Waiting to Choose*

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Abstract

We study the role of heuristic versus deliberative processing in intertemporal choice. Using studies in the Democratic Republic of Congo and an online labor market, we show that waiting periods –designed to prompt deliberation by temporally separating news about choice sets from choices themselves– can shift individuals away from decisions consistent with heuristic thinking and lead to substantially more patient choices. These findings are not captured by exponential or hyperbolic discounting. Additionally, waiting periods have larger effects on those shown to be particularly prone to myopia as a result of prior trauma, highlighting their potential as targeted interventions.

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1 Introduction

Distinguishing choices that stem from 'fast,' automatic versus 'slow,' deliberative processing has been a key component of the research program examining how and when decisions deviate from the normative benchmarks of neoclassical economic theory (Kahneman, 2011; Tversky and Kahneman, 1974). Automatic processing often relies on simple heuristics that reduce the complexity of a decision problem but may result in systematic behavioral biases which form the basis of behavioral economics research (Rabin, 1998). Choices stemming from deliberative processing are closer to the normative benchmark.[1] Myopic behavior in intertemporal choice has often been attributed to the use of heuristics (Read et al., 2013; Rubinstein, 2003), with heuristic models outperforming conventional theory in describing nearsighted decisions (Ericson et al., 2015), while more far-sighted choices have been ascribed to deliberative processing (Metcalfe and Mischel, 1999; Thaler and Shefrin, 1981).

Research in this domain has typically taken the use of heuristics to be a fixed property of individuals' preferences (e.g., Ericson et al., 2015). In contrast, this paper provides evidence that features of the choice environment can affect an individual's propensity to use heuristics versus more deliberative processing. Specifically, we show that waiting periods – which are designed to prompt deliberative processing by temporally separating information about a choice set and the choice itself – can shift individuals' preferences over intertemporal allocations and lead to substantially more patient choices.

Waiting periods are often imposed in practice when myopia and impulsivity are perceived as particularly harmful. Many U.S. states impose a waiting period of up to 14 days between the purchase and receipt of a gun.[2] Similar waiting periods are often imposed for those seeking to get married or divorced, and prescribed as effective strategies for avoiding myopic choices in negotiations (Brooks, 2015) and conflict resolution (Burgess, 2004). These policies are predicated on the idea that despite facing a similar choice set in the future, inserting a delay between the time when that set comes into focus and the ability to actually carry out the choice may be effective in

[1] Recent work in economics has used the distinction between heuristic thinking and deliberative processing to explain anomalies in the cross-section of stock returns (Barberis et al., 2013), excessive focus on the leftmost odometer digits in used-car sales (Lacetera et al., 2012), misperceptions of the US Federal Income Tax (Rees-Jones and Taubinsky, 2016), and the phenomenon of 'wiggling out' of altruistic choices in moral dilemmas (Kessler et al., 2015)

[2] Nine states and the District of Columbia have imposed waiting periods for the purchase of firearms ranging from 24 hours (Illinois) to 14 days (Hawaii).

prompting individuals to shift their mode of thinking and lead to a change in the final decision. Yet despite the frequent use of waiting periods in these settings, little research has been done to examine whether such prompts for deliberative processing actually affect intertemporal choice.

For example, 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 will decide what portion to save and what to spend. Now imagine that a firm offered to deliver the refund immediately: would the individual's decision to spend versus save change if the waiting period was eliminated?[3] The standard model of exponential discounting, which assumes stationarity, consistency and invariance in preferences (Halevy, 2015), predicts that, all things being equal, intertemporal choices such as the decision to spend now versus save for later should be the same in both cases. The behavioral model of quasi-hyperbolic discounting (Laibson, 1997; O'Donoghue and Rabin, 1999), which relaxes stationarity and consistency, makes a similar prediction.[4]

In this paper, we show that prompts for deliberative processing such as waiting periods lead to substantially more patient choices and rule out alternative explanations such as non-stationary time preferences and the ability to mentally commit to a plan. Our first study was run in the field and examined the intertemporal choices over consumption goods. We worked with a local grocery store in Bukavu, Democratic Republic of Congo (DRC). Upon arriving at the grocery store, customers were randomly assigned into one of two treatments. In both, individuals received a coupon that could be exchanged for one bag of flour (1kg) on a pre-specified redemption date. For every day the coupon was saved after the earliest redemption 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. Our main dependent outcome is when the coupon was redeemed: whether customers redeemed it earlier for a smaller reward or waited to redeem it for the later, larger reward. We find that individuals who had a waiting period redeemed their coupons later (relative to the earliest possible redemption

[3]This example is pertinent in light of the recent partnership between Walmart, a retail store, and H&R Block, a tax preparation service. Customers are able to file their taxes at Walmart, with H&R Block providing the refund immediately on a pre-paid debit card through a no cost Refund Anticipation Loan.

[4]This assumes an absence of a binding commitment device. Present bias predicts more spending relative to saving than exponential discounting, but this is irrelevant of the waiting period.

date), and consequently for more flour, than those who did not have a waiting period. Those in the latter group redeemed their coupon on the earliest possible redemption date 25% of the time while those who had a waiting period did so only 9% of the time – the 16 percentage point difference is both statistically and economically significant.

This study's setting also allows us to test whether those who, based on prior research, were more likely to exhibit a propensity for automatic processing and use of heuristics were differentially affected by the treatment. Exposure to violence has been shown to be associated with lower impulse control (Osofsky, 1995), and greater myopia (Voors et al., 2012) and propensity to exhibit behavioral biases such as the "certainty effect" (Callen et al., 2014). As part of the study, we elicited participants' prior exposure to violence using methods commonly used in the literature (Voors et al., 2012) and find that, in line with prior work, individuals who were exposed to violence were substantially more likely to redeem the coupon on the earliest possible date relative to those who were not exposed. However, this difference only exists when customers had the opportunity to redeem the coupon on the day of the receipt. There was no difference between the groups when both experienced a waiting period.

Our second study sought to replicate the initial findings in a more controlled setting and rule out alternative explanations. Individuals made allocation decisions between labor and leisure across hour-long work periods. We used an online labor market to recruit a population that was experienced in making intertemporal labor-leisure decisions in a similar context. Each participant allocated real-effort tasks between two work periods, where any time not spent on the effort task could be used to engage in a leisure activity of her choosing (e.g. watch streaming movies, read a book, etc.). Delaying tasks to a later work period resulted in a greater total task requirement, such that allocating tasks to the earliest available period maximized total leisure time. In one treatment, participants were given the information about their choice and had the opportunity to make their choice directly after; in another treatment, they were given the 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 was constrained in the time allowed to make a decision, but the latter group was given a waiting period before the choice could be made while the former was not. We find that those who had a waiting period made substantially more patient choices than those who did not, allocating significantly more effort tasks to the earlier work period and enjoying more leisure time as a result. To rule out the possibility that this effect was driven by differences in the timing of work periods (e.g. one group getting used to waiting), we ran a control treatment where the first hour did not involve work, as in the waiting period treatment, but participants were given information about the allocation decision after this interval rather than before. They then had the opportunity to make a choice. If our results were driven by timing of the work periods, then participants in this control treatment would have made similar allocation decisions as those in the waiting period treatment; in contrast, if waiting periods prompt deliberative processing as we hypothesize, results in the control treatment should resemble decisions in the treatment without a waiting period. We find evidence for the latter.

We argue that waiting periods promote a shift from heuristic to deliberative processing, which results in more patient choices. However, an alternative explanation for our results could be that individuals have non-stationary time preferences (e.g. present bias) and can mentally commit to a plan when given time. To differentiate between these two explanations, we ran a treatment where 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 can be explained by non-stationary preferences, then the allocation decision over two future work periods should should be similar to the choices of those who actually experienced a waiting period beforehand. However, this group still put off significantly more effort tasks to the later work period relative to those who had a waiting period before the allocation decision. We structurally estimate a model that allows us to make direct comparisons between the relative effects of violations of stationarity on patience, which are commonly studied in behavioral economics, versus violations of time invariance that stem from individuals making a different allocation after a waiting period despite facing the same choice set.[5] The results suggest that in this setting waiting periods have a larger effect on patience than the ability to plan and commit for the future.

Our findings are complementary to recent work by Gabaix and Laibson (2016) who present a formal model of how deliberation can affect intertemporal choice. In their framework, a decision-maker receives noisy signals about future utility and acts as if she will not learn new information

[5] Halevy (2015) presents the three properties of time preferences – stationarity, time consistency, and invariance – and describes how violations can be identified.

later on. The noise results in imperfect simulations of the future, which leads to impatience and myopia. Deliberation causes the decision-maker to extract further signals about future utility, either through memory recall or other forms of introspection, which generates a better simulation and potentially less myopic decisions. Their model suggests that since in every period the decision-maker acts as if no new information will be learned in future periods, introducing mandatory prompts for deliberation such as a waiting period should lead to less myopic decisions than when no such prompts are provided. This prediction is consistent with our results.

The rest of the paper is organized as follows. Section 2 presents the consumption goods experiment in the DRC and demonstrates the effects of waiting periods on intertemporal decisions in a field setting. Section 3 outlines the effort allocation experiment and the results. Section 4 discusses the findings and concludes.

2 Waiting Periods for Consumption Goods

2.1 Design and Implementation

Our study was conducted at a small grocery store in a residential area in Bukavu, a city on the Eastern border of the Democratic Republic Congo (DRC). A total of 258 customers participated in the study, where each made a decision of when to redeem a coupon for a set amount of flour. 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. As a result, differences in the transaction costs of redeeming a coupon across days of the study were likely small. 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.

Upon arriving at the store and agreeing to participate, all customers completed a detailed survey on their demographics and prior exposure to violence. 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 participant chose which was more convenient for them. On average the survey took 30 minutes to complete.[6]

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.[7] 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.

In this design participants faced a series of binary choices which terminated upon redemption of the coupon. For example, when a participant received a coupon in the Immediate treatment, she could choose to redeem the coupon that day for one bag of flour or to wait. If she chose to redeem, there would be no more choices. If she chose to wait, the next day the participant would face the choice of redeeming the coupon for two bags of flour or to wait. Conditional on waiting until the fifth day, the participant would choose between five bags of flour or nothing (since this was the end of the redemption schedule).[8] In the Waiting Period treatment, the participant received a coupon along with the information on the redemption schedule but did not face a choice until the following day. The next day, she could decide whether to redeem the coupon for one bag of flour or to wait, and so on, until five days later. Given the intertemporal tradeoff involved in each choice, the value of the coupon at the time of redemption serves as our primary measure of patience.

We use the questionnaire 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 1). Importantly, neither measures of trust of others, stated preference for risk, nor exposure to violence of any type were correlated with treatment assignment.

Additionally, our setting allowed us to examine whether waiting periods had a differential

[6]Full questionnaire available upon request.
[7]Each coupon had an ID matching it with a questionnaire, a date of issue and a code signifying the treatment.
[8]Due to the material incentives and participants' daily visits to the store, only one person did not redeem their coupon by the last possible day (this individual was in the Waiting Period treatment).

Table 1: Observable Balance across Treatments

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 people (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	0.38	0.30	0.08

$^{*}: p < 0.10$.

effect on the intertemporal choices of those exposed to violence. This population is of particular interest for the study of waiting periods based on prior work showing that exposure to violence decreases capacity for emotional regulation (Osofsky, 1995; Houltberg et al., 2012), a key factor in individuals' propensity for automatic versus deliberative processing (Loewenstein, 2000; Evans et al., 2005). Those exposed to violence have been shown to make more myopic choices (Voors et al., 2012) and be more prone to decision biases such as the "certainty effect" (Callen et al., 2014). This suggests that the introduction of waiting periods – designed to prompt deliberative processing – may have a particularly large effect on the intertemporal choices of those who had previously been exposed to violence.[9]

The grocery store in our study was located near an active combat zone and our population comprised of people that differed in their exposure to violence.[10] We measure exposure to violence at the individual level using survey methods commonly used in the literature (Voors et al., 2012) to classify prior exposure to violence. In our sample, 34% identified as being directly exposed to violence while 66% were either indirectly exposed to violence (e.g. members of family injured) or not exposed at all.

Because the conflict in the DRC, and especially the Great Lakes region of the eastern DRC, has been going on for many years, there is a span of time periods during which our participants could have experienced the reported exposure. This uncertainty casts doubt on whether, for those exposed to violence in the more distant past, the exposure is the proximate cause of any heterogeneous treatment effect we observe in the Immediate treatment. Preferences may change not because of the exposure itself but because of other long-term effects. For this reason, we use variables from our survey to control for other factors that are correlated with violence. Because some of these controls are endogenous, they will not produce an unbiased estimate of the effect of exposure to violence. Instead, they serve as a robustness check for prior exposure to violence as a significant factor in explaining behavioral variation. Direct exposure is correlated with other, less direct types of exposure to violence, such as damage, destruction and confiscation of one's home. We use property damage as a variable to (at least partially) control for loss of wealth as a channel for the relationships we observe.

While we cannot completely rule out alternative channels besides exposure to violence, per se, we note that any heterogenous treatment effects on those exposed to violence are identified from the exogenous randomization into the Immediate and Waiting Period treatments.

[9] It should be noted that we are not the first to study the effects of prior trauma such as exposure to violence and natural disaster on preferences. See Voors et al. (2012), Bchir and Willinger (2013), Callen (2015) and Lien et al. (2015) on time preference and Eckel et al. (2009), Callen et al. (2014) and Hanaoka et al. (2015) on risk preference.

[10] For more than 20 years, the DRC has been facing an ongoing, complex and multifactor militarized conflict. By 2008, the first and second Congo wars and their aftermaths had killed 5.4 million people mostly in the East Congo (Coghlan et al., 2007) and random violence was widespread (Elbert et al., 2013). Despite the UN efforts, including the Goma peace agreements of 2008 and 2009, fighting among various armed groups continues to the present (AI, 2004, 2008a,b, 2012; MSF, 2013). According to the reports from local and international NGOs and the US State Department (Mahecic, 2012; MSF, 2005; USDOS, 2014), the violence perpetrated by armed groups in the region was largely indiscriminate.

2.2 Results

We break our results into two subsections. First, we present reduced-form results that characterize the data and effects of the experimental manipulations. Second, we structurally estimate discounting parameters of a model that allows for violations of time invariance to contribute to the growing literature estimating the magnitude of deviation from standard models of time preference and to cast the effects of treatment status in an interpretable and externally relevant metric.

2.2.1 Reduced-Form Estimates

Figure 1, Panel A shows the fraction of individuals who chose to redeem the coupons on the earliest available date (for one bag of flour) by treatment.[11] The introduction of a waiting period has a substantial effect on 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 ($p = 0.001$).

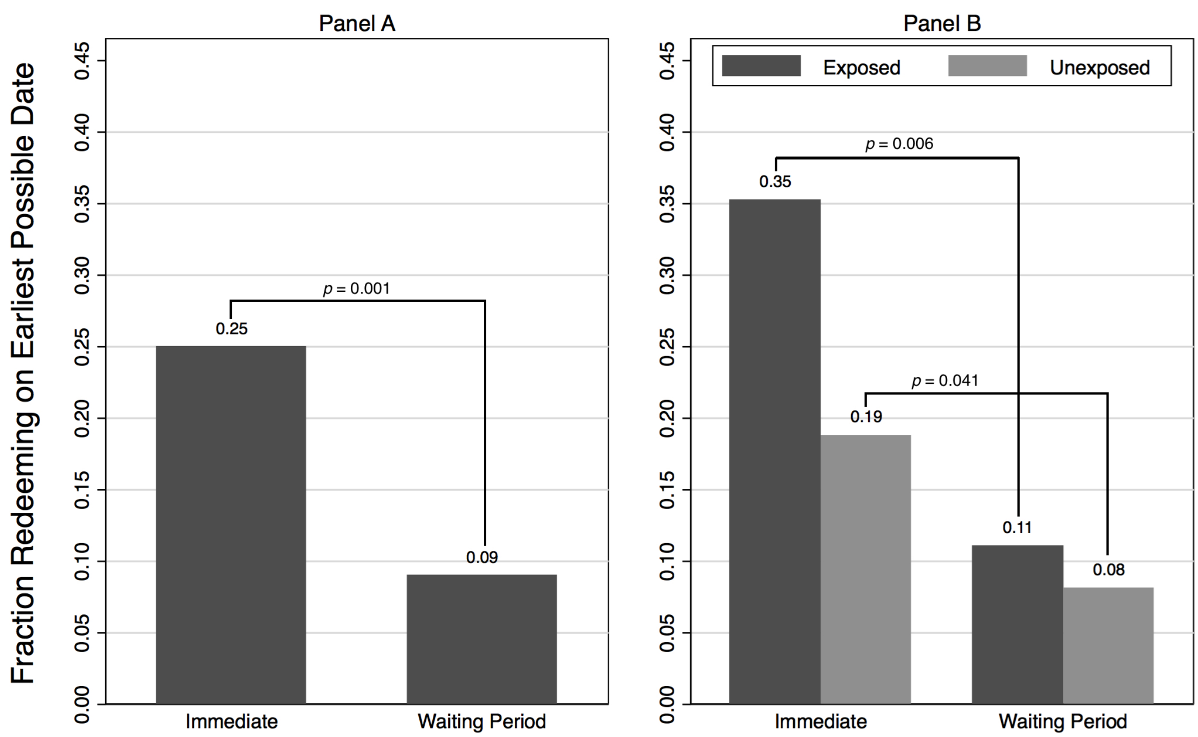

Figure 1: Fraction Redeeming Coupon on Earliest Possible Date by Treatment

[11] All p-values in Figure 1 are from OLS linear probability models.

We next examine whether the introduction of a waiting period had a differential effect on those who had been previously exposed to violence. Figure 1, Panel B shows the frequency of early redemption by treatment and exposure to violence. There is a differential effect of treatment across the two exposure groups. For participants who had been previously exposed to violence, the waiting period had a very large effect: 35% redeemed the coupon on the earliest available date in the Immediate treatment versus 11% in the Waiting Period treatment ($p = 0.006$). The treatment effect is smaller but significant for the unexposed group: the waiting period reduces earliest-possible redemption from 19% to 8% ($p = 0.041$). The differential effect is strong enough such that in the Immediate treatment, early redemption differs significantly by exposure (35% versus 19%, $p = 0.032$); in contrast, this difference in early redemption nearly disappears in the Waiting Period treatment (11% versus 8% unexposed, $p = 0.605$). The difference-in-difference of 13% is large and marginally significant with study-day fixed effects ($p = 0.080$). Adding non-endogenous control variables of age and gender increases the difference to 18% ($p = 0.067$); adding a set of endogenous controls further increases it to 19% ($p = 0.061$).[12] These results suggest that waiting periods have a differentially pronounced effect for those who have been shown to be more prone to automatic processing and myopic behavior, even when conditioning on some alternative pathways through which this effect may operate.

Note that the observed variation in behavior cannot be explained by differences in beliefs about future uncertainty (e.g. future survival odds). Any difference in beliefs on the earliest redemption date in the Immediate treatment will also be present on the earliest redemption date in the Waiting Period treatment. Other than being preceded by a waiting period in the latter, the choice between redeeming the coupon for 1 bag of flour or not is the same in both treatments, and hence should be affected by beliefs in the same way.

Moving beyond the binary measure, we estimate the relationship between exposure to violence and redemption amount separately for each treatment and then test for a difference between the coefficients. Results are presented in Table 2, with study day fixed effects included in all specifications. We find that exposure to violence in the Immediate treatment corresponds to redeeming the coupons 0.718 days sooner, and therefore for 0.718 fewer kg of flour ($p = 0.014$), relative to those

[12]Controls include: an indicator for whether the participant has children, whether they are employed, a measure of risk preference, a measure of their perceived control over their life, whether they experienced property damage during the war and indicators for level of educational attainment.

not directly exposed to violence. There is no such link in the Waiting Period treatment: exposure to violence prompts coupon use 0.020 days sooner ($p = 0.925$). The difference-in-difference of 0.699 days (or kg) is significant ($p = 0.052$). The difference-in-difference is robust to including the same set control variables as with the binary estimated (see Footnote 12) – the estimate remains unchanged and marginally significant ($p = 0.055$). Augmenting the regressions with an even more extensive list of controls, which includes variables such as access to food, clean water, shelter, medical supplies, phone networks and employment status, has no effect on the coefficient in the Immediate treatment and leads to a small increase in absolute value of the coefficient in the Waiting Period treatment.[13]

Despite attempts to minimize potential confounds such as differential transaction costs, the difficulties endemic to running the study in the field prevent us from completely ruling them out. In this sense, the findings from the second study are complementary; observing analogous effects of waiting periods on intertemporal decisions in a setting where transaction costs and other confounds are minimized suggests that such confounds are unlikely to be the sole drivers of variation between the Immediate and Waiting Period treatments in the field study.

2.2.2 Structural Estimates

We estimate a discounting model that utilizes the treatment variation in whether subjects experienced a waiting period prior to their first choice to identify B, which we call the deliberation parameter. The parameter B captures the difference between allocation decisions where the choice is made directly after learning the choice set compared to, all else equal, decisions where information about the choice set and the choice itself are separated by a waiting period.

On the first day of the Immediate treatment, when an individual receives a coupon that is immediately redeemable, they make a choice without a prompt encouraging deliberative processing. On every other day of the study – including the first day of the Waiting Period treatment – choices are made after a waiting period. We define N as an indicator variable for whether information and choice coincide, and it equals one only on the first day of the Immediate treatment. Because all choices made in this study are of the form, "do I use the coupon today or wait?" we do not

[13] Similarly, the qualitative results from the binary-outcome regressions are unaffected by the inclusion of these controls.

Table 2: Redemption Amount by Treatment

Treatment:	Immediate	Waiting Period	Immediate	Waiting Period
	(1)	(2)	(3)	(4)
Exposure to Violence	-0.718**	-0.020	-0.761***	-0.075
	(0.291)	(0.210)	(0.287)	(0.215)
H_0: Immediate = Waiting Period	$\chi^2(1) = 3.790^*$		$\chi^2(1) = 3.670^*$	
	$p = 0.052$		$p = 0.055$	
Male			-0.974***	-0.079
			(0.312)	(0.215)
Age			0.023	-0.009
			(0.014)	(0.012)
Has children			-0.243	0.353
			(0.389)	(0.245)
Employed			0.173	-0.095
			(0.295)	(0.203)
Risk-loving (0-3 scale)			0.085	-0.108
			(0.149)	(0.100)
Life control (0-3 scale)			0.360***	-0.074
			(0.119)	(0.084)
Property damage			0.697**	0.484**
			(0.305)	(0.212)
Constant	3.127	3.208	2.918	2.946
	(0.445)	(0.258)	(0.804)	(0.547)
Education FE	N	N	Y	Y
Day FE	Y	Y	Y	Y
Observations	136	122	127	117

*** : $p < 0.01$, ** : $p < 0.05$. Robust standard errors are reported in parentheses below each estimate.

separately estimate exponential and quasi-hyperbolic discount factors (the design of our second study will allow us to do this). We model subjects' utility from the flour as

$$V(c) = (B^N \delta)^k v(c) \quad , \tag{1}$$

where c is the bags of flour, k is the delay until flour receipt, δ is the daily discount factor and $v(\cdot)$ is the instantaneous flour utility function, specified parametrically as c^α. The effects of the waiting period are captured through the deliberation parameter B, which is modeled is a shock to the exponential discount factor that applies when $N = 1$. In the taxonomy of Halevy (2015), a $B < 1$ implies a violation of the time invariance property: the individual makes a more patient allocation decision after a waiting period despite facing the same choice set.

Participants faced a series of binary choices. On the first day of coupon eligibility, they could redeem the coupon for one bag of flour or wait. Waiting is valued according to the maximum of all the values the coupon can obtain in the future (what they would plan to obtain in the future). Each time an individual arrived at a decision, they chose between the current value of the coupon or the maximum of the remaining values. We use this approach to construct maximum likelihood estimates of δ, B and α. Individuals who redeemed the coupon right away will thus contribute one event to the likelihood function, while individuals who redeemed the coupon for its maximum value will contribute four.

Because the first day of the experiment coincided with the earliest redemption date in the Immediate treatment, participants are subject to B; they discount their options on the first day at unity and future options on days 2-5 using discount factors $B\delta, (B\delta)^2, ...(B\delta)^4$. If a participant endogenously chooses to wait, on the second day of the experiment she discounts future options using $\delta, \delta^2, \delta^3$. In contrast, while participants in the Waiting Period treatment discount the future at the same rate as those in the Immediate treatment on the first day of the experiment, they cannot yet redeem their coupon. After waiting for a day, participants discount their available option at unity and future redemption options on days 2-5 using discount factors $\delta, \delta^2, ...\delta^4$. Because of the waiting period, the B parameter does not affect their available choices of redeeming the coupon or waiting. We estimate B for the full sample as 0.608 (S.E. = 0.102), which is significantly different than one ($p < 0.001$).[14]

3 Waiting Periods and Effort Allocation

3.1 Design and Implementation

Participants (N=122) were recruited from an online labor market (Amazon Mechanical Turk) to complete a series of real-effort tasks over a span of approximately three hours. Each was informed

[14]Appendix A.1 outlines the estimation strategy in further detail and presents results separately by exposure to violence.

that in order to complete the study and earn a $20 payment they must finish a number of tasks over the course of two one-hour work periods – WP1 and WP2, respectively. We adopt the approach of Augenblick et al. (2015) in allowing participants to allocate the effort tasks intertemporally between the two work periods using a series of Convex Time Budgets (Andreoni and Sprenger, 2012). A participant could not advance until the work period had ended even if all allocated effort tasks were completed – she would have to remain at or near the computer for the time left in the period to run out. However, the span between finishing the effort tasks and the start of the next work period was explicitly labeled as free time, during which the participant could engage in a leisure activity of her choice.[15] Hence the decision to allocate tasks between work periods involved an intertemporal tradeoff between work and leisure.

The task was designed to be onerous and effortful, consisting of counting the numbers of zeros in a 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. Importantly, before they were presented with the convex time budgets and given information about the effort allocation decision, participants had to successfully complete two sample tasks in order to understand and become familiar with the nature of the work.

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 which 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 3 presents the convex budgets.

After completing the convex budgets, participants were also given two binary choices that served as consistency checks for positive time discounting. The first offered a choice between 40

[15]We did not want to restrict participants' ability to choose their preferred leisure activity, and recommended that they could spend this time watching streaming movies, reading a book, etc. End-line surveys suggest participants indeed spent the time on leisure activites (responses available upon request).

Table 3: Choices 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 refers 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.

tasks in WP1 (and zero tasks in WP2) 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. Positive time discounting predicts that participants should prefer to allocate effort tasks to WP2 in the first choice and WP1 in the second. Moving the rate from one side of zero to the other should produce a large shift in choices.

To ensure that completing tasks sooner did not result in an earlier end to the experiment, work periods were constrained to last approximately one hour. 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.

We implemented four different treatments. All treatments presented participants with the same budgets as described above and were divided into three one-hour periods. The main difference between treatments was whether WP1 and WP2 were the first two periods or the last two periods. Figure 2 outlines all four treatments.

In the Immediate treatment, participants were presented with 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

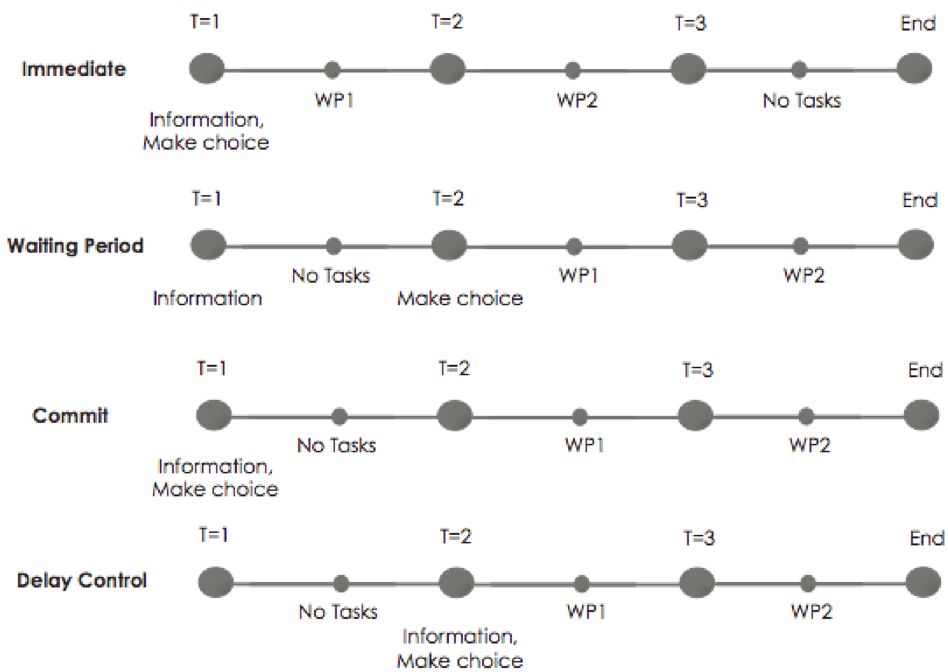

Figure 2: Outline of Experimental Conditions

payment. In the Waiting Period treatment, participants were presented with 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 their decision.

Comparing allocation decisions between the Immediate and Waiting Period treatment allows us to test the effect of waiting periods on intertemporal choice. If, as predicted, waiting periods increase patience, then participants in the Waiting Period treatment should allocate a greater amount of tasks to the earlier work period (WP1) for budgets with a positive interest rate, thereby maximizing their leisure time.

The Commit treatment was designed to examine whether a model that allowed for non-stationary time preferences (e.g. present bias through quasi-hyperbolic discounting) and the ability to follow through on an ex-ante plan could explain the effect of waiting periods on patience. The timing of the work periods was the same as in the Waiting Period treatment, with the only difference that participants made allocation decisions before the waiting period versus after. In other words, they made pre-committed choices for future behavior. The comparison between the Commit and Immediate treatments resembles a typical study attempting to identify violations of stationarity, with a one-hour period serving to separate the 'present' from the 'future' (Halevy, 2015).[16] If choices in the Commit treatment look similar to those in the Waiting Period treatment, then the effects of the waiting period on patience would be consistent with a model that allows for non-stationary time preferences such as quasi-hyperbolic discounting and the ability to follow through on an ex-ante plan. If choices between the two treatments differ, then this would provide evidence for the role of heuristic versus deliberative processing in intertemporal decision-making.

Lastly, we designed the Delay Control treatment as a robustness check to ensure that any variation in behavior between the Immediate and Waiting Period treatments was due to the waiting period rather than differences in the timing of work periods. In the Delay Control treatment, participants learned that they would make a decision regarding the distribution of effort tasks but were not presented with the budgets until after a one hour period with no work. After being presented with the budgets, participants make an allocation decision between WP1 and WP2. The Delay Control treatment has the same timing of WPs as the Waiting Period treatment, but participants did not have a waiting period between being presented with the information and the ability to make a choice. This treatment allows us to rule out alternate explanations such as the delay getting participants used to waiting in general, as well as basic differences in preferences over the timing of outcomes.

It should be stressed that in no treatment were participants time constrained when making their allocation decisions. Participants could take as long as they want when deciding how to allocate effort between WPs in each of the four treatments; the main difference was whether or not 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.

3.2 Results

As for the first study, we divide our results into two subsections. We first present the reduced-form results followed by the structural estimation.

[16] Identification of the present-bias parameter models of quasi hyperbolic discounting depend critically on how the 'present' is defined. McClure et al. (2007) demonstrate that delaying the earliest reward by a 10 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 10 minutes (with the earliest reward available 10 minutes later). Based on this evidence, we believe the one hour period is sufficient to identify present bias; parameter estimates presented in the next section support this assumption.

3.2.1 Reduced-Form Estimates

Participants' allocation decisions were consistent with positive discounting. Examining decisions on the convex budgets, participants allocated significantly more tasks to WP1 as the interest rate increased in all four treatments (all p-values ≤ 0.016).[17] Looking at the binary choices, the majority of subjects allocated all tasks to Work Period 2 when the interest rate was negative and the majority allocated all tasks to Work Period 1 when the interest rate was barely positive (difference $= 46\%, p < 0.001$). This is consistent with positive discounting; only one of 122 subjects switched in the opposite direction.

Turning to treatment differences, we examine allocation decisions on the convex budgets. Comparing the Immediate and Waiting Period treatments, participants allocated significantly more tasks to WP1 in the latter than the former ($p = 0.023$). The magnitude of this effect is large: about 17% of the Immediate mean, or half a standard deviation. Figure 3 illustrates these findings.[18]

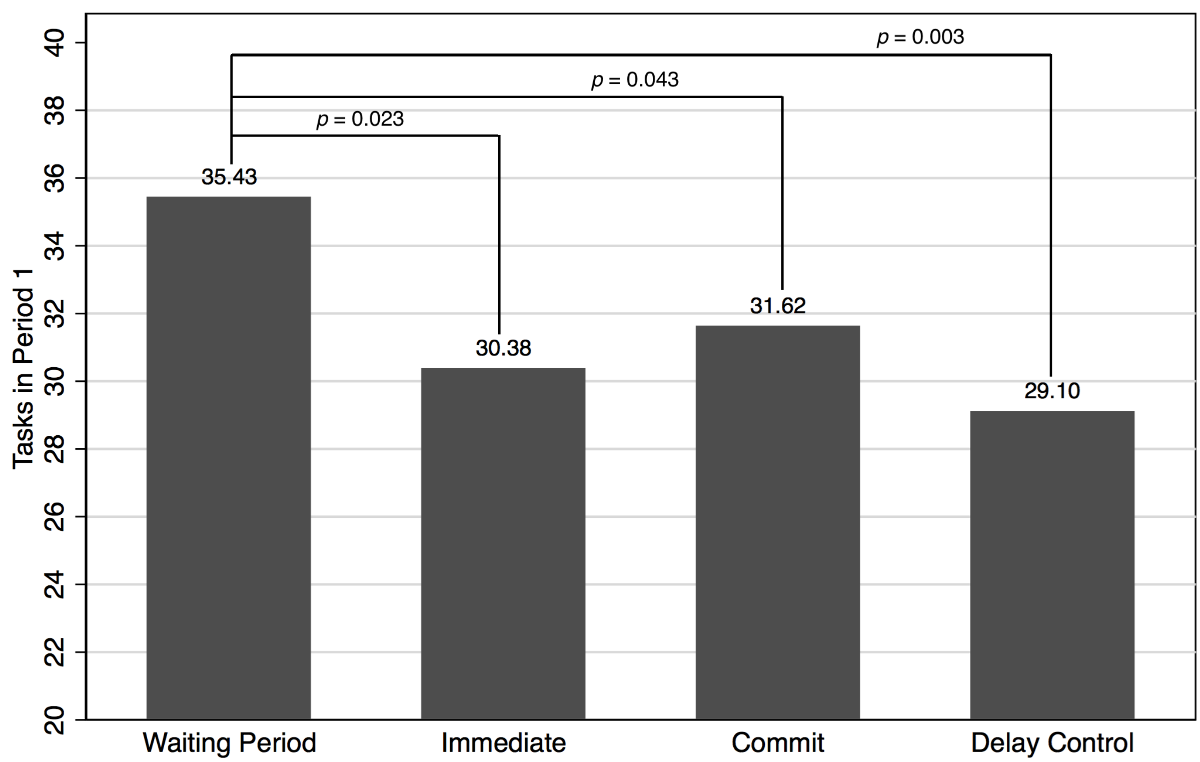

Figure 3: Task Allocations to Work Period 1 on Convex Choices, by Treatment

[17]Standard errors clustered at the individual level for all analyses that combine data from multiple budgets.
[18]The p-values are from OLS regressions with standard errors clustered at the individual level.

Figure 4 focuses in on our main difference of interest – comparing the choices of participants in the Waiting Period treatment to those in the Immediate treatment – to show the treatment effect 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. Participants in the Waiting Period treatment allocated significantly more tasks to WP1 than those in the Immediate treatment across all three budgets (all p-values ≤ 0.039). Decisions on Budget 4, which had a 0% implied interest rate, did not involve a tradeoff between fewer tasks now versus more tasks later. We can use choices on this budget to test whether the Waiting Period treatment led participants to allocate more effort tasks to WP1 in general, or only when this would maximize leisure over effort time. The treatment effect shrinks by 28% on this budget and is not statistically significant. Together, these results suggest that introducing a waiting period between information about a choice and the choice itself led to more patient decisions. Regression estimates of this difference are presented in Table 4, with both OLS and two-limit Tobit estimates that adjust for the censoring at the budget endpoints.

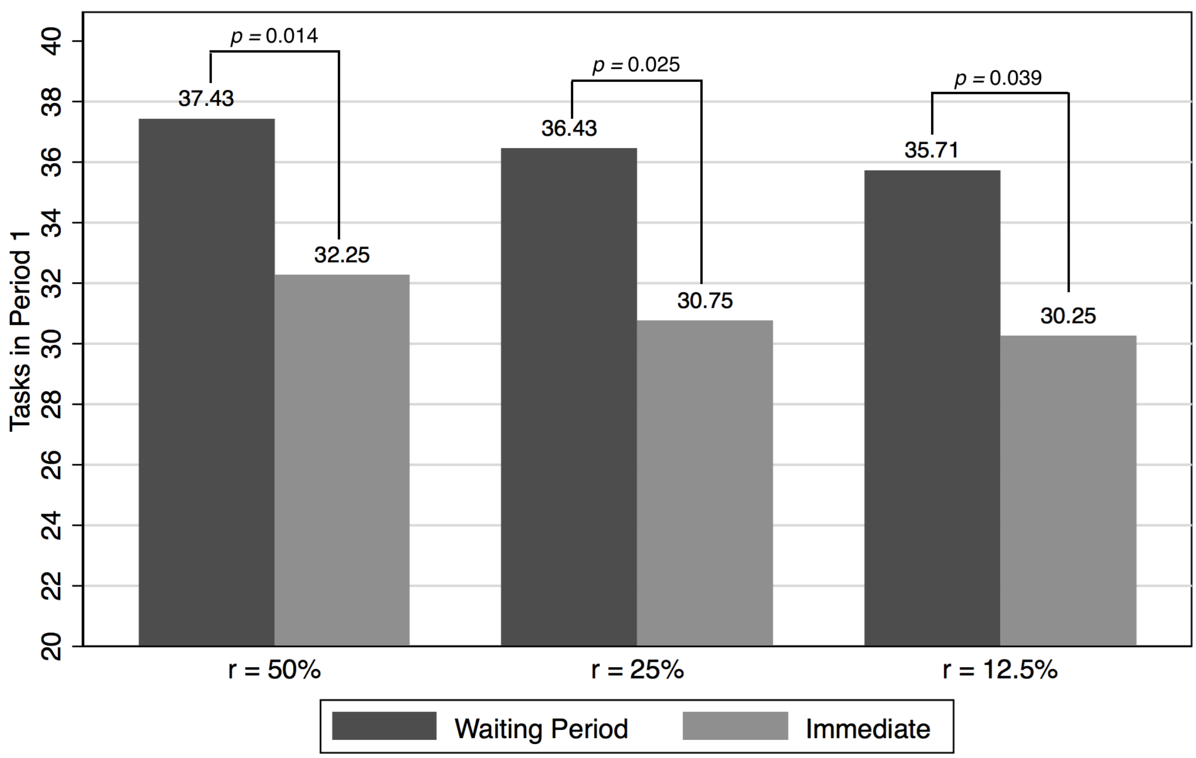

Figure 4: Task Allocations to Work Period 1 on Positive-interest Convex Choices, by Rate

Table 4: Effect of Treatment on Task Allocation to Work Period 1

Interest rate:	50%	25%	12.5%	0%
	(1)	(2)	(3)	(4)
	Panel A: OLS			
Waiting Period	5.179**	5.679**	5.464**	3.893
	(2.072)	(2.498)	(2.622)	(2.798)
Commit	0.750	2.000	2.500	-0.250
	(2.329)	(2.391)	(2.387)	(2.784)
Delay Control	0.417	0.183	0.417	-6.117*
	(2.514)	(2.805)	(2.804)	(3.233)
Constant (Immediate)	32.250	30.750	30.250	28.250
	(1.797)	(1.889)	(1.893)	(1.960)
	Panel A: 2-Limit Tobit			
Waiting Period	14.049**	15.914**	15.814**	7.351
	(6.085)	(6.655)	(6.995)	(5.496)
Commit	1.180	3.308	4.652	-0.282
	(5.044)	(5.161)	(5.440)	(4.913)
Delay Control	0.540	0.465	0.750	-9.936*
	(5.321)	(5.714)	(5.893)	(5.552)
Constant (Immediate)	39.686	37.637	37.450	33.046
	(3.918)	(4.083)	(4.267)	(3.648)
N	122	122	122	122

** : $p < 0.05$, * : $p < 0.10$. Standard errors clustered on individual level are reported in parentheses below each estimate.

Comparing decisions in the Commit treatment to those in the Waiting Period treatment allows us to test whether the effects of the latter are consistent with non-stationary time preferences and the ability to follow through on ex-ante plans. Those in the Waiting Period treatment allocate significantly more tasks to WP1 than those in the Commit treatment ($p = 0.043$). This suggests that the effect of the waiting period cannot solely be explained by violations of stationarity (e.g. present bias).

Table 4 also shows the effects of the Commit and Waiting Period treatment relative to the Immediate treatment by budget. Comparing the Waiting Period to the Commit coefficients, the Waiting Period coefficient is at least twice as large on all positive interest rate budgets. OLS and

Tobit models reject equality on Budget 1 ($p = 0.016$ and $p = 0.026$, respectively) and Budget 2 ($p = 0.097$ and $p = 0.050$, respectively). We fail to reject equality at conventional levels on Budget 3 ($p = 0.205$ and $p = 0.105$, OLS and Tobit respectively) though the directional difference remains sizable. This pattern is consistent with waiting periods having a larger effect as the payoff for patience, i.e. the interest rate, increases. The two treatments did not differ for Budget 4 which had an interest rate of 0%.

For Budgets 1-3, the coefficients of the Commit treatment are all positive though not significant, providing suggestive evidence for violations of stationarity. However, the reduced form analysis does not capture the full range of variation in the data when identifying present bias as a violation of stationarity. In the next section we estimate structural parameters of a model that allows for violations of both stationarity and time invariance, and show that while we do identify significant violations of stationarity, corresponding to a present bias parameter $\beta < 1$ in a model of quasi-hyperbolic discounting, it does not explain the full impact of waiting periods on patience.

Lastly, as a robustness check, we compare the Waiting Period treatment to Delay Control. Participants in the former treatment allocated significantly more tasks to WP1 than the latter ($p = 0.003$). Table 4 also presents results from OLS and Tobit regressions which confirm this finding, budget-by-budget. Looking at the budgets with a positive interest rate, the coefficient on the Waiting Period treatment is significantly different from the Delay Control treatment across the whole set, for both OLS and Tobit specifications. For the same budgets, allocation decisions in the Delay Control treatment were not significantly different from those in the Immediate treatment. Participants in the Delay Control treatment did seem to allocate *fewer* effort tasks to WP1 on Budget 4 (0% interest rate) compared to those in the Immediate treatment, but this difference was only marginally significant.

Together, these results suggest that waiting periods had a positive effect on patience in line with a framework that differentiates heuristic and deliberative processing in intertemporal choice. Next, we examine the data using structural estimation.

3.2.2 Structural Estimates

Let x_t be the tasks allocated to be done in the respective period, $t \in \{1, 2, 3\}$. δ is the one period discount factor and β is analogous to the present-bias parameter typically used in models of quasi-hyperbolic discounting (Laibson, 1997; O'Donoghue and Rabin, 1999). Parameters B and N are defined in the same way as in Section 2.2.2: B is the deliberation parameter and N is an indicator variable for whether an individual makes her choice with or without a waiting period. An individual's disutility from effort in this framework is

$$U(x_1, x_2, x_3) = u(x_1) + \beta\left[(B^N\delta)u(x_2) + (B^N\delta)^2 u(x_3)\right] \quad , \tag{2}$$

where $u(\cdot)$ is the instantaneous disutility of effort function, which we parametrically specify in Appendix A.2.

Downward deviations from one in both β and B imply greater impatience. The parameter β captures the difference between allocation decisions where one time period lies in the present and the others in the future, and decisions between periods that all lie in future. In the taxonomy of Halevy (2015), a $\beta < 1$ would imply a violation of the stationarity property of time preferences in the direction of present bias: in addition to standard exponential discounting, the individual down-weights all future periods by a common factor. As stated earlier, a $B < 1$ implies a violation of the time invariance property: the individual makes a more patient allocation decision after a waiting period despite facing the same choice set.

All intertemporal tradeoffs made in our study feature a one-hour delay. We estimate the one-hour discount factors separately for each treatment, where individuals minimize their effort disutility subject to the work requirement. The estimation of treatment-specific discount factors does not rely on particular assumptions of functional form because we only examine allocations over one time delay. The parameter β is identified as the ratio of the observed discount factor in the Immediate treatment, $B\beta\delta$ to the observed discount factor in Commit, $B\delta$. The parameter B is identified as the ratio of the observed discount factor in Immediate $B\beta\delta$ to the observed discount factor in Waiting Period $\beta\delta$. Appendix A.2 discusses our maximum likelihood approach to this estimation, using a modified Stone-Geary disutility function to flexibly model effort on our tasks.

For allocation decisions over effort and leisure, we find $\beta < 1$ and of similar magnitude to estimates in prior work (Augenblick et al., 2015): $\beta = 0.912$ (S.E. = 0.042, $p = 0.037$, tested against $\beta = 1$).[19] The estimate of the deliberation parameter B is also significantly less than one:

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

$B = 0.841$ (S.E. = 0.046, $p = 0.001$, tested against $B = 1$). Notably, we find that B is significantly different from β ($p = 0.045$). In sum, while our design captures significant present-bias measured as a violation of stationarity, the independent effect of waiting periods on intertemporal decisions is statistically greater.

4 Conclusion

Across two studies, we demonstrate the significant effect of prompting deliberative processing through waiting periods on intertemporal choices. The studies are complementary: the first study introduces waiting periods in a field setting where grocery store customers in the DRC decide when to redeem a food coupon that accrues value the longer it remains unredeemed, while the second study examines the effects of waiting periods on effort allocation decisions in a more controlled environment. We find that waiting periods lead to more patient decisions across both settings. Customers wait longer to redeem their coupon, and obtain more flour as a result, when the initial choice to redeem or not follows a waiting period. Similarly, participants choose to complete more effort tasks earlier, thereby maximizing their leisure time, when the allocation decision is preceded by a waiting period than when it is not. Rather than being driven by non-stationary time preferences, such as in models of quasi-hyperbolic discounting, the effect of waiting periods on intertemporal choices appears to require a temporal separation between information about a choice and the ability to make it. Following the taxonomy of Halevy (2015), this suggests a violation of the invariance property of time preferences, an aspect of dynamic consistency that has rarely been studied in the past. These findings are consistent with models that explicitly consider the role of deliberation in intertemporal choice (Kahneman and Frederick, 2002; Gabaix and Laibson, 2016).

Our results have implications for policy aimed at intertemporal decision-making. Economists have noted the lack of demand for commitment devices despite the prediction for such demand from behavioral models of discounting designed to capture myopic behavior (Laibson, 2015).[20] Our findings suggest that the difference in choices over future periods versus choices between the present and the future – the hypothesized driver of demand for commitment – is relatively small

[20] It should be noted that Laibson (2015) shows that demand for commitment is actually predicted by models of quasi-hyperbolic discounting for only a narrow set of parameters, which could also explain its paucity.

compared to choices with or without waiting periods. Additionally, unlike commitment contracts, waiting periods are frequently found in the real world. Encouraging deliberation through the use of waiting periods for intertemporal decisions may be an underutilized policy tool that is not costly and less paternalistic than forced commitment, but may have substantial benefits for choices over time.

The results from our field study study suggest scope for using waiting periods as a targeted intervention for individuals prone to myopic decisions. Unlike past experimental work on time preferences, individuals in our study did not make binding decisions about allocations between different future dates. We allowed for the possibility that in the Waiting Period treatment individuals would wake up on the day after receipt and decide to redeem their coupons immediately. This did not happen. Instead, individuals who had a waiting period before making their initial choice were substantially less likely to redeem the coupon early and earned more flour from the study than those who did not have a waiting period. This intervention was particularly effective for participants who were more prone to making early redemption choices in the Immediate treatment – those who had been previously exposed to violence.

Since waiting periods do not restrict individuals' choice sets nor the information received, they have potential to be expanded as a policy tool to encourage more patient decision-making across many different domains. For example, consider the tax 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 will significantly affect the choice of whether to spend the money or to save it. A firm offering to deliver the refund immediately through an Anticipation Loan, for example, may lead the individual to spend more of the sum as opposed to saving it, which could have substantial negative downstream consequences even if the loan came at no extra fee.

Additionally, though we focus on the effects of deliberative processing on intertemporal choice, our findings have implications for other decision domains where the use of heuristics has been documented. For example, the availability and representativeness heuristics have been implicated in biasing risk perception, leading to systematic insensitivity to prior probabilities and beliefs in illusory correlations, respectively (Kahneman and Frederick, 2002). Based on the results presented here, introducing prompts for deliberative processing such as a waiting period should decrease reliance on these heuristics and lead to less biased choices under uncertainty. Future research should explore the scope of using waiting periods to reduce behavioral biases in other decision contexts.

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A Appendix

A.1 Structural Estimation in the Consumption Goods Study

We conceptualize the data as a series of binary choices –to redeem the coupon for its present value or to wait. For example, consider an individual making a choice on the first redemption-eligible day of the Waiting Period treatment (the day after they get the coupon. She can either redeem the coupon for one bag of flour (about a kg) or wait until the next day. This is a binary choice based on comparing the utility of one bag of flour on the current day and the maximum of the utilities from two bags the next day, three bags the day after that, etc. She will redeem the coupon for one bag if

$$1 > \max\{\delta 2^{\alpha} + \epsilon_2, \delta^2 3^{\alpha} + \epsilon_3, \delta^3 4^{\alpha} + \epsilon_4, \delta^4 5^{\alpha} + \epsilon_5\} \quad , \tag{3}$$

where $\{\epsilon_2, ...\epsilon_5\}$ are the normal, i.i.d. unobservable components of coupon redemption on each possible future redemption date, α is the utility curvature parameter defined over bags of flour and δ is the daily discount rate.[21] B, the deliberation parameter, does not appear because the individual has been forced to consider their choice for at least a day before making their first decision.

Calling the probability of observing redemption on that first eligible date $Pr(c = 1)$,

$$Pr(c = 1) = Pr(1 > \delta 2^{\alpha} + \epsilon_2) \cdot Pr(1 > \delta^2 3^{\alpha} + \epsilon_3) \cdot Pr(1 > \delta^3 4^{\alpha} + \epsilon_4) \cdot Pr(1 > \delta^4 5^{\alpha} + \epsilon_5) \quad , \tag{4}$$

which becomes

$$Pr(c = 1) = \Phi(1 - \delta 2^{\alpha}) \cdot \Phi(1 - \delta^2 3^{\alpha}) \cdot \Phi(1 - \delta^3 4^{\alpha}) \cdot \Phi(1 - \delta^4 5^{\alpha}) \tag{5}$$

[21] Assuming unobservable components of only future utilities from the decision-maker's point of view makes the construction of choice probabilities much simpler by allowing for independence of the unobservables of each pairwise comparison.

using the normality assumption. Choosing to redeem the coupon on the day after the first eligible date is thus the probability that it is not redeemed on the first eligible date and that the utility on the second eligible date is greater than the max of the utilities of the options that could be attained by waiting again.

$$Pr(c = 2) = (1 - Pr(c = 1)) \cdot Pr(2 > \max\{\delta 3^{\alpha} + \epsilon_3, \delta^2 4^{\alpha} + \epsilon_4, \delta^3 5^{\alpha} + \epsilon_5\} \quad , \tag{6}$$

which using independence and normality becomes

$$Pr(c = 2) = (1 - Pr(c = 1)) \cdot \Phi(2 - \delta 3^{\alpha}) \cdot \Phi(2 - \delta^2 4^{\alpha}) \cdot \Phi(2 - \delta^3 5^{\alpha}) \quad . \tag{7}$$

It follows that

$$Pr(c = 3) = (1 - Pr(c = 1)) \cdot (1 - Pr(c = 2)) \cdot \Phi(3 - \delta 4^{\alpha}) \cdot \Phi(3 - \delta^2 5^{\alpha}) \quad , \tag{8}$$

$$Pr(c = 4) = (1 - Pr(c = 1)) \cdot (1 - Pr(c = 2)) \cdot (1 - Pr(c = 3)) \cdot \Phi(4 - \delta 5^{\alpha}) \quad , \tag{9}$$

and

$$Pr(c = 5) = (1 - Pr(c = 1)) \cdot (1 - Pr(c = 2)) \cdot (1 - Pr(c = 3)) \cdot \Phi(\delta 5^{\alpha} - 4) \quad . \tag{10}$$

Only minor adjustments are required to model choices in the Immediate condition. In this case, the B parameter matters for the decision to redeem on the earliest possible date. Calling the probability of that choice $Pr_I(c = 1)$:

$$\begin{aligned} Pr_I(c = 1) = Pr(1 > B\delta 2^{\alpha} + \epsilon_2) \cdot Pr(1 > (B\delta)^2 3^{\alpha} + \epsilon_3) \cdot \\ Pr(1 > (B\delta)^3 4^{\alpha} + \epsilon_4) \cdot Pr(1 > (B\delta)^4 5^{\alpha} + \epsilon_5) \quad , \end{aligned} \tag{11}$$

which becomes

$$Pr_I(c = 1) = \Phi(1 - B\delta 2^{\alpha}) \cdot \Phi(1 - (B\delta)^2 3^{\alpha}) \cdot \Phi(1 - (B\delta)^3 4^{\alpha}) \cdot \Phi(1 - (B\delta)^4 5^{\alpha}) \quad . \tag{12}$$

The B parameter doesn't affect any subsequent choices in the Immediate treatment, so the choice probability expressions for $Pr_I(c = 2)$ to $Pr_I(c = 5)$ are the same as in the Waiting Period treatment, except for the replacement of $Pr(c = 1)$ with $Pr_I(c = 1)$ in each nested location.

These probability expressions are used to construct a log-likelihood function which we estimate via maximum likelihood, with separate B parameters for the exposed and unexposed groups. We estimate substantial utility curvature of $\alpha = 0.186$ (S.E. $= 0.199$) and a high daily discount factor of $\delta = 0.830$ (S.E. = 0.055). The low α and δ help the model match the fact that roughly 61% of our subjects select interior redemption choices –coupons redeemed for more than the minimum but less than the maximum value. Following from our treatment effects, both B_u and B_e, the deliberation parameters for the unexposed and exposed groups, respectively, are significantly less than one, with B_e is roughly half the size of B_u. For the group previously exposed to violence, we estimate $B_e = 0.364$ (S.E. = 0.230, $p = 0.006$ tested against one), meaning that choices made right after notification are discounting by an additional 44% *per-period*. For the unexposed group we estimate $B_u = 0.734$ (S.E. = 0.110, $p = 0.016$ tested against one). While B_u is much closer to one than B_e, we emphasize that this is a very substantial deviation in discounting behavior from those in the Waiting Period treatment; even for the unexposed population, the period leads to substantially more patient choices. Although the difference between the two point-estimates is fairly large from a decision-making point of view, our estimates are not precise enough for it to be statistically significant at conventional levels ($p = 0.138$).

A.2 Structural Estimation in the Effort Allocation Study

We alter the perspective of our model for estimation slightly so that choices in all treatments can be considered in a two-period model. Call x_1 tasks allocated to Work Period 1 and x_2 tasks allocated to Work Period 2 and r the the interest rate by which undone tasks grow. We model subjects as choosing from the convex choice set by solving

$$\min_{x_1, x_2} U(x_1, x_2) = x_1^\gamma + \beta^T B^N \delta x_2^\gamma \quad \text{s.t.} \quad x_1 + \frac{x_2}{1+r} = 40 \quad , \tag{13}$$

where δ is their discount factor between periods, N is the indicator for whether the decision maker did not experience a waiting period prior to their choice, T is an indicator for whether WP1 takes place in the same time period as the choice is made, and γ governs the instantaneous disutility of effort function. B and β are still defined as the deliberation and present bias parameters, respectively.

We make two additional adjustments to allow for more flexibility in terms of effort cost. First, we add Stone-Geary 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. We assume that background utility does not systematically vary with time independently of the task allocation decision. This means that we can view choices in all of our treatments as two-period allocations between WP1 and WP2. 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 x_1 in the Work Period 2 effort level. Our utility function is thus

$$U(x_1, x_2) = (x_1 + \omega_1)^\gamma + \beta^T B^N \delta(x_2 + \omega_2 + \omega_3 x_1)^\gamma \quad . \tag{14}$$

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

$$x_1^* = \frac{40Z(1+r) + \omega_2 Z - \omega 1}{1 + Z(1+r) - \omega_3 Z} \quad , \tag{15}$$

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

$$x_{1,(i,t)} - \frac{40Z_t(1+r_t) + \omega_2 Z_t - \omega 1}{1 + Z_t(1+r_t) - \omega_3 Z_t} + \epsilon_{i,t} = 0 \quad , \tag{16}$$

where $x_{1,(i,t)}$ is our observed choice for period 1 tasks by person i on task t. The likelihood associated with that observation is

$$\phi\left(x_{1,(i,t)} - \frac{40Z_t(1+r_t) + \omega_2 Z_t - \omega 1}{1 + Z_t(1+r_t) - \omega_3 Z_t}\right) \tag{17}$$

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 $x_{1,(i,t)} = 0$, then we assume that

$$\epsilon_{i,t} > \frac{40Z_t(1+r_t) + \omega_2 Z_t - \omega 1}{1 + Z_t(1+r_t) - \omega_3 Z_t} \quad , \tag{18}$$

and the likelihood contribution is

$$\Phi\left(- \frac{40Z_t(1+r_t) + \omega_2 Z_t - \omega 1}{1 + Z_t(1+r_t) - \omega_3 Z_t} \right) \quad . \tag{19}$$

If $x_{1,(i,t)} = 40$, then we assume that

$$\epsilon_{i,t} < \frac{40Z_t(1+r_t) + \omega_2 Z_t - \omega 1}{1 + Z_t(1+r_t) - \omega_3 Z_t} - 40 \quad , \tag{20}$$

and the likelihood contribution is

$$\Phi\left(\frac{40Z_t(1+r_t) + \omega_2 Z_t - \omega 1}{1 + Z_t(1+r_t) - \omega_3 Z_t} - 40 \right) \quad . \tag{21}$$

In our two binary choice tasks, subjects simply select the smaller value between $(40+\omega_1)^\gamma$ and $\beta^T B^N \delta(40(1+r)+\omega_2)^\gamma$. 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

$$\begin{aligned} Pr(x_{1,(i,t)} = 40) = Pr((40+\omega_1)^\gamma - \beta^T B^N \delta(40(1+r_t)+\omega_2)^\gamma + \epsilon_{i,t} < 0) = \\ \Phi(\beta^T B^N \delta(40(1+r_t)+\omega_2)^\gamma - (40+\omega_1)^\gamma) \end{aligned} \tag{22}$$

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

$$\begin{aligned} Pr(x_1 = 0) = Pr((40+\omega_1)^\gamma - \beta^T B^N \delta(40(1+r)+\omega_2)^\gamma + \epsilon_{i,t} > 0) = \\ \Phi((40+\omega_1)^\gamma - \beta^T B^N \delta(40(1+r_t)+\omega_2)^\gamma) \end{aligned} \tag{23}$$

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. When we estimate

γ, a value greater than one is returned, meaning that the second-order conditions of the minimization problem hold. For the purposes of estimation, we replace $\beta^T B^N \delta$ in the above equations with $D_I I + D_W W + D_C C$, where I, W, C are treatment indicators for Immediate, Waiting Period and Commit, respectively. Expressed in terms of the present-bias parameter β and the deliberation parameter B, $D_I = \delta\beta B$, $D_W = \delta\beta$ and $D_C = \delta B$. Choices in the Immediate treatment are made right after the choice set is learned, meaning $N = 1$ and B applies, and the choices have immediate utility consequences, meaning $T = 1$ and β also applies. The Waiting Period and Commit treatments each remove one of those influences, allowing us to estimate $B = \frac{D_I}{D_W}$ and $\beta = \frac{D_I}{D_C}$.

We estimate $\gamma = 1.25$ (S.E. = 0.05), indicating increasing marginal disutility of performing the counting task. There is no evidence on any background effort level in Work Period 1 ($\omega_1 = 0$, degenerate standard error), but there is evidence of background effort in Work Period 2 ($\omega_2 = 4.71$, S.E. = 2.46). Additionally there is some evidence of effort spillover across period ($\omega_3 = 0.25$, S.E. = 0.02). We estimate discount factors of $D_I = 0.97$ (S.E. = 0.07), $D_W = 1.15$ (S.E. = 0.11), and $D_C = 1.06$ (S.E. = 0.09). They very short time horizon means that we should expect no discounting from an exponential model point of view. Indeed, none of the estimates is significantly different from one ($p = 0.65, 0.18$ and 0.48 respectively).