0.10$. All estimates from OLS models. In columns (1)-(4), bootstrapped standard errors from 1000 replications are reported in parentheses below each estimate, to adjust for non-normality of the error distribution. Output is reproducible with a seed of 1. In column (5), standard errors clustered at the individual level are reported in parentheses below each estimate.

Table A.4: Observable Balance across Treatments, DRC Study

Variable	Immediate	Waiting Period	Difference
Female	0.41	0.42	-0.01
Age	30.90	30.59	0.31
Secondary education or beyond	0.79	0.77	0.02
Has children	0.69	0.75	-0.05
Employed	0.44	0.39	0.06
Distance from city center (1-3 scale)	1.57	1.61	-0.04
Feels safe at home (1-4 scale)	2.34	2.53	-0.20*
Access to food (1-4 scale)	2.39	2.39	0.00
Access to clean water (1-4 scale)	2.40	2.29	0.11
Access to medical care (1-4 scale)	2.05	2.13	-0.08
Access to shelter (1-4 scale)	2.36	2.40	-0.04
Access to phone network (1-4 scale)	2.66	2.40	0.26*
Life got better last year (1-5 scale)	3.04	3.14	-0.10
Expects life better next yr. (1-5 scale)	3.72	3.73	-0.08
Not afraid to take risks (1-4 scale)	3.03	3.12	-0.09
Feels in control of life (1-4 scale)	2.32	2.23	0.08
Worries about future (1-4 scale)	2.74	2.88	-0.14
Plans for next week (1-4 scale)	3.10	3.13	-0.04
Trusts others (1-4 scale)	2.38	2.55	-0.17
Close to community (1-4 scale)	2.94	3.05	-0.11
Property damage due to conflict	0.46	0.50	-0.04
Direct exposure to violence during war	0.38	0.30	0.08

$^{*}: p < 0.10$.

Table A.5: Impact of Waiting Period on
Likelihood of Minimum-value Coupon Redemption, Full Results

Model:	OLS	Probit
	(1)	(2)
Waiting Period	-0.166***	-0.243***
	(0.048)	(0.082)
Food Access	-0.020	-0.020
	(0.044)	(0.045)
Food Access X Waiting Period	0.013	0.013
	(0.050)	(0.066)
Distance from Store	0.037	0.037
	(0.060)	(0.058)
Distance from Store X Waiting Period	-0.000	0.038
	(0.069)	(0.087)
Trust in Others	-0.003	-0.003
	(0.041)	(0.039)
Trust in Others X Waiting Period	0.014	0.022
	(0.048)	(0.066)
Risk Tolerance	-0.020	-0.020
	(0.044)	(0.042)
Risk Tolerance X Waiting Period	0.067	0.133*
	(0.050)	(0.077)
Constant	0.250	0.251
	(0.037)	(0.038)
N	258	252

$^{***}: p < 0.01$. Coefficients from Probit models are the marginal effects associated with switching from the Immediate to Waiting Period treatment. Robust standard errors are reported in parentheses below each estimate. Food access, trust in others and risk tolerance are all measured on 1-4 scales, and distance from the store is measured on a 1-3 scale. All control variables are de-meaned. We lose six observations with the addition of control variables due to incomplete survey responses.

A.4 Sample Experiment Instructions

Both the online and laboratory studies were run using the Qualtrics platform. All .qsf files are available in the AEA Data and Code Repository.

Sample Task

To continue, please complete the two example tasks below.

0	0	0	1	1	0	1	0	1	0	1	1	1	1	0
1	0	0	1	0	1	0	0	0	1	1	1	1	1	1
1	0	0	0	0	0	0	0	0	0	0	0	0	1	1
1	1	1	0	0	0	1	1	0	1	0	0	0	0	1
1	1	1	1	1	0	1	0	1	1	1	1	1	0	1
0	1	0	0	0	0	1	1	0	0	1	1	1	1	0
1	0	0	1	0	0	0	0	1	0	0	1	1	0	1
0	1	1	0	1	0	1	0	1	0	1	0	1	0	1
1	0	0	1	1	0	1	0	0	1	0	0	0	0	1
1	1	1	0	1	1	1	0	0	1	1	1	1	1	1

Example Table 1

How many zeros are in the table above?

0	0	1	0	1	1	0	1	1	1	0	0	1	1	0
0	1	0	0	0	0	1	1	1	0	0	0	0	0	0
1	1	0	0	1	1	0	1	1	0	0	1	1	0	0
0	0	0	1	0	1	1	1	1	0	0	0	0	1	1
1	0	1	1	0	0	1	0	1	1	0	1	0	1	1
0	1	1	1	1	0	1	0	0	0	1	1	0	0	0
0	0	0	1	1	0	0	1	1	1	1	0	1	0	0
0	0	0	0	1	0	0	0	1	0	0	1	0	0	1
0	1	1	0	1	1	1	0	1	1	1	1	1	0	0
1	1	0	1	1	1	0	0	0	0	1	0	1	0	0

Example Table 2

How many zeros are in the table above?

>>

Immediate Treatment

To finish the study and earn your payment, you will be given a choice about how many tasks to do and when to do them. The study is broken up into two work periods. Work Period 1 will begin immediately after you make a choice and will last for approximately 1 hour. Work Period 2 begins directly after that and also lasts for approximately 1 hour.

You will be given a choice of how many tasks to do in each work period.

If you choose not to work during a work period or if you finish your tasks early within a work period, you have free time for the rest of the work period. The program is timed in such a way that you cannot advance to the next work period until the full hour has elapsed. Once you finish the tasks you chose to complete in a work period, you can spend the rest of the time in the period however you want. For example, you can open another browser window and surf the internet, read a book, study, etc. Once the one hour ends, the next work period will begin. Here you will work on the number of tasks you chose to complete in that period.

Once Work Period 2 is over, you will have one additional hour of free time. After this, we will give you a brief survey to complete.

>>

Waiting Period Treatment

To finish the study and earn your payment, you will be given choices about how many tasks to do and when to do them. We will first describe the choices to you. You will then have an hour to think about the choices and are free to do whatever you want to pass the time at your lab station. The one restriction is that you cannot communicate with others.

At the end of the hour you will be asked to make choices of how many tasks to do in each of the two work periods. Work Period 1 will begin after you make a choice and will last for approximately 1 hour. Work Period 2 begins directly after that and also lasts for approximately 1 hour.

You will be given a choice of how many tasks to do in each work period.

For example, you may be faced with a choice between:

Option A) 10 tasks in Work Period 1 and 0 tasks in Work Period 2
Option B) 5 tasks in Work Period 1 and 6 tasks in Work Period 2
Option C) 0 tasks in Work Period 1 and 12 tasks in Work Period 2

If you choose not to work during a work period or if you finish your tasks early within a work period, you have free time for the rest of the work period. The program is timed in such a way that you cannot advance to the next work period until the full hour has elapsed. Once you finish the tasks you chose to complete in a work period, you can spend the rest of the time in the period however you want. For example, you can open another browser window and surf the internet, read a book, study, etc. Once the one hour ends, the next work period will begin. Here you will work on the number of tasks you chose to complete in that period.

Once Work Period 2 is over, you will have a brief survey to complete.

You have a variety of options for how many tasks you need to do in Work Period 1 (starting immediately after your choices) and Work Period 2 (starting in approximately 1 hour).

Each column in the table below represents one option. For example, Option A involves doing 0 tasks in Work Period 1 *and* 60 tasks in Work Period 2. Option B involves doing 4 tasks in Work Period 1 *and* 54 tasks in Work Period 2, etc.

PERIOD	**TASK OPTIONS**										
	A	B	C	D	E	F	G	H	I	J	K
WORK PERIOD 1 - IMMEDIATELY	0	4	8	12	16	20	24	28	32	36	40
WORK PERIOD 2 - IN 1 HOUR	60	54	48	42	36	30	24	18	12	6	0

Please examine the table above and choose your preferred option by putting the letter of the option into the box below.

My preferred option is: