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Is Generosity Time-Inconsistent? Present Bias across Individual and Social Contexts

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Abstract

We investigate dynamically inconsistent time preferences across contexts with and without interpersonal trade-offs. In a longitudinal experiment participants make a series of intertemporal allocation decisions of real-effort tasks between themselves and another person. Our results reveal that agents are present-biased when making choices that only affect themselves but not when choosing for others. Despite this asymmetry, we find no evidence for time-inconsistent generosity, i.e., when choices involve trade-offs between own and other's consumption. Structural estimations reveal no individual-level correlation of present bias across contexts. Discounting in social situations thus seems to be conceptually different from discounting in individual situations.

Keywords: Present bias; altruism; stability; real effort; dictator game; intertemporal choice.

JEL Classification Numbers: C91; D64; D90

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1 Introduction

When faced with intertemporal trade-offs, many economic decision makers display a present bias, i.e, a desire for immediate gratification that leads them to become disproportionally more impatient when choices directly affect the present (Strotz, 1956; Loewenstein and Prelec, 1992; Laibson, 1997; O'Donoghue and Rabin, 1999; Frederick et al., 2002). Evidence for this comes from a variety of settings, such as financial decision-making (Ashraf et al., 2006), exercising (DellaVigna and Malmendier, 2006), and effort provision (Augenblick et al., 2015; Le Yaouanq and Schwardmann, 2019), supporting the notion that intertemporal decision-making is often time-inconsistent. The existing body of evidence almost exclusively focuses on present bias in situations in which only own consumption is at stake. Yet, intertemporal trade-offs also play an important role in *social situations*, in which choices also affect the well-being of others. However, so far, relatively little is known about how people trade-off own and others' payoffs that occur at different points in time, and how social preferences unfold in such dynamic contexts.

In this paper, we provide a systematic analysis of time discounting in individual and social contexts within a unified framework. To this end, we consider two types of situations, one in which either only own or only others' consumption is at stake, and another in which there is a trade-off between own and others' consumption. We seek to answer the following two questions. First, do economic agents exhibit time-inconsistent generosity? That is, do people become more (or less) generous when the consequences of their actions are delayed, and does this effect depend on whether the delay affects the immediacy of consumption or not? The potential presence of a time-inconsistency in generosity may not only have important implications for the modeling of social preferences in intertemporal contexts, but can further inform policy makers in how to design regulations aimed at fostering prosocial behavior. For example, to gather support for redistributive policies which require giving up one's own income for the benefit of the socially disadvantaged, policy makers may be able to leverage the factor time in order to promote such policies. Depending on how voters discount redistribution that happens in the future, politicians can benefit from announcing them well in advance (if generosity is substantially present-biased) or without much advance notice (if generosity is future-biased). Similarly, charities collecting donations, NGOs recruiting volunteers, or firms or organizations searching for helpers to work on onerous tasks (e.g., organizing a company event, cleaning shared facilities, writing a referee report) might evoke very different degrees of generosity depending on whether requests are made in advance or on the spot.

Second, we ask whether there is a difference between present bias in own consumption compared to consumption decisions which are taken on behalf of others. Situations in which individuals make intertemporal decisions for others are frequent. Think, for instance, of asset managers investing on behalf of their clients, doctors choosing treatments for their patients, or parents deciding what is best for their children. As already argued by Schelling (1984), in many situations, casual observation suggests that agents might be willing to delegate choices to friends or family in the belief that when they choose on one's behalf, they are to a lesser extent subject to temptations.[1] Put differently, if present bias represents an impulsive, temptation-driven desire for immediate gratification as, for example, argued by McClure et al. (2004), one could expect that agents evaluate others' consumption in a less biased, more controlled and analytical manner.[2]

While at first sight the two research questions seem to be distinct from one another, upon reflection is becomes clear that they are closely intertwined. Specifically, when deciding about how generous to be towards others in the future, it seems natural to assume that individuals think about and take into account how the other person discounts future consumption. Furthermore, it seems reasonable to expect that agents who in individual contexts are willing to increase own instantaneous consumption at the expense of their future selfs, display a similar desire for immediate gratification in situations in which the costs of such behavior are borne by someone else. These considerations suggest a tight connection between a present bias in individual and social contexts.

To formalize these ideas, we develop a theoretical framework which parallels the multi-attribute utility approach used by Andersen et al. (2018) and Cheung (2015) for analyzing intertemporal risk preferences. Specifically, we propose a utility function which allows for differences in discounting of own and others (atemporal) utility, while at the same time accounting for equality-efficiency trade-offs in own consumption versus another person's consumption. The key insight from our analysis is that if individuals exhibit differences in present bias between own and others' consumption, generosity is subject to time inconsistency. In particular, if individuals are more present-biased for themselves, this increases the relative weights of own vis-à-vis others' consumption when consequences are immediate rather than delayed. As a consequence, plans to behave generous in the future will be replaced by more selfish ones, once the present arrives. If, on the contrary, there are no differences in relative discounting between self and others, altruistic behavior should be time-consistent, and thus unaffected by the timing of decisions and consequences as in this case, the relative weight of own compared to others' consumption is constant over time.

[1] Schelling (1984) lists a number of examples, including handing over car keys to others when drinking, telling friends not to lend them money (when in a casino, for example), or relying on groups to commit to lose weight. We view these examples as plausibly supporting the notion that when evaluating others' consumption, agents might be less (or not at all) present-biased, but do not delve deeper into the related, but separate, question of whether we should observe delegation of choices to others in addition or as an alternative to commitment devices provided by markets. We note, however, that implicit in the delegation argument is that one can trust the other person enough to "do the right thing".

[2] To our knowledge, in the existing literature, only Albrecht et al. (2011) directly compare present bias for oneself and another person, but, in a setting quite different from ours, find no aggregate effect of a difference in present bias. Other studies have focused on patience rather than present bias per-se, finding mixed evidence (Shapiro, 2010; Howard, 2013; Rodriguez-Lara and Ponti, 2017; Rong et al., 2019; de Oliveira and Jacobson, 2021).

To test for these conjectures, we run a three-week longitudinal experiments with a total of $n = 240$ participants. Participants are asked to make intertemporal allocation decisions of units of effort (i.e., negative leisure consumption) for varying prices using a convex budget set approach (Andreoni and Miller, 2002; Fisman et al., 2007; Andreoni and Sprenger, 2012). The effort task is based on Erkal et al. (2011) and consists of encrypting a string of letters into numbers. Like in Augenblick et al. (2015), allocation decisions are made at two points in time – an initial allocation in week 1, and a subsequent allocation in week 2 – while effort needs to be exerted in week 2 or in week 3. To incentivize all decisions, after participants complete their week 2 decision, we randomly select one decision – either from week 1 or from week 2 – to be implemented and determine participants' workload. Differences between initial and subsequent allocation decisions allow for a precise measurement of dynamic inconsistency.

Each participant makes choices in two types of allocation decisions. In the first, participants face intertemporal trade-offs that either only affect themselves or only affect another person, i.e., choices in which there is no conflict between own and others' consumption. These choices allow us to test whether there is any difference in the discounting of own and others' consumption in the absence of any interpersonal trade-offs. We call these decisions *intrapersonal choices*. In the second type of allocation decisions, participants face intertemporal trade-offs in a social context in which they allocate tasks between themselves and another person. In contrast to choices in standard (static) dictator games, we systematically vary the timing of when the consequences for the decision maker and the recipient realize; either both immediately, both delayed, or one delayed and the other immediately. We refer to these decisions as *interpersonal choices*. Based on our theoretical framework, the variations in the timing of consequences and decisions allow us to structurally estimate time preference parameters for own and others' consumption in the individual and the social domain, and to compare their stability across domains.

The results from our intrapersonal choices reveal substantial differences in discounting for oneself and others. In particular, we find that when deciding for themselves, participants allocate 4.9% more tasks to the sooner date when deciding in advance rather than in the present. Our structural estimations reveal that this implies a present bias parameter, β_s, of 0.909 to 0.916, which is statistically different from one, a finding that is consistent with work by Augenblick et al. (2015) for slightly different tasks and procedures. When participants decide on behalf of someone else, in contrast, we find a decrease of only 1.2% across the two decision dates. Our structural estimations indicate that this corresponds to present bias estimates for others consumption, β_o, between 0.969 and 0.972, which are significantly larger than the ones obtained for own consumption, and not significantly different from one.

Given the asymmetry in present bias when deciding for oneself or on behalf of another person, the prediction based on our theoretical framework is that generosity should be subject to time-inconsistency. In particular, what we would expect to observe is that in allocations where both agents need to complete the tasks in week 2, participants allocate substantially more tasks to themselves when choosing in advance (week 1) rather than in the present (week 2). When both agents need to work in week 3, in contrast, the number of tasks allocated to oneself should decrease to a lesser extent (if at all) between the two weeks. This is not what we find. Instead, our results reveal that generosity is relatively unaffected by the timing of consequences. In particular, we find no evidence for present-biased generosity because there is no significant difference between allocations made for week 2 and allocations made for week 3. Structural estimations confirm this finding by revealing that for the choices in the dictator games we cannot reject that β_s is equal to β_o.

Taken together, our results reveal that while there are pronounced differences in the way agents discount own and others' consumption when considered in isolation, this asymmetry vanishes when simultaneously evaluating own and others' consumption in choices in which there are trade-offs between the two. This result is further corroborated when examining the individual-level stability of present bias across the two contexts. Our results reveal no significant correlation of present bias estimates across the two domains, neither for own consumption nor for the consumption of others. This further strengthens our aggregate findings, suggesting that the way an agent discounts one's own and another person's consumption seems to be conceptually different depending on whether choices involve interpersonal trade-offs or not. One potential explanation for this findings is that, based on insights from multi-attribute choice (see e.g., Houston and Sherman, 1995), individuals may become less sensitive towards the timing of decisions when moving from choices that only affect themselves, to choices that affect both themselves and others. Evidence consistent with this line of reasoning comes from Cubitt et al. (2018), who find that time matters less when intertemporal decisions involve comparing options of different types (e.g., apples now versus oranges later) compared to decisions that only involve options of the same type (e.g., apples now versus apples later). We discuss this and other potential explanations for our findings in more detail in Section 6.

Our paper contributes to two so far largely unrelated strands of the literature that have been of central interest in economic research. On the one hand, our study contributes to the literature on time preferences and dynamically inconsistent behavior, one of the main pillars of behavioral economics (see Frederick et al., 2002; Cohen et al., 2017; Ericson and Laibson, 2019, for reviews of the literature). On the other hand, our paper contributes to the literature on other-regarding preferences (see Sobel, 2005; Cooper and Kagel, 2009, for reviews of the literature), and, more specifically, altruistic behavior in dictator games (e.g., Forsythe et al., 1994; Hoffman et al., 1996; Engel, 2011). Yet, while studies investigating time preferences have almost exclusively focused on individual-decision contexts, the literature on prosocial behavior has mainly looked at static situations, ignoring the intertemporal component inherent in most real-world situations.

The extension of social preferences to dynamic situations has received some recent interest in the literature: Breman (2011) (in a field experiment) and Andreoni and Serra-Garcia (2019) (in a lab experiment) find that charitable donations can be increased when agents are asked to commit to future donations, rather when asked to donate on the spot. In contrast to this, Kovarik (2009) and Dreber et al. (2016) study dictator game giving and find that giving decreases when delaying both the own and the recipient's monetary payments to the same extent. Our paper is distinct from these studies in that we estimate time preference parameters structurally, and that we compare present bias across individual and social contexts. Moreover, we study generosity in the effort domain rather than via monetary transfers, addressing the concerns that (i) generous acts in the field such as helping a friend or a colleague often occur in the non-monetary domain, and (ii) laboratory experiments may not be well suited to capture present bias in money (Augenblick et al., 2015; Balakrishnan et al., 2020). Yet, despite these differences, the fact that we do not find any strong effect of time on generosity fits with the inconclusive evidence provided by previous literature. As we show in Section 5, one potential explanation for the mixed evidence lies in the presence of a high degree of individual heterogeneity; while some individuals become more generous when asked in advance, for a similarly large fraction of individuals the opposite is true.

The remainder of the paper is organized as follows. The next section presents the design of our experiment. In Section 3, we provide a theoretical framework for the analysis of intertemporal choice when decisions affect both oneself and others. Section 4 analyzes the data from our intrapersonal choices and Section 5 presents the results from our interpersonal decisions. In Section 6 we discuss the implications of our results in light of our and alternative theories. Section 7 concludes.

2 The Experiment

Our experiment investigates participants' allocation decisions about the completion of a real-effort encryption task. Similar to Augenblick et al. (2015), we implement a longitudinal experiment that takes place at three dates over three consecutive weeks. All meetings are conducted in the laboratory, and all participants are required to participate at all dates of the experiment. In the first two weeks, participants have to make a series of allocation decisions that can affect their own as well as another participant's workload in week 2 and week 3. In the following, we present the experimental design in more detail. First, in Section 2.1, we describe the real-effort task participants have to work on. In Section 2.2, we then present the decision environment in which effort allocations are made. Finally, in Section 2.3 we provide details about the general experimental procedures, payments, and recruitment.

2.1 Encryption Task

Our encryption task is based on Erkal et al. (2011). In this task, participants have to encode a string of letters (a "word") to numbers. Each word consists of eight letters. The numbers are given by an encryption table, showing all 26 letters of the alphabet as well as corresponding three-digit numbers. The participants' task is to type in the correct three-digit number corresponding to each letter into an empty textbox (see Figure 1 for a screenshot). After all eight letters are encrypted, participants have to press a "submit" button. If the task is solved correctly, a new word appears, along with the information about the total number of correctly solved tasks so far and the remaining number of tasks to solve. In case of an incorrect entry, participants are informed about their mistake (this happened in less than 4% of the cases). In this case, all entries are deleted and participants have to encrypt the same word again. There is no time limit for correctly encrypting a word.

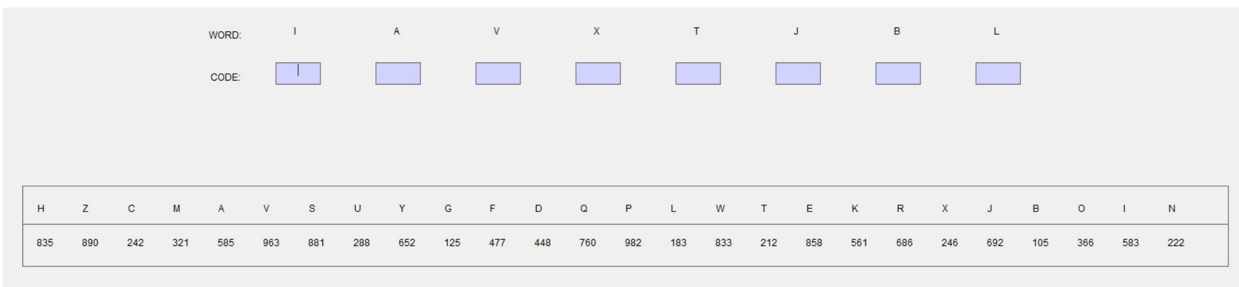

Figure 1: Screenshot of the encryption task

To mitigate learning effects over time and to make the exertion of effort as comparable as possible across the different dates of our experiment, we use a double randomization technique, as introduced by Benndorf et al. (2019). After each correctly solved word, each letter is associated to a new, randomly allocated, three-digit number, and the position of all letters is randomly reshuffled.[3]

2.2 Effort Allocations

In both week 1 and week 2, participants make a series of allocation decisions in which they have to allocate tasks between between week 2 and week 3. We distinguish between two types of decisions, which we refer to as *intrapersonal* and *interpersonal* allocation decisions. In the intrapersonal allocation decisions participants make choices in two blocks without any interpersonal

[3]It seems that we were largely successful in our attempt to mitigate learning effects. While in week 1 participants took on average 39.0 seconds per task, in weeks 2 and 3 this number slightly drops to 36.3 and 35.4, respectively. These numbers are based on the minimum work of 10 tasks that each participant has to complete each week, as discussed below.

Decision Type	Block	X	Y
Intrapersonal	SELF	s_t	s_{t+1}
	OTHER	o_t	o_{t+1}
Interpersonal	SOONSOON	s_t	o_t
	LATELATE	s_{t+1}	o_{t+1}
	SOONLATE	s_t	o_{t+1}
	LATESOON	s_{t+1}	o_t

Table 1: Allocation decisions within each of the six blocks

trade-offs. In particular, in block SELF participants choose how many tasks they want to solve in week 2 and how many tasks they want to solve in week 3. In block OTHER they face the exact same trade-off but now choose on behalf of another participant.

In the interpersonal allocation decisions participants make choices in four blocks. Here they have to decide, similar to standard dictator games, how many tasks they want to solve themselves and how many tasks another person has to solve. In two out of these four blocks, the time at which effort needs to be exerted is the same for the dictator and the receiver. In block SOONSOON agents decide about allocations of tasks which need to be completed in week 2, while in block LATELATE the decision environment is the same but the working date is week 3. In the following, we refer to these blocks as *symmetric dictator games*. In the other two blocks, the time at which the agents need to exert effort differs, we therefore call them *asymmetric dictator games*. In SOONLATE, the dictator has to work in week 2, while tasks allocated to the recipient have to be completed in week 3. In LATESOON, the roles are reversed such that the dictator has to work in week 3 and the recipient has to work in week 2. The order in which participants face these six blocks was randomized.[4]

Allocations are made in a convex time budget (CTB) environment (Andreoni and Sprenger, 2012). Participants allocate tasks between two accounts, X and Y, whereby the exchange rate between X and Y differs from decision to decision. In particular, every task allocated to account Y reduces the number of tasks allocated to account X by R. Within each block, we use the following six rates: $R \in \{0.5, 0.75, 1, 1.25, 1.5, 2\}$. For example, a rate of 0.5 implies that each task allocated to account Y reduces the number of tasks allocated to account X by 0.5. Formally, a participant thus faces a budget constraint of the form $X + R \cdot Y = m$.

In each decision $m = 50$, hence, since negative number of tasks are not allowed, a participant

[4]The randomization was as follows: Half of the participants face the intrapersonal allocations first, followed by the symmetric dictator games and vice versa for the other half (always SELF before OTHER and SOONSOON before LATELATE). We then independently randomize whether these four blocks are followed by LATESOON or SOONLATE, leaving us with four different orderings. We do not find any evidence for systematic order effects.

Figure 2: Screenshot of the allocation environment

can allocate at most 50 tasks to account X, while for account Y the maximum varies between 25 tasks ($R = 2$) and 100 tasks ($R = 0.5$). Depending on the block, account X and Y had different meanings. This is summarized in Table 1, where s stands for tasks allocated to oneself (*self*) and o stands for tasks allocated to someone else (*other*). The subscript indicates the time when the tasks have to be solved, t corresponds to week 2, and $t+1$ corresponds to week 3. As an example, Figure 2 shows a screenshot of the allocation environment in block SoonSoon.

The real-effort task that we chose mandates that the number of allocated tasks is discrete. As Chakraborty et al. (2017) point out, in Augenblick et al. (2015) the authors chose a rounding method that leads to dominated choices being available to participants, and participants do indeed choose such dominated allocations. In our design this is not the case as we remove allocations in a way that no dominated allocations can be chosen.[5] This approach seems most favorable as these violations may often be simply due to participants being unaware that dominant options are available.

Each participant makes a total of 72 allocation decisions: 36 in week 1 and 36 in week 2 (six blocks with six different task rates each). Importantly, participants in week 1 are informed that they will have to make allocation decisions in week 2 again, but they are not reminded of their initial week 1 allocations in week 2. After all decisions have been made in week 2, participants are randomly assigned the role of "decision maker" or "receiver". Roles are assigned by letting participants draw a colored card out of a bag containing the same number of blue and red cards.

[5] More precisely, we allow for $X \in \{0, 1, 2, ..., 49, 50\}$ and, as a first step, round all Y to the closest integer. For $R > 1$, this leads to cases where two allocations (X, Y) and (X', Y) with $X > X'$ are both available. As a second step, we remove such "double appearances" in Y by keeping the allocation which does not contain a rounded value. For example, when $R = 2$ we have $(0, 25)$ and $(1, 25)$ and remove the latter. If both allocations contain rounded values, we remove the dominant alternative of the two, e.g., for $R = 1.25$ we remove $(2, 38)$ and keep $(3, 38)$.

	Minimum work	Allocation decisions	Allocation that counts chosen	Complete work
Week 1	✓	✓		
Week 2	✓	✓	✓	✓
Week 3	✓			✓

Table 2: Summary of the experiment

Then, pairs of one decision maker and one receiver are formed. After that, one of the 72 allocations of the decision maker is chosen at random as the "allocation that counts". The allocated number of tasks from this decision then determines how many tasks each participant of the pair has to complete on the two work dates, in addition to a minimum requirement of 10 tasks that need to be completed at the beginning of every week (see below).[6] This procedure ensures that each decision is elicited in an incentive-compatible way.

In addition to their choices, in each week participants are required to complete a "minimum work" of 10 encryption tasks prior to making their allocation decisions or completing their allocated tasks. As discussed in Augenblick et al. (2015, p.1077), this ensures that (i) at all dates participants incur the cost of coming to the lab, (ii) in week 1 participants get an idea how tedious the task is, and (iii) on both allocation dates, participants have gone through the same amount of work before making their choices. Table 2 summarizes our experimental design, containing all tasks participants face in each of the three weeks.

2.3 Recruitment, Payments, & Procedures

All sessions were computerized using the software Ztree (Fischbacher, 2007). We recruited participants using ORSEE (Greiner, 2015). In the invitation email, participants were informed about the longitudinal nature of the experiment. In particular, they were told that the experiment consists of three experimental sessions that each lie one week apart from each other. They were further told that they should only register if they can ensure that they participate at all three dates. The sessions took always place at the same day of the week, the same time of the day, and in the same laboratory. Before each session, participants were send an email reminder about the remaining sessions. When invited for the experiment, participants were informed that the total average time of the experiment would be around 3 hours, but that the duration of each session could vary between 15 and 90 minutes.

[6] In case a decision from block SELF or OTHER is selected, the respective other person only has to complete the minimum work. Similarly, in cases where the selected allocation decision does not specify any work by design, e.g., week 3 in block SOONSOON, only the minimum work has to be completed.

If participants showed up to all three experimental sessions and completed all tasks as specified by the randomly selected allocation, they received a completion payment of €40. If they failed to show up to one of the sessions in weeks 2 or 3, they were still eligible for a payment of €4, which corresponds to the usual show-up fee paid to participants at the Cologne Laboratory of Experimental Research (CLER) where this study was run. All payments were administered at the end of the third session in week 3 and participants knew this in advance.

At the beginning of each experimental session, participants received written instructions that were also read aloud by one of the experimenters. Instructions contained detailed information about the timeline of the experiment as well as the tasks to be solved in each of the three weeks (see Appendix H for a copy of the instructions). After that, in each of the three weeks participants had to complete the minimum work of 10 encryption tasks. Subsequently, in week 1 and week 2 participants made their allocation decisions. In week 1, the session ended after the allocation decisions, followed by a short demographic questionnaire. In week 2 (after the allocation decisions) and week 3 (after the minimum work) participants had to solve the number of tasks as specified by the allocation that counts. After completing all tasks, participants could silently leave the lab without disturbing the other participants. In week 3, participants received their payments immediately after completing their allocated tasks at their desk.

One concern with this procedure is that participants may fear that others could draw conclusions about their allocation decisions. This is particularly relevant for the dictator games as previous literature has shown that social image concerns can increase prosociality (Benabou and Tirole, 2006; Charness and Gneezy, 2008; Andreoni and Bernheim, 2009). Note, however, that given our random implementation of the allocation that counts, by design, about half of the participants in each session are expected to only complete the minimum work in a given week. As a result, it is almost impossible for participants to infer others' degree of selfishness or impatience from the time they spend in the lab. We are hence confident that such concerns played no role in our setup.

Our data comes from two sets of experiments, a first study with $n = 110$ participants, and a replication study with $n = 130$ participants. The replication study was pre-registered at the AEA registry, and its sample size was determined using power analysis.[7] Out of the $n = 240$ participants who took part in our experiments, $n = 223$ showed up and completed all tasks in week 2.[8]

One crucial requirement for being able to identify an individual's time preference parameters

[7]We determined the sample size based on the diff-in-diff when comparing blocks SOONSOON and LATELATE (see Section 5 for details). The standardized effect size from the original study is $d = 0.297$ and we aimed to have 80% power to detect a similar effect size at a significance level of 5%. See AEARCTR-0005042 for more details.

[8]An additional eight participants dropped out between week 2 and week 3. These participants appear not to be different from others based on their allocation tasks, indicating that they did not know or plan to not show up in week 3 when making their week 1 or week 2 decisions. We hence do not drop these participants from our analysis. All our results, however, are robust to excluding these additional participants.

is that we observe some variation in their allocation decisions. If in at least one week there is no variation in a participant's response to changes in the exchange rate R, behavior conveys limited information about time preferences. For example, in the interpersonal decisions, participants who always allocate zero tasks to themselves can easily be identified as being completely selfish, but nothing can be said about their time preferences in this context. Hence, in our analyses we only focus on those participants that do exhibit some positive amount of variation in their allocation decisions in both week 1 and week 2. For our block SELF (OTHER) analysis, we have to drop eight (nine) participants who display no variation in at least one of the two weeks, leaving us with a sample of $n = 215$ ($n = 214$) participants.[9] For our dictator game decisions, we find a total of $n = 58$ participants who do not exhibit any variation in at least one of the weeks, all of them because they do not allocate any tasks to themselves (37 out of these participants behave fully selfish in both weeks). Applying these restrictions means that our remaining sample is more generous than the average. Importantly, however, this selected sample is not more or less patient than the average. That is, when analyzing the intrapersonal decisions, we find that time preferences, and in particular present bias, do not differ between selfish and non-selfish participants. In Appendix G, we also provide robustness checks which relax our exclusion restrictions and confirm that the estimates are qualitatively very similar.

3 Present Bias and Generosity: Some Theory

The goal of this section is to develop a coherent framework for intertemporal choices over own and another person's consumption. We start with the simple case in which intertemporal decisions only affect own consumption. Consider a decision maker who decides in some period τ about how to allocate consumption in period(s) $t, t+1, \ldots$ where $t \geq \tau$. We denote own consumption in period t resulting from an allocation decision in period τ by $s_{t,\tau}$, which is evaluated by the utility function $u_s(\cdot)$. In the usual case where agents decide about pleasant consumption, $u_s(\cdot)$ is assumed to be increasing and (weakly) concave. In cases like in our experiment where agents allocate unpleasant tasks, it is easiest to think about these functions as increasing and (weakly) convex cost-of-effort functions which agents seek to minimize. Allowing for agents to be present-biased (Strotz, 1956; Laibson, 1997; O'Donoghue and Rabin, 1999; Frederick et al., 2002), an agent's utility at time t from a decision made at time τ can then be written as:

$$\beta_s^{\mathbf{1}\{t \neq \tau\}} \delta_s^{t-\tau} u_s(s_{t,\tau}) + \beta_s \sum_{k=1}^{T} \delta_s^{t-\tau+k} u(s_{t+k,\tau}) \tag{1}$$

[9] There is some overlap between our exclusion restrictions across the different blocks. Two participants are excluded in both SELF and OTHER, leaving us with $n = 208$ participants when analyzing both blocks jointly.

As is well known, if $\beta_s < 1$, the agent exhibits present bias, meaning that she discounts all future consumption by an additional factor which does not affect the relative discounting between any two future periods, but increases the importance of the present relative to all future periods.

In our experiment, agents in block SELF allocate their consumption according to the budget constraint $s_{t,\tau} + Rs_{t+1,\tau} = m$. Inserting this budget constraint into (1) yields the following first-order condition:

$$\frac{u_s'(s_{t,\tau})}{u_s'(s_{t+1,\tau})} = \frac{1}{R}\,\beta_s^{\mathbf{1}\{t\neq\tau\}}\;\delta_s \tag{2}$$

From this, it becomes apparent that in situations where agents face the same allocation problem (t is fixed) at two different points in time, namely once in the present ($\tau = t$) and once in advance ($\tau < t$), any differences in behavior can be attributed to $\beta_s \neq 1$. Given the prevalence of present bias ($\beta_s < 1$) as reported by previous literature (e.g., Augenblick et al., 2015; Imai et al., 2019), we predict:

Prediction 1. *In block* SELF, *participants allocate fewer tasks to the sooner date when decisions have immediate consequences (week 2) compared to when they decide in advance (week 1).*

Intuitively, in our experiment present bias leads agents to allocate more tasks to the later date when decisions are immediate ($\tau = 2$) because this reduces the immediate disutility from having to work more in the present. When decisions only affect the future ($\tau = 1$), in contrast, agents' decisions are not influenced by present bias, leading to time-inconsistent allocations.

Next, we discuss the case in which decisions only affect the consumption of others, $o_{t,\tau}$. In our experiment, these decisions are captured by block OTHER, in which agents face the same type of decisions as in block SELF, but now choose on behalf of another person. In light of the existing literature which analyzes decision making for others it seems natural to assume that preferences over own consumption differ from preferences over other people's consumption. For example, there is some empirical evidence showing that agents discount very differently when deciding for themselves rather than on behalf of another person (Shapiro, 2010; Albrecht et al., 2011; de Oliveira and Jacobson, 2021). Similarly, in the domain of risky decision making, Andersson et al. (2016) show that agents exhibit lower degrees of loss aversion when deciding for others rather than for themselves. In the most general form of our model, we thus shall allow both the time preference parameters as well as the atemporal utility function to differ depending on whether own or others' consumption is evaluated. In the following, we will therefore use the superscript o to describe β_o, δ_o and $u_o(\cdot)$. As discussed in the introduction, it seems plausible to think that when deciding for others, agents are less present-biased, possibly because they face less temptation for immediate gratification and decide in a more controlled and analytical manner. This leads to the following prediction:

Prediction 2. *In block* OTHER*, participants allocate a similar amount of tasks to the sooner date when they decide in advance (week 1) rather than when decisions have immediate consequences (week 2).*

Such behavior is consistent with $\beta_o = 1$, hence we assume that there is no present bias in others' consumption. A direct corollary from these two predictions is that when comparing decisions in block SELF with decisions in block OTHER, we should observe a stronger decrease from week 2 to week 3 in tasks allocated to the sooner date in own consumption than in others' consumption ($\beta_s < \beta_o$).

We now turn to the case in which decisions affect both own and others' consumption. While the specification in (1) is suitable for analyzing intrapersonal decisions, i.e., those decisions where there are no trade-offs between own and another person's consumption, for the choices in our dictator games, these trade-offs are important and hence need to be properly taken account by the model. The few papers in the relevant literature provide little guidance on what the appropriate model should be. We therefore turn to the literature on multi-attribute utility in the domain of risk and time preferences. Andersen et al. (2018) and Cheung (2015) analyze intertemporal choices under risk and propose a model in which the (concave) intertemporal utility function takes the sum of atemporal utilities as its argument, and a standard expectation operator captures the weights of the different states. While we do not have any risk in our setting, consumption for oneself and consumption for another person can, for the purposes of the modeling approach, be treated analogously to different states of the world. In particular, we can capture the trade-offs between self and other by introducing a and $1-a$, with $0 \leq a \leq 1$, as weights of own vis-à-vis others' consumption, and $\rho \geq 1$, which models the concavity of intertemporal utility.[10] This yields the following specification:

$$a \left(\beta_s^{\mathbf{1}\{t \neq \tau\}} \delta_s^{t-\tau} u_s(s_{t,\tau}) + \beta_s \sum_{k=1}^{T} \delta_s^{t-\tau+k} u_s(s_{t+k,\tau}) \right)^{\rho} + (1-a) \left(\beta_o^{\mathbf{1}\{t \neq \tau\}} \delta_o^{t-\tau} u_o(o_{t,\tau}) + \beta_o \sum_{k=1}^{T} \delta_o^{t-\tau+k} u_o(o_{t+k,\tau}) \right)^{\rho} \tag{3}$$

To understand the intuition behind the role of ρ, note that for $\rho = 1$, the discounted utility from own and others' consumption are perfect substitutes (i.e., preferences are linear) but as ρ increases, the agent's desire to smooth consumption between herself and the other person becomes stronger,

[10]Hence, compared to the specifications in Andersen et al. (2018) and Cheung (2015) for intertemporal choice behavior under risk, a and $1-a$ can be understood as analogous to states of the world which realize with probability p and $1-p$, respectively. ρ is the analogous of a coefficient of relative intertemporal risk aversion, as it captures how consumption is smoothed between oneself and another person.

which increases equality between individuals at the expense of reduced efficiency.[11] Note that if $u_i(\cdot)$ is linear and all decisions and all consumption takes place in period t, this formulation is analogous to the constant elasticity of substitution (CES) functional form used by, for example, Andreoni and Miller (2002) and Fisman et al. (2007).[12]

In the dictator games of our experiment, agents allocate consumption between themselves and another person according to the budget constraint $s_{t,\tau} + Ro_{t',\tau} = m$. When, like in our symmetric dictator games (blocks SOONSOON and LATELATE), own and others' consumption realize at the same time $t = t'$, while in the asymmetric cases (blocks SOONLATE and LATESOON) t and t' will be different. Inserting the budget constraint into (3) yields the following first-order condition:

$$\left(\frac{u_s(s_{t,\tau})}{u_o(o_{t',\tau})}\right)^{\rho-1} \frac{u_s'(s_{t,\tau})}{u_o'(o_{t',\tau})} = \frac{1}{R}\left(\frac{\beta_o^{\mathbf{1}\{t'\neq\tau\}}\delta_o^{t'-\tau}}{\beta_s^{\mathbf{1}\{t\neq\tau\}}\delta_s^{t-\tau}}\right)^{\rho} \frac{1-a}{a} \tag{4}$$

To illustrate how time preferences affect generosity, we first discuss the case in which own and others' consumption accrue at the same time ($t = t'$). The first observation is that if agents discount own and others' consumption to the same extent, i.e., if $\beta_s = \beta_o$ and $\delta_s = \delta_o$, the degree to which agents discount future consumption is irrelevant when deciding about optimal allocations. Intuitively, in this case discounting affects own and others' consumption in the same way, leaving relative preferences between the two unchanged. To understand how changes in the timing affect the allocations when this is not the case, i.e., if $\beta_s \neq \beta_o$ and/or $\delta_s \neq \delta_o$, note that due to the convexity of $u_s(\cdot)$ and $u_o(\cdot)$, the left hand side of equation (4) is increasing in $s_{t,\tau}$ and decreasing in $o_{t,\tau}$. Hence, an increase (decrease) in generosity caused by variation in τ or t must be caused by a decrease (increase) of the term $\left(\frac{\beta_o^{\mathbf{1}\{t'\neq\tau\}}\delta_o^{t'-\tau}}{\beta_s^{\mathbf{1}\{t\neq\tau\}}\delta_s^{t-\tau}}\right)$.

What does this imply for decisions made in our two symmetric dictator games in blocks SOONSOON ($t = 2$) and LATELATE ($t = 3$)? Recall that while in the latter decisions always have consequences that lie in the future, in the former decisions become immediate when moving from week 1 ($\tau = 1$) to week 2 ($\tau = 2$). Close inspection of (4) reveals that in block SOONSOON generosity is larger at $\tau = 1$ if and only if $\beta_s\delta_s < \beta_o\delta_o$ and vice versa. An agent thus becomes more (less) generous when deciding in advance if she discounts one-period delays in own consumption more (less) strongly than one-period delays in others' consumption. In block LATELATE, in contrast, differ-

[11] To make the role of ρ precise, consider the case where $u_s(\cdot) = u_o(\cdot)$ and $a = 0.5$, i.e., an agent who cares about her own workload exactly as much as about another person's workload. For $\rho = 1$, this agent is indifferent between the effort allocations $\{(s_t, s_{t+k}), (o_t, o_{t+k})\} = \{(10,20), (40,30)\}$ and $\{(s_t, s_{t+k}), (o_t, o_{t+k})\} = \{(40,20), (10,30)\}$. For $\rho > 1$, however, the agent prefers the second allocation because it allocates work more equally across the two people.

[12] The formulation in (3) improves upon the specification proposed by Shapiro (2010) and Rodriguez-Lara and Ponti (2017) who simply use different weights for the discounted utility of own consumption and others' consumption, respectively. This restricts social preferences to be linear in the sums of discounted utility. Allowing for $\rho \geq 1$ can, thus, account for a broader class of social preferences.

ences between initial allocations in week 1 and subsequent allocations in week 2 can be accounted for by $\delta_s \neq \delta_o$; initial allocations will be more generous if and only if $\delta_s < \delta_o$. The predictions for our two asymmetric dictator games follow the same logic. While in block SOONLATE any differences in generosity across weeks must be due to $\beta_s \delta_s \neq \delta_o$, in block LATESOON differences between initial and subsequent allocation decisions occur if $\delta_s \neq \beta_o \delta_o$.

From this it becomes clear that our model is, contrary to some alternative models (see discussion below), sufficiently flexible to account for different kinds of increases and decreases in generosity over time. However, under the assumption that time preferences are stable across intrapersonal and interpersonal contexts, precise and testable predictions can be derived for the dictator game behavior in our experiment. In particular, following Predictions 1 and 2, we should expect agents to exhibit present bias when evaluating own consumption in the dictator games, but not when evaluating others' consumption. Thus, by assuming that $\beta_s < \beta_o = 1$, we predict:

Prediction 3. *Agents exhibit present-biased generosity, that is a stronger decrease in generosity between initial and subsequent decisions in block* SOONSOON *compared to block* LATELATE.

The intuition for this prediction is straightforward. As outlined above, any differences in allocations across the two weeks in LATELATE have to be due to $\delta_s \neq \delta_o$ as consumption for both agents is always in the future. In SOONSOON, in contrast, consequences for both agents become immediate in week 2. Hence, differences in allocations across weeks can be either due to $\delta_s \neq \delta_o$ or due to $\beta_s \neq \beta_o$. Therefore, by calculating the *difference-in-difference* across the two blocks, we can isolate the pure effects of (relative) present bias on generosity. That is, irrespective of any potential differences in long-run discounting, under our assumption that $\beta_s < \beta_o$, we expect generosity to be time-inconsistent, as defined in Prediction 3.

Note that while such a shift is consistent with $\beta_s < 1$ and $\beta_o = 1$, based on the symmetric dictator games alone, we can only establish whether $\beta_s < \beta_o$ or not, but nothing can be said about the absolute levels of these parameters. To achieve identification of these parameters, we can, however, rely on the decisions in the asymmetric dictator games. In particular, similar to the logic above, we can use the difference-in-difference between allocations made in a symmetric and an asymmetric block to draw direct inference about either β_s or β_o.

To illustrate this, recall that a higher level of generosity in week 1 than in week 2 in SOONSOON can be attributed to $\beta_s \delta_s < \beta_o \delta_o$, while in LATESOON such a decrease in generosity would be due to $\delta_s < \beta_o \delta_o$. Hence, if indeed $\beta_s < 1$, we would expect the decrease in generosity in SOONSOON to be larger than in LATESOON. Intuitively, what happens is that when moving from week 1 to week 2, in SOONSOON the tasks for both agents become immediate, while in LATESOON this is only true for the other person's consumption. Hence, any differences between the two effects must be attributed to β_s as it measures the "importance of immediacy" in own consumption. In a similar manner, we can calculate all other possible differences-in-differences between a symmetric and an asymmetric block. In each case such a comparison is directly linked to the identification of either β_o or β_s.[13]

The asymmetric dictator games thus allow us to identify time preference parameters from the interpersonal choices alone, without having to rely on the intrapersonal decisions. This, in turn, allows us to provide an empirical test of whether, as assumed above, time preferences are indeed stable across the two decision contexts, or whether they are malleable and context-dependent.

We note that one key feature of our model is to allow time preference parameters to differ for oneself and the other person. The model does not, however, delve deeper into where such a difference may come from. In the introduction, we highlighted the possibility that if present bias is rooted in an impulsive and temptation-driven desire for immediate gratification (as, e.g., argued by McClure et al., 2004), agents might evaluate others' consumption in a less present-biased and more controlled and analytical manner. It may also be the case that in settings where unpleasant consumption is at stake, present bias arises because decision makers are too optimistic about how willing they are to work in the future, maybe because they do not fully anticipate negative shocks (such as being tired), as recently argued by Breig et al. (2020). When discounting others' consumption, agents may have different beliefs about such shocks, which is a different way of saying that they are less (or more) present-biased.[14] Our experiment is not designed to distinguish between these different (and any possible other) explanations.

Another key feature of our model is that it is based on the idea that people behave generously because they are truly altruistic in the sense that they care about the other person's well-being. In this case, it directly follows that the other person's utility should be discounted at the time when it realizes. If, in contrast, we would assume that generosity is primarily driven by feelings of "warm glow" (Andreoni, 1989; 1990), then it is less obvious whether such feelings should also be discounted. Instead, in this case it seems more natural to assume that, irrespective of when the other person receives the gift, the utility from warm glow flows immediately, i.e., when the decision to be generous is made. Andreoni and Serra-Garcia (2019) provide a formal model in this spirit by assuming that agents receive social image utility when a prosocial decision is made as well as when it materializes. In Appendix B we present a model which is based on a similar

[13] To be precise, the comparison between SoonSoon and SoonLate can be used for making inferences about β_o. Comparing LateLate and SoonLate can be used as an alternative to identify β_s, while comparing LateLate and LateSoon can be used as an alternative to identify β_o.

[14] Note that for the argument that there is a potential difference in present bias for own and others' consumption it suffices to assume that such beliefs about future shocks are different. Whether beliefs about own shocks are more accurate than those about others' matters – in the notation of our model – for whether $\beta_s < \beta_o$ or $\beta_s > \beta_o$. The former situation would correspond to the case where agents are too optimistic about, e.g., their own tiredness, but are correctly calibrated in predicting tiredness of others.

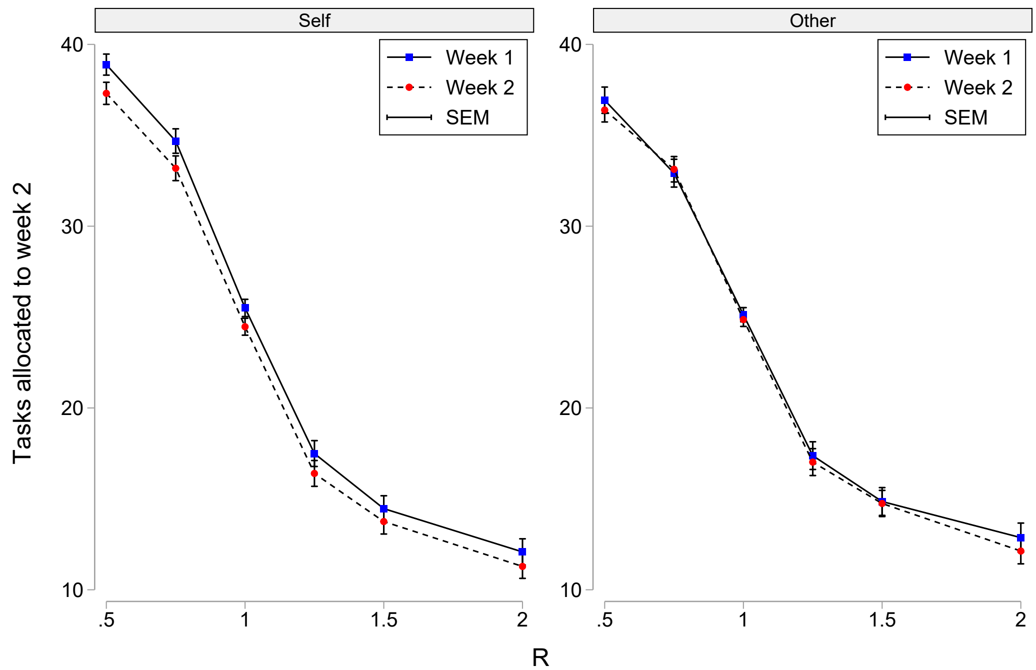

Figure 3: Effort allocations in intrapersonal decisions (SELF: $n = 215$, OTHER: $n = 214$)

assumption and show that it delivers predictions which are qualitatively similar to our model with $\beta_s < \beta_o$. Unlike our model, however, it cannot rationalize time-inconsistencies which lead to an increase in generosity for immediate decisions, i.e., $\beta_s > \beta_o$.

4 Effort Allocation in Intrapersonal Choices

We start our analysis by describing the choices made in the two intrapersonal blocks SELF and OTHER. This allows us to evaluate whether people discount own and others' consumption differently in situations in which there are no interpersonal trade-offs.[15]

4.1 Deciding for oneself

Our results for block SELF are summarized by the left panel of Figure 3. It depicts, for each week and task rate, the number of tasks allocated to the sooner work date in week 2. As can be seen, both lines are downward sloping, indicating that participants' choices follow a basic law of

[15]All results presented in this and the next section are based on the pooled data from our first and our replication study. See Appendix E for a breakdown of the results separately for the two waves.

Rate R	SELF ($n = 215$) $\tau = 1$ Tasks soon	$\tau = 2$ Tasks soon	t-test	OTHER ($n = 214$) $\tau = 1$ Tasks soon	$\tau = 2$ Tasks soon	t-test
0.5	38.89	37.31	$p = 0.003$	36.93	36.39	$p = 0.395$
	(8.48)	(8.92)		(10.57)	(9.64)	
0.75	34.68	33.19	$p = 0.010$	32.92	33.13	$p = 0.716$
	(9.85)	(9.95)		(11.19)	(10.24)	
1	25.50	24.47	$p = 0.076$	25.13	24.86	$p = 0.509$
	(6.95)	(6.81)		(5.71)	(5.57)	
1.25	17.49	16.40	$p = 0.043$	17.38	17.02	$p = 0.536$
	(10.44)	(10.42)		(11.14)	(10.81)	
1.5	14.47	13.75	$p = 0.133$	14.86	14.75	$p = 0.848$
	(10.37)	(10.07)		(11.20)	(10.53)	
2	12.08	11.28	$p = 0.103$	12.87	12.13	$p = 0.203$
	(10.54)	(9.61)		(11.69)	(10.31)	
Overall	23.85	22.73	$p = 0.001$	23.35	23.05	$p = 0.287$
	(13.88)	(13.55)		(13.85)	(13.34)	

Note: The table denotes the number of tasks allocated to the sooner date, separately for block SELF (left panel) and block OTHER (right panel). For each rate R, the p-value reported stems from a t-test with standard errors clustered at the individual level.

Table 3: Intrapersonal decisions: Aggregate behavior by task rate

demand: as R increases, it becomes "cheaper" to allocate more tasks to the later date.[16] More interestingly, we observe a systematic downward shift in the number of tasks allocated to the sooner date in week 2 compared to week 1, in line with Prediction 1. On average, participants allocate 1.12 fewer tasks to the sooner work date when it is the present (-4.7%, 23.85 compared to 22.73; t-test, $p = 0.001$), indicating a significant and economically meaningful present bias in own consumption. These results are corroborated by the left panel of Table 3, showing the number of tasks allocated to the sooner work date separately for each R. The table further reveals that there is very little evidence for long-term discounting. This is most clearly seen for $R = 1$. In this case, participants in week 1 allocate on average 25.50 tasks (or 51.0%) to the sooner date, thus splitting the workload almost evenly across weeks.

To estimate the time-preference parameters from these choices structurally, we follow previous literature and parameterize the utility function as $u_s(s_{t,\tau}) = s_{t,\tau}^{\gamma_s}$, where γ_s determines the agent's preference for smoothing consumption over the two periods. We adopt two different estimation

[16] As shown in Table A1 in Appendix A, 95 percent of choices are monotonically decreasing in R and 68 percent of participants have no monotonicity violation in their effort choices. These numbers are comparable to the ones reported in Augenblick et al. (2015) who find 95 percent of effort choices to be monotonically decreasing in R. In addition, 22% of the choices are corner solutions, which is somewhat lower than the 31% observed in Augenblick et al. (2015) and much lower than the numbers typically observed in monetary discounting (e.g., 70% in Andreoni and Sprenger, 2012 and 86% in Augenblick et al., 2015).

	Self ($j = s$)			Other ($j = o$)		
	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$	(4) FOC $\omega = 10$	(5) CFS $\omega = 10$	(6) CFS $\omega = 0$
γ_j	1.954	2.139	1.719	2.093	2.357	1.867
	(0.111)	(0.163)	(0.115)	(0.154)	(0.237)	(0.166)
δ_j	1.026	1.030	1.010	0.972	0.970	0.952
	(0.025)	(0.027)	(0.025)	(0.019)	(0.022)	(0.021)
β_j	0.916	0.909	0.913	0.969	0.969	0.972
	(0.024)	(0.028)	(0.027)	(0.024)	(0.027)	(0.025)
Observations	1380	1380	1380	1392	1392	1392
Participants	215	215	215	214	214	214
$H_0(\hat{\delta}_j = 1)$	$p = 0.284$	$p = 0.270$	$p = 0.687$	$p = 0.129$	$p = 0.175$	$p = 0.020$
$H_0(\hat{\beta}_j = 1)$	$p < 0.001$	$p = 0.001$	$p = 0.001$	$p = 0.186$	$p = 0.250$	$p = 0.266$

Note: The table reports the parameter estimates for the choices made in blocks Self (left panel) and Other (right panel), respectively. Columns (1) and (4) use the log-linearized first order condition, while the other columns use the closed form solution for the number of tasks allocated to the sooner date. Standard errors are clustered at the individual level and calculated via the delta method.

Table 4: Parameter estimates for blocks Self and Other

approaches. In the first approach ("FOC"), we follow Augenblick et al. (2015) and Andreoni and Sprenger (2012) and log-linearize the first-order condition in (2) (see Section 3). In the second approach, we instead use the closed form solution for effort allocated to the sooner date ("CFS"). In both cases, we use two-limit Tobit maximum-likelihood to estimate the parameters. We also take into account participants' "background consumption", ω, which is relevant in our setting since participants in each period have to complete the minimum work requirement of 10 tasks in addition to their allocated tasks, which, in turn, might affect their optimal allocation. While in our first approach we set $\omega = 10$, which avoids the natural logarithm to be undefined for corner solutions, in our second approach we can set $\omega = 10$ and $\omega = 0$. The second approach thus helps us to investigate the robustness of our estimates with respect to different estimation techniques as well as with regard to the inclusion of the minimum work requirement into the estimation. Further details on the two estimation approaches and how we recover the discounting parameters from the regression coefficients can be found in Appendix C.

The results of our estimations are shown in the left panel of Table 4. In line with our reduced-form results from above, the results reveal strong and significant evidence for present bias in own consumption. The estimates for β_s vary between 0.909 and 0.916 across specifications, and are always significantly lower than one (all $p < 0.002$). We find no evidence for long-term discounting; the weekly discount rate δ_s varies between 1.010 and 1.030, but it is never significantly different from 1 (all $p > 0.269$). We can thus state our first result:

Result 1. *In line with Prediction 1, we find strong evidence for present bias in own consumption as manifested in a significant decrease in tasks allocated to the sooner date when deciding in advance (week 1) rather than in the present (week 2).*

Given the similarity of our block SELF design to the one used in Augenblick et al. (2015), it is sensible to compare the findings of both studies, in particular as there are a few notable differences across the two studies. First of all, while in Augenblick et al. (2015) initial allocations were made in the lab and subsequent allocations were made online, all our allocations decisions took place in the the same lab at exactly the same time of the same day of the week. Furthermore, the encryption task we use is slightly different from theirs (they additionally use Tetris as a second, arguably more fun, real-effort task). Despite these differences, the results from both studies are remarkably similar. Augenblick et al. (2015) estimate a β of 0.888, compared to our β_s estimate of 0.916 (see model (1) in Table 4, which is the approach that Augenblick et al. (2015) use for their structural estimation). Our findings are further in line with the results of a recent meta-analysis by Imai et al. (2019), who report mean present bias estimates of studies using convex time budgets in the effort domain of 0.88 - 0.91. This suggests that present bias in own non-monetary consumption is a robust finding across different subject pools, experimental procedures, and tasks.

4.2 Deciding on behalf of others

We now turn to the analysis of choices made on behalf of someone else in block OTHER. One concern when interpreting decisions made for others is that decision-makers might not take these decisions seriously as they do not have a direct (pecuniary) impact on themselves. As we show in Table A1 in Appendix A, such concerns are not warranted. We find that when choosing for others, 92 percent of choices are monotonically decreasing in R, and 64 percent of participants have no monotonicity violation. These numbers are very similar to the ones observed in block SELF, indicating that any differences between the two blocks are not caused by differences in decision quality.

The results from block OTHER are summarized in the right panel of Figure 3 and Table 3. Compared to the choices in block SELF, a different picture emerges. In particular, the differences between initial allocations in week 1 and subsequent allocations in week 2 are now much less pronounced. On average, participants allocate 0.30 fewer tasks to the sooner work date when consequences are immediate. This corresponds to a decrease of only 1.3%, which is not statistically significant (week 1: 23.35, week 2: 23.05; t-test, $p = 0.287$).

Using the same two approaches as above, we corroborate the reduced-form findings by structurally estimating time preference parameters. As shown in the right panel of Table 4, we find little evidence for present bias in others' consumption. The estimates for β_o range between 0.969 and 0.972, which are slightly below but not significantly different from one (all $p > 0.185$). For long-run discounting, we obtain δ_o estimates between 0.952 and 0.982, which are also somewhat below but, with one exception (column (6)), not significantly different from one ($p > 0.128$).[17]

Result 2. *In line with Prediction 2, we find no evidence for present bias in others' consumption as manifested by a similar amount of tasks allocated to the sooner date when deciding in advance (week 1) rather than in the present (week 2).*

To directly compare whether the degree of present bias differs between choices made for oneself and on behalf of others, we structurally estimate all four discounting parameters jointly. In the estimation, we constrain the curvature of the cost of effort function to be the same for own and others' consumption ($\gamma = \gamma_s = \gamma_o$). The results from this estimation are shown in Table A2 in Appendix A. The picture that emerges from this analysis is consistent with the findings above. Specifically, we obtain β_s estimates between 0.905 and 0.911 and β_o estimates between 0.972 and 0.977. We can reject the hypothesis that β_s and β_o are the same at the 5% level (all $p < 0.034$).[18]

4.3 Individual-level analysis

To shed some light on the underlying heterogeneity of these results, in the following, we estimate time preference parameters at the individual level. To do this, we use the approach based on the closed-form solution, and concentrate on the case with $\omega = 10$. Furthermore, following the aggregate estimation presented in Table A2 in Appendix A, we jointly estimate the discounting parameters for both blocks, thereby restricting $\gamma = \gamma_s = \gamma_o$. This reduces the number of parameters to be estimated from a given number of observations, which increases the precision of the estimation. We obtain reasonable individual-level estimates for more than 90% of the participants (189 out of 208).[19]

Figure 4 plots the distributions of the individual estimates for β_s (left panel) and β_o (right

[17]While δ estimates of ≈ 0.96 could be considered economically meaningful, it should be noted that for the intrapersonal decisions, δ is not identified through experimental variation. One should hence be cautious when interpreting these estimates.

[18]This result is confirmed when calculating the difference-in-difference in allocation decisions across weeks between blocks SELF and OTHER as a simple reduced-form measure. Using data from the same set of participants as in our structural estimation, we find this measure to amount to -0.85 tasks, which is statistically significantly different from zero (t-test, $p = 0.028$).

[19]The behavior of ten participants is fully consistent with utility maximization, but we can only identify bounds on β_s and β_o, i.e., whether they are (weakly) above or below one, because they have insufficient variation across weeks. For the remaining participants, following Augenblick and Rabin (2019), we use Grubb's outlier test with a confidence level of 99.99%, which is rejected for nine participants. See Appendix D for a more detailed description of our procedures and the full list of individual estimates (excluded cases are highlighted).

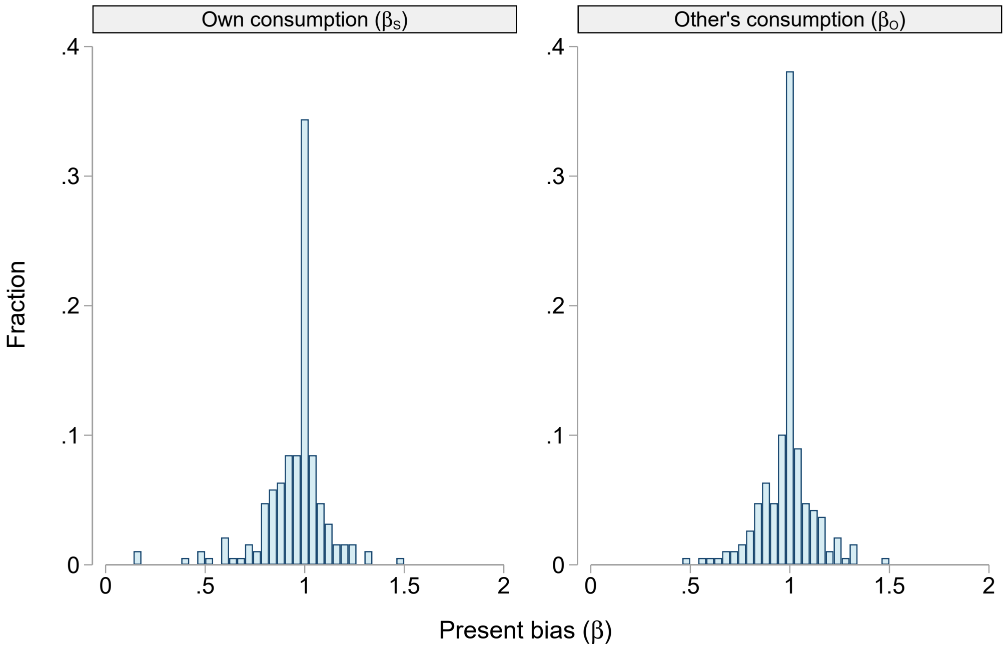

Figure 4: Distribution of present bias estimates for own and others' consumption

panel). In line with our aggregate results from above, we find that the mean β_s is significantly lower than the mean β_o (0.950 vs. 0.987; paired t-test, $p = 0.005$), indicating systematic differences in time-inconsistency when own or others' consumption is at stake. Furthermore, while in both cases we observe a big spike around 1 indicating (close to) dynamically consistent discounting behavior, in both cases there is also pronounced heterogeneity across individuals.

To highlight this heterogeneity, we can use the individual-level estimates to classify participants into different "discounting types", as done in some previous studies (see e.g., Ashraf et al., 2006; Meier and Sprenger, 2010). We follow Augenblick et al. (2015) and classify a participant as "present-biased" if their estimated $\beta < 0.99$, as "future-biased" if $\beta > 1.01$, and as "dynamically consistent" otherwise. The distribution of these types are shown in Table 5. It reveals that more participants are classified as present-biased when own rather than others' consumption is at stake. At the same time, we find a smaller fraction of participants being classified as dynamically consistent and future-biased when deciding for oneself rather than for others. The share of those with a future bias in own consumption is thereby similar to the one reported in Augenblick et al. (2015). Finally, we find some moderate but highly significant positive correlation between β_s and β_o ($\rho = 0.289, p < 0.001$), an observation we will come back to in Section 7.

	N	Mean (s.d.)	Proportion present biased ($\beta < 0.99$)	Proportion dynamically consistent ($0.99 \leq \beta \leq 1.01$)	Proportion future biased ($\beta > 1.01$)
β_s	189	0.950 (0.166)	0.471	0.265	0.264
β_o	189	0.987 (0.132)	0.392	0.291	0.317

Table 5: Summary statistics of individual-level estimates for β_s and β_o in the intrapersonal choices.

5 Effort Allocation in Interpersonal Choices

We now turn to the results from the interpersonal choices, in which decision makers have to allocate effort between themselves and another person, i.e., those decisions that can be considered generalized versions of dictator games. Before analyzing the effects of timing on generosity, however, we briefly relate the overall level of generosity displayed by our participants to the existing evidence on altruistic behavior. This is interesting, since in contrast to most previous literature, we study altruistic behavior using effort rather than money (see Noussair and Stoop, 2015; Danilov and Vogelsang, 2016, for notable exceptions). In a meta study of 131 standard monetary dictator games, Engel (2011, p. 607) reports that around 36% of the people give nothing, and that among those who give a positive amount to the receiver, the average amount given is 43% of the pie. The most comparable benchmark from our data is the case where consequences for both the dictator and the recipient are immediate, that is week 2 in SoonSoon, and $R = 1$. Using our whole sample, i.e., temporarily including those we drop for the subsequent analysis, we find that 32% of our participants allocate zero tasks to themselves for this decision. Among those who are not completely selfish, participants allocate on average around 36% of the tasks to themselves. Hence, we find that while the fraction of completely selfish individuals is not too different across domains, conditional on giving, generosity in effort is somewhat weaker than in the monetary domain.

5.1 Aggregate Analysis

Our results are summarized by Figure 5, illustrating the mean number of tasks allocated to oneself, separately for each task rate R. The upper two panels depict the allocation decisions for the symmetric dictator games, while the lower two panels display the decisions from the asymmetric dictator games. A first visual impression from Figure 5 is that all lines are downward sloping, indicating that the “cheaper” it becomes to allocate tasks to the other person, the fewer tasks participants allocate to themselves. Overall, we find that 92 percent of choices are monotonically decreasing in R, suggesting that participants understood our allocation environment. Furthermore,

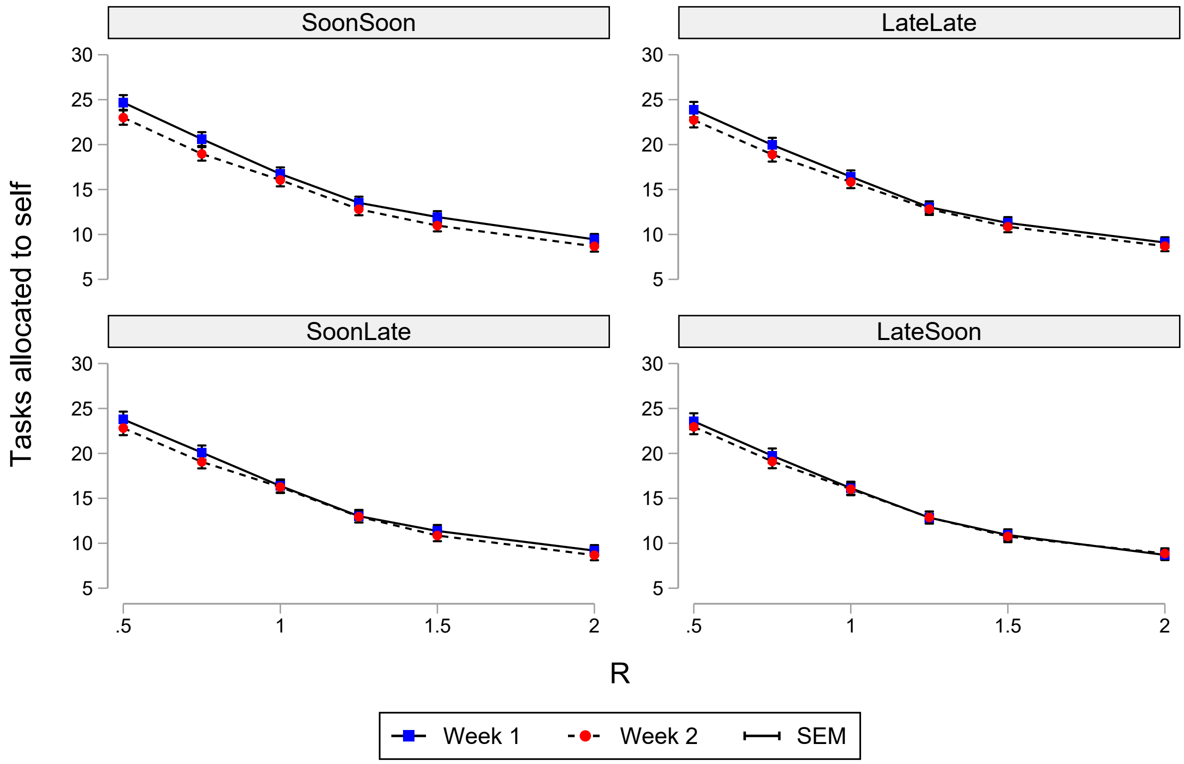

Figure 5: Effort allocations in dictator games ($n = 165$)

deviations from monotonicity are typically very small with a median required allocation change of one task to restore monotonicity (see Table A1 in Appendix A for additional information and a full breakdown by block).

More importantly, when comparing initial allocations made in week 1 and subsequent allocations made in week 2 within each block, several interesting patterns emerge. For block SOONSOON, in week 2 we observe a significant downward shift in work allocated to oneself. Aggregated over all rates, the average number of tasks allocated to oneself decreases by 6.7% (from 16.16 to 15.08; t-test, $p = 0.020$), indicating that generosity decreases when consequences are immediate (see Table A3 in Appendix A for an overview of allocations by exchange rate R). Recall from Section 3, that this suggests $\beta_s \delta_s < \beta_o \delta_o$.

To investigate whether the decrease in generosity is, as hypothesized, indeed driven by a relative present bias ($\beta_s < \beta_o$), we can compare these results with those from LATELATE. In particular, as explained in Section 3, we can use the difference-in-difference between initial and subsequent allocation decisions between both blocks to isolate the effect of relative present bias. For LATELATE, we find a (weakly significant) decrease in generosity by 4.1% across the two weeks (week 1: 15.62, week 2: 14.98; t-test, $p = 0.068$). Consequently, when combining the effects between the two blocks, the difference-in-difference amounts to 2.6% or 0.44 tasks, which is not statistically significant (t-test, $p = 0.247$). We thus obtain the following result:

Result 3. *Contrary to Prediction 3, we find no evidence for present-biased generosity; the change in tasks allocated to oneself between week 1 and week 2 is not significantly different between* SOONSOON *and* LATELATE.

We now turn to the results of the asymmetric dictator games as summarized by the lower two panels of Figure 5 (see also Table A4 in Appendix A). For SOONLATE, we find that the number of tasks allocated to oneself decreases by 3.4% (from 15.64 in week 1 to 15.11 in week 2), which is not significantly different from zero (t-test, $p = 0.224$). For block LATESOON, we find an average decrease of 1.4% (from 15.32 in week 1 to 15.10 in week 2), which also does not reach statistical significance (t-test, $p = 0.608$). As explained in Section 3, the results from these blocks do not have a straightforward interpretation on their own, but we can use them, in combination with the allocation decisions in the symmetric dictator games, to draw inferences about β_s and β_o.

Such an analysis, however, does not reveal a fully consistent picture. For present bias in others' consumption both possible comparisons point to $\beta_o \approx 1$: the difference-in-difference between SOONSOON and SOONLATE (0.54 tasks; t-test, $p = 0.239$) as well as between LATELATE and LATESOON (0.42 tasks; t-test, $p = 0.184$) is rather small and not statistically significant. For present bias in own consumption, however, we obtain diverging results: while the comparison between LATESOON and SOONSOON suggests that $\beta_s < 1$, due to a significant difference-in-difference of 0.85 tasks (t-test, $p = 0.044$), the difference-in-difference between SOONLATE and LATELATE favors the interpretation that $\beta_s \approx 1$ (0.11 tasks; t-test, $p = 0.733$).

To provide a more rigorous analysis of time preferences in our dictator games, we can use the allocation decisions from all blocks combined to estimate time preference parameters structurally. Our estimation approach thereby closely follows the ones from the intrapersonal choices (see Appendix C for a detailed description). We present the results from these estimations in Table A5 in Appendix A. The estimates are very much in line with the reduced-form findings presented above. In particular, the results suggests dynamic consistency in own as well as others' consumption. Especially the estimates for δ_s, δ_o, and also β_o are revealed to be very close to and not significantly different from one. The estimates for β_s are somewhat below (ranging between 0.967 and 0.974) but not significantly different from one (all $p > 0.132$). Furthermore, in contrast to the results from the intrapersonal choices, we cannot reject the hypothesis that $\beta_s = \beta_o$ (all $p > 0.157$).

Overall, we thus conclude that the timing of actions and consequences has very little systematic effects on generosity in our context. We can further conclude that the time preference parameters in our social context of the dictator game are substantially different from those obtained in the intrapersonal decisions, an observation we will come back to in our discussion.

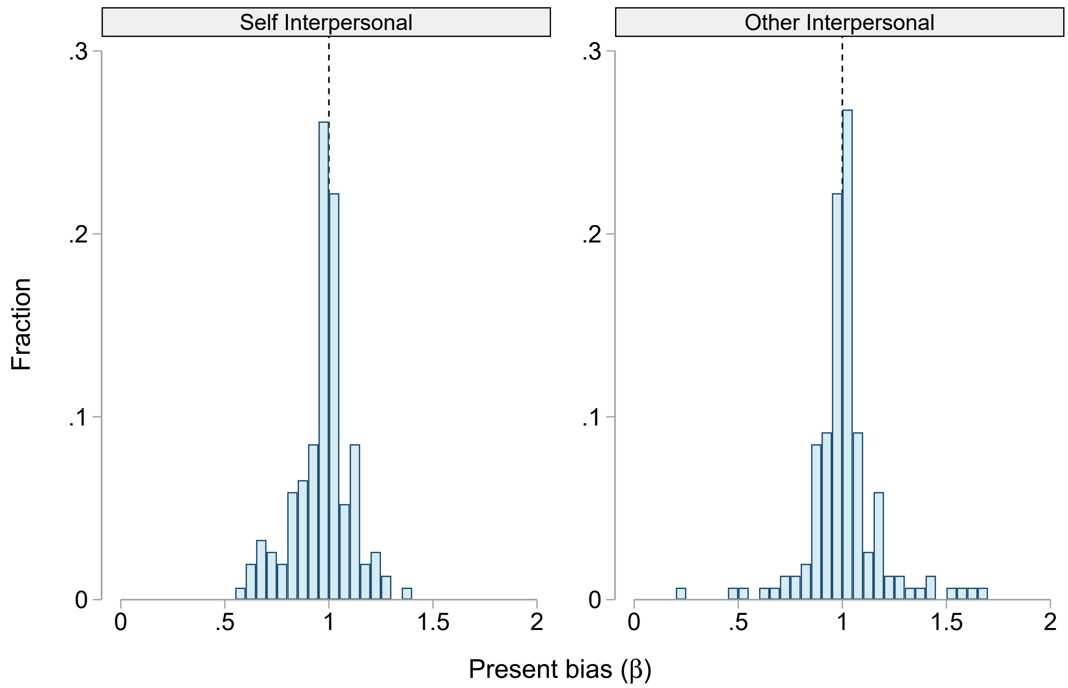

Figure 6: Distribution of individual estimates for present bias in own and others' consumption from interpersonal choices.

5.2 Individual-level analysis

To investigate whether the absence of any time inconsistency in the dictator games is an artifact of aggregating decisions over all individuals, in the following, we structurally estimate time preference parameters at the individual level. We obtain reasonable estimates for 153 out of 165 participants (see Appendix D for further details). The results are shown in Figure 6, depicting the distribution of individual estimates for β_s (left panel) and β_o (right panel). The figure reveals that there is pronounced heterogeneity across individuals. While many individuals in fact display present bias as indicated by estimates smaller than one, an almost equally large fraction of individuals display future bias, as indicated by estimates larger than one. Most estimates lie between 0.75 and 1.25, which seems a quite plausible range. Table 6 provides summary statistics of the different estimates. We obtain a mean β_s of 0.972 and a mean β_o of 1.008, which are both very close to aggregate estimates of these parameters.

An individual-level comparison of β_s and β_o suggests the presence of two distinct types of individuals: (i) those who become more selfish when consequences are immediate, as manifested by $\beta_s < \beta_o$ and (ii) those who become more generous, i.e., for which $\beta_s > \beta_o$. Our results reveal that both types are prevalent and exist in roughly equal proportions. For 55.6% of individuals we

	N	Mean (s.d.)	Proportion present biased ($\beta < 0.99$)	Proportion dynamically consistent ($0.99 \leq \beta \leq 1.01$)	Proportion future biased ($\beta > 1.01$)
β_s	153	0.972 (0.137)	0.458	0.196	0.346
β_o	153	1.008 (0.175)	0.418	0.190	0.392

Table 6: Summary statistics of individual-level estimates for β_s and β_o in the interpersonal choices.

estimate $\beta_s < \beta_o$, while for 41.8% we find $\beta_s > \beta_o$. These numbers indicate that the lack of any time inconsistency at the aggregate level is, at least in part, the result of pronounced but symmetric heterogeneity.

6 Discussion

Our results so far have revealed that there is a systematic difference in present bias in own compared to others' consumption in intrapersonal choices, i.e., when deciding for oneself or on behalf of another person, but not when one's own and others' consumption is at stake at the same time (interpersonal choices). As a consequence, we find that generosity does not systematically vary with the timing of consequences for self and others. The general picture that emerges from this analysis is that there are fundamental differences in the way people discount future consumption depending on whether allocation decisions involve interpersonal trade-offs or not. In the following, we discuss potential reasons for these findings, and provide some additional analysis that will help interpreting our results.

We start with providing additional evidence demonstrating that the way people discount in individual and social contexts is indeed very different. This is shown in Figure 7, depicting the relationship of our individual-level estimates across the interpersonal and intrapersonal decision situations. The left panel shows this relationship for present bias in own consumption, and the right panel shows the same relationship for present bias in others' consumption. In both cases, we only find a weakly positive but insignificant correlation across the two contexts (β_s: $\rho = 0.12$, $p = 0.144$; β_o: $\rho = 0.05$, $p = 0.566$). Together with our aggregate findings from above, these results indicate that present bias in own consumption is not a universal behavioral trait, but one that is context-specific, as also suggested by some previous studies (see e.g., Chabris et al., 2008).

One interpretation of our data is that individuals become generally less sensitive towards time when moving from choices that only affect oneself to choices that affect both oneself and others, i.e., when moving from an individual to a social context. This interpretation bears some resemblance with evidence from a recent study by Cubitt et al. (2018), who find that time matters less

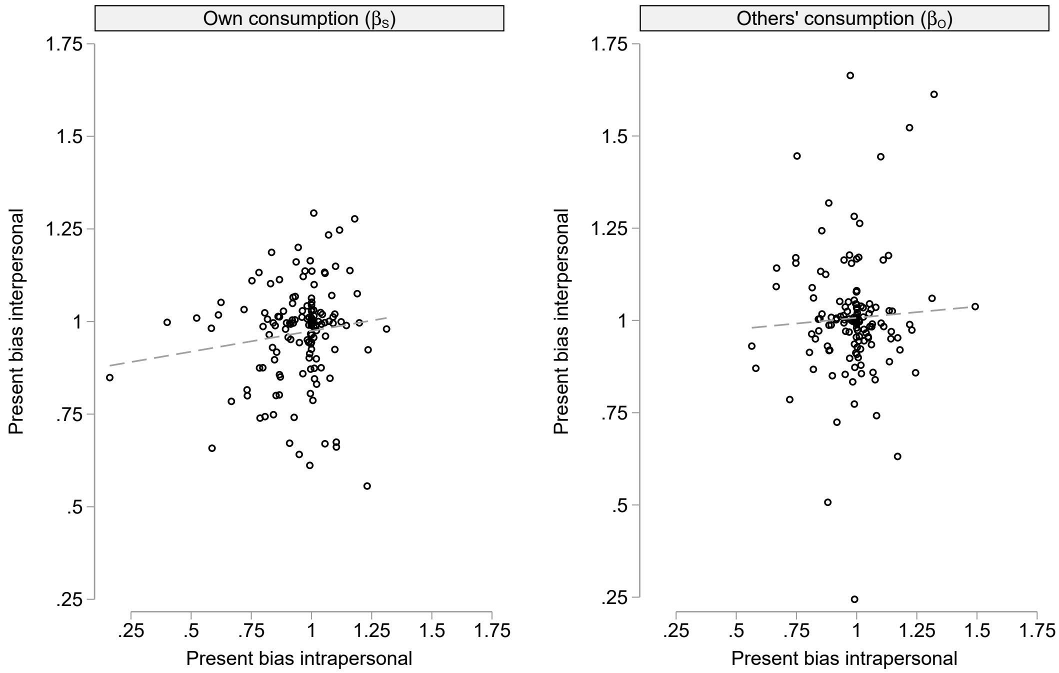

Figure 7: Correlation of present bias in own and others' consumption across intrapersonal and interpersonal choices. The dashed lines indicate linear fits from OLS regressions

(i.e., individuals are more patient) when intertemporal decisions involve comparing options of different types (e.g., apples now versus oranges later), compared to intertemporal decisions that only involve options of the same type (e.g., apples now versus apples later). One potential explanation for this finding is based on insights from multi-attribute choice (see e.g., Houston and Sherman, 1995), which assumes that the decision weight put on a particular attribute decreases the more different attributes there are. According to this, when making intertemporal decisions involving different types of objects, the attribute time receives less weight compared to a case that involves only one type of object, because there are many attributes that differ between objects, which all enter into the decision-making process. Applied to our context, this logic predicts that time should matter less in the interpersonal than in the intrapersonal decisions because while in the latter time is the only attribute that differs across options, in the former decisions involve both an intertemporal as well as an interpersonal trade-off, thus decreasing the weight of each of those attributes. Similar predictions can be derived from salience or focusing theories (Bordalo et al., 2012; Kőszegi and Szeidl, 2013), e.g., when assuming that adding a trade-off between own and others' consumption to an intertemporal choice problem makes the factor time less salient.

Another possible interpretation of our findings is that when decisions have consequences that affect both oneself and others, discounting of own and others' consumption become more similar to each other. Evidence for the possibility of such an effect is provided by Andersson et al. (2016) who show that when decisions have identical consequences for oneself and another person, the degree of loss aversion is smaller than when deciding only for oneself, but similar to when deciding on behalf of others. In a similar vein, Rodriguez-Lara and Ponti (2017) find decision makers try to accommodate the preferences of others by shifting decisions away from the own preferred option towards the preferred option of the partner. Yet, while these studies have focused on situations in which payoff consequences for both players were fully aligned, here we focus on choices in which there is a trade-off between own and others' consumption. Hence, given the different nature of our setup, it is unclear whether the effects reported in these former studies translate into the context considered here.

Finally, it might be that the effects of delay on prosocial behavior are simply heterogeneous and multi-faceted. As revealed by our individual-level analysis above (see Section 5.2), in the context of our dictator games we find pronounced heterogeneity in time preferences with some individuals exhibiting strong degrees of present bias, and some others displaying substantial degrees of future bias. That is, while some individuals become less generous when consequences are immediate rather than delayed, other individuals become more generous. Previous literature has proposed the existence of such distinct types, by assuming that either giving is tempting (as in Dreber et al., 2016) or that being selfish is tempting (as in Saito, 2015). The fact that in our sample both of these types occur with similar frequency, suggests that the absence of any strong effect at the aggregate level can, at least in part, be explained by pronounced but symmetric individual heterogeneity.

The prevalence of different discounting types in the social domain can further help explain why previous work on this topic has produced rather mixed results; while Andreoni and Serra-Garcia (2019) and Breman (2011) find that delaying consequences increases prosocial behavior, Kovarik (2009) and Dreber et al. (2016) find evidence for an effect in the opposite direction. Despite some differences between our work and these studies, e.g., with regard to whether the beneficiary of altruistic choices is a charity or another person, or whether allocations concern monetary payments or workload, the fact that there is pronounced heterogeneity across individuals can account for the conflicting evidence, as different random draws from the overall population may lead to very different outcomes, especially if sample sizes are small. In addition, it is also possible that there exist a number of unpublished studies which did not find a significant effect in either direction for setups similar to ours. The reason is that studies with significant results are more likely to be published (the so called "publication bias", see e.g., Andrews and Kasy, 2019), exaggerating the true effect of delay on generosity.

7 Conclusion

In this paper we provide a systematic analysis of time discounting across contexts with and without interpersonal trade-offs. We first show that time-inconsistent behavior in form of present bias is prevalent in decision contexts that only involve own consumption, but not when deciding on behalf of others. Second, in contrast to what a theory of dynamic altruistic behavior would predict based on these asymmetries, we find no robust evidence for time inconsistency in the social domain. Instead, we show that generosity – here measured in form of being willing to work on a tedious task instead of delegating it to someone else – is largely unaffected by the timing of consequences.

Our first set of results from the intrapersonal choices contribute to a literature which investigates decision making for others (see Füllbrunn et al., 2020, for a recent overview). With regard to intertemporal choice, previous literature has, with the exception of Albrecht et al. (2011), focused on patience rather than present bias, and the evidence from these studies is rather mixed (Shapiro, 2010; Howard, 2013; Rodriguez-Lara and Ponti, 2017; Rong et al., 2019; de Oliveira and Jacobson, 2021). Here, we provide robust evidence for an asymmetry in present bias between self and others. Our finding that only the choices over own consumption reveal a present bias allows for (at least) two different interpretations. Either, when choosing for another person agents behave as if they choose what they believe the person would have chosen for herself, but mistakenly believe that the other person is time-consistent in their choices. Alternatively, decision makers hold correct beliefs about the present bias of others, but decide to implement time-consistent allocations because they believe that this is the intertemporal allocation of consumption which, from a normative perspective, *should* be implemented for the other agent.

Recent work by Fedyk (2017) shows that while agents are unable to foresee their own present bias, they are relatively accurate in predicting the present bias of others. Combining this insight with our findings, this suggests that choices made on behalf of others reflect paternalism rather than simple benevolence, and that when not affected directly, agents treat present-biased choices as temptation-driven and in need of correction. This is in line with neuroeconomic evidence (McClure et al., 2004; Albrecht et al., 2011), which links present bias to the more affective and more impulsive system compared to a more deliberative and reasoned system which may play a more central role when discounting others' consumption. Our results further provide support for what Ambuehl et al. (2019) call projective paternalism, i.e., the idea that when choosing on behalf of others, decision makers act as if they assume that their own preferences are relevant for others. Projective paternalism should be manifested in a positive correlation between present bias in own and others' consumption, which is what we find. Yet, more research is needed to gain a deeper understanding of the underlying psychological mechanisms when discounting own and others' consumption. Evidence from this research could provide important insights into the ongoing discussion about how to incorporate present bias into welfare analysis (see, e.g., Bernheim and Rangel, 2009).

Our results from the dictator games are surprising for at least two reasons. First, previous literature on intertemporal choice has provided ample evidence for the prevalence of time-inconsistent behavior across many different domains, both in the laboratory and the field (see Frederick et al., 2002; Ericson and Laibson, 2019, for overviews). Our results show that such time inconsistency does not extend to social contexts, i.e., to those decisions where there exist intertemporal trade-offs between own and others' consumption. Second, with regard to the research on the determinants of social preferences, prosocial behavior has been shown to be quite malleable and context-dependent. For instance, previous studies have shown that people are often less generous when they can avoid information about their actions (Dana et al., 2006; 2007), when they can avoid being asked to give (Andreoni and Rao, 2011; DellaVigna et al., 2012; Andreoni et al., 2017), when they can diffuse being pivotal (Falk et al., 2020), or when transfers entail risk (Brock et al., 2013; Exley, 2015). In contrast to these findings, our results reveal that prosociality is relatively unaffected by the timing of consequences, suggesting little malleability in this dimension.

Our findings pose some challenges for economic theories concerned with the question of how to incorporate other-regarding preferences into a context in which consequences play out over time. Our theoretical approach to allow for different discounting parameters for own and others' consumption is based on the conceptually plausible idea that these discounting parameters correspond to those from individual decisions contexts. However, we find no evidence for such a stability of preferences across contexts, suggesting that there are likely other important determinants of behavior in environments with both an intertemporal as well as an interpersonal dimension. As we show in Appendix B, alternative models based on warm glow or image concerns also cannot account for our observed patterns. Designing further studies aimed at uncovering some of these factors is, in our opinion, a fruitful avenue for future research.

Finally, our results suggest that policy interventions trying to leverage the timing of decisions to promote generosity (e.g., in form of support for redistribution policies or willingness to donate) have to take a very specific form in order to be successful. Simply shifting the burden farther into the future, e.g., by implementing donations with some delay, is unlikely to lead to strong positive effects. Yet, on a positive note, our finding that "time matters less" in social contexts suggests that there might in fact be widespread support for policies that reduce consumption today but benefit society in the future, simply because people focus on the generous act itself and not on how far in the future the positive impact realizes. Moreover, the fact that many individuals do react to the timing of consequences in social contexts, albeit in a rather heterogeneous fashion, opens the door for subgroup-specific interventions. The challenge, however, is to identify and target such individuals. Future work aimed at identifying these different types should thus prove extremely valuable for policymakers or NGOs.

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Is Generosity Time-Inconsistent? Present Bias across Individual and Social Contexts

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Appendix for Online Publication

Contents

A Additional Tables and Figures

	% non-monotonic choices	% blocks with monotonicity violations	% fully consistent subjects	Median (Mean) degree of monotonicity violation	
				if > 0	Total
SELF	5.5	20.2	67.9	2 (3.2)	0 (0.7)
OTHER	7.5	24.3	63.6	3 (12.3)	0 (3.0)
SOONSOON	7.6	27.6	60.0	2 (3.2)	0 (0.9)
LATELATE	7.1	25.5	62.4	2 (3.1)	0 (0.8)
SOONLATE	8.1	28.2	57.6	2 (3.3)	0 (0.9)
LATESOON	8.6	29.1	58.2	2 (3.7)	0 (1.1)

Note: The degree of monotonicity violation is measured as the absolute number of tasks that need to be reallocated to restore monotonicity within a block.

Table A1: Monotonicity violations

	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$
γ	1.992	2.196	1.756
	(0.121)	(0.179)	(0.127)
δ_s	1.001	1.005	0.989
	(0.024)	(0.026)	(0.024)
β_s	0.911	0.905	0.909
	(0.026)	(0.030)	(0.029)
δ_o	0.980	0.984	0.096
	(0.018)	(0.021)	(0.019)
β_o	0.972	0.976	0.977
	(0.022)	(0.025)	(0.023)
Observations	4992	4992	4992
Participants	208	208	208
$H_0(\hat{\beta}_s = 1)$	$p = 0.001$	$p = 0.001$	$p = 0.001$
$H_0(\hat{\beta}_o = 1)$	$p = 0.206$	$p = 0.324$	$p = 0.315$
$H_0(\hat{\beta}_s = \hat{\beta}_o)$	$p = 0.034$	$p = 0.032$	$p = 0.030$

Note: The table reports the parameter estimates for the choices made in blocks Self and Other under the restriction that $\gamma_s = \gamma_o = \gamma$. Column (1) uses the log-linearized first order condition, while the columns (2) and (3) use the closed form solution for the number of tasks allocated to the sooner date. The estimation uses the data from those 208 subjects who have sufficient variation in block Self and block Other. Standard errors are clustered at the individual level and calculated via the delta method.

Table A2: Parameter estimates for blocks Self and Other combined

Rate R	SoonSoon $\tau = 1$ Task self	SoonSoon $\tau = 2$ Task self	SoonSoon t-test	LateLate $\tau = 1$ Task self	LateLate $\tau = 2$ Task self	LateLate t-test	Diff-in-diff [t-test]
0.5	24.68	23.00	$p = 0.018$	23.89	22.73	$p = 0.031$	0.52
	(10.53)	(10.24)		(11.03)	(10.59)		[$p = 0.400$]
0.75	20.62	18.96	$p = 0.013$	19.96	18.89	$p = 0.032$	0.58
	(9.83)	(9.67)		(10.11)	(10.06)		[$p = 0.361$]
1	16.74	16.06	$p = 0.241$	16.42	15.84	$p = 0.207$	0.10
	(9.25)	(9.09)		(9.02)	(8.87)		[$p = 0.844$]
1.25	13.52	12.81	$p = 0.129$	13.02	12.82	$p = 0.595$	0.51
	(8.74)	(8.61)		(8.55)	(8.34)		[$p = 0.182$]
1.5	11.94	10.99	$p = 0.054$	11.28	10.87	$p = 0.262$	0.53
	(8.41)	(8.26)		(8.24)	(7.97)		[$p = 0.191$]
2	9.45	8.68	$p = 0.127$	9.10	8.70	$p = 0.228$	0.37
	(7.66)	(7.57)		(7.53)	(7.21)		[$p = 0.402$]
Overall	16.16	15.08	$p = 0.020$	15.62	14.98	$p = 0.068$	0.44
	(10.48)	(10.17)		(10.46)	(10.10)		[$p = 0.247$]

Note: The table denotes the number of tasks allocated to oneself, separately for block SoonSoon (left panel) and block LateLate (right panel). The p-values reported stem from t-tests with standard errors clustered at the individual level. The last column shows the difference-in-difference across week 1 and week 2 allocations between block SoonSoon and LateLate.

Table A3: Symmetric dictator games: Aggregate behavior by task rate ($n = 165$)

Rate R	SoonLate $\tau = 1$ Task self	SoonLate $\tau = 2$ Task self	SoonLate t-test	LateSoon $\tau = 1$ Task self	LateSoon $\tau = 2$ Task self	LateSoon t-test
0.5	23.78	22.82	$p = 0.131$	23.57	22.95	$p = 0.324$
	(11.10)	(10.14)		(11.47)	(10.50)	
0.75	20.10	19.06	$p = 0.075$	19.74	19.11	$p = 0.283$
	(10.04)	(9.38)		(10.43)	(9.73)	
1	16.38	16.26	$p = 0.831$	16.13	16.02	$p = 0.844$
	(9.22)	(8.55)		(9.17)	(8.69)	
1.25	13.02	12.96	$p = 0.911$	12.85	12.87	$p = 0.959$
	(8.88)	(8.44)		(8.49)	(8.60)	
1.5	11.37	10.86	$p = 0.256$	10.92	10.76	$p = 0.720$
	(8.54)	(8.13)		(8.14)	(8.25)	
2	9.19	8.68	$p = 0.234$	8.70	8.86	$p = 0.711$
	(7.82)	(7.27)		(7.36)	(7.45)	
Overall	15.64	15.11	$p = 0.224$	15.32	15.10	$p = 0.608$
	(10.59)	(9.94)		(10.58)	(10.14)	

Note: The table denotes the number of tasks allocated to oneself, separately for block SoonLate (left panel) and block LateSoon (right panel). The p-values reported stem from t-tests with standard errors clustered at the individual level. Contrary to the table for the symmetric dictator games, here we do not report the difference-in-difference results because, as explained in Section 3 in the main text, they do not have a straightforward interpretation.

Table A4: Asymmetric dictator games: Aggregate behavior by task rate ($n = 165$)

	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$
$\sigma = \frac{1}{\rho-1}$	0.064	0.003	0.179
	(0.051)	(0.044)	(0.072)
$A = \left(\frac{1-a}{a}\right)^{\frac{1}{\rho-1}}$	0.510	0.537	0.398
	(0.027)	(0.027)	(0.032)
δ_s	1.009	1.008	1.009
	(0.018)	(0.018)	(0.023)
β_s	0.974	0.974	0.967
	(0.017)	(0.017)	(0.023)
δ_o	1.010	1.014	1.018
	(0.017)	(0.016)	(0.021)
β_o	1.013	1.008	1.011
	(0.020)	(0.020)	(0.026)
Observations	7920	7920	7920
Participants	165	165	165
$H_0(\hat{\beta}_s = 1)$	$p = 0.133$	$p = 0.127$	$p = 0.147$
$H_0(\hat{\beta}_o = 1)$	$p = 0.511$	$p = 0.667$	$p = 0.655$
$H_0(\hat{\beta}_s = \hat{\beta}_o)$	$p = 0.158$	$p = 0.209$	$p = 0.221$

Note: The table reports the parameter estimates from all dictator games (blocks SoonSoon, SoonLate, LateLate, and LateSoon). Column (1) uses the log-linearized first order condition, while columns (2) and (3) use the closed form solution for the number of tasks allocated to oneself. Standard errors are clustered at the individual level and calculated via the delta method.

Table A5: Parameter estimates from all dictator games

B An Alternative Model of "Warm Glow"

As discussed at the end of Section 3, once we think about generosity being motivated by feelings of "warm glow" it is natural to assume that positive utility may be received both at the time of decision as well at the time in the future when the decision materializes for the other person. Andreoni and Serra-Garcia (2019) show empirically that agents are more likely to make a donation to charity when this decision realizes in the future rather than immediately. They rationalize their finding by assuming that when donations are in the future, agents who care about their social image receive image utility from donating both when the decision to donate is made as well as when the charity actually receives the donation, with the latter possibly being smaller than the former due to discounting. Similarly, Shapiro (2020) assumes that agents receive warm glow utility when a gift is made plus warm glow utility when the beneficiary's utility increases, with the latter being discounted since it is assumed to happen in the future.

Given that in our case agents decide about unpleasant consumption and because we so far expressed our theory in terms of minimizing one's own and the other persons disutility from working on the tasks, we think of $g(o_{t,\tau})$ as a form of "warm glow cost", i.e., a disutility from confronting the other person with more tasks. Accordingly, $g(\cdot)$ is an increasing and convex function which an agent seeks to minimize. We assume that $g(o_{t,\tau})$ is weighted by a factor $1 \geq \eta_{t,\tau} > 0$, depending on when a decision was taken (τ) and the period at which the decision realizes (t). Hence, for decisions which realize immediately, i.e., when $\tau = t$, the warm glow (dis)utility is given by $\eta_{t,t} g(o_{t,\tau})$. If however, the decision is made in a previous period, i.e., $\tau = t - k$, with $k > 0$, the total warm glow (dis)utility is given by $\eta_{t,t} g(o_{t,t-k}) + \eta_{t,t-k} g(o_{t,t-k})$. Without loss of generality, we normalize $\eta_{t,t} = 1$. We also assume that $\eta_{t,t} = \eta_{t-1,t-1}$, which allows us to simplify the notation by denoting η_k as the weight on warm glow of a decision which was taken k periods earlier. Thus, a natural interpretation of η_k is that it discounts warm glow (dis)utility from decisions which materialize in the future.

Paralleling our approach in Section 3, we can use this model to analyze the incentives to give in our dictator games. To illustrate this, we focus on the two symmetric cases, blocks SoonSoon and LateLate. In block SoonSoon, agents then minimize

$$
\begin{array}{ll}
u_s(s_{t,t}) + g(o_{t,t}) & \text{if} \quad \tau = t \\
\beta_s \delta_s u_s(s_{t,t-1}) + g(o_{t,t-1}) + \eta_1 g(o_{t,t-1}) & \text{if} \quad \tau = t - 1
\end{array}
$$

The first order condition can then be expressed as:

$$\frac{u_s'(s_{t,\tau})}{g'(o_{t,\tau})} = \frac{1}{R}\left(\frac{1+\eta_1}{\beta_s\delta_s}\right)^{\mathbf{1}\{t\neq\tau\}}$$

The difference between week 2 decisions ($\tau = t$) and week 1 decisions ($\tau = t-1$) are thus driven by $\frac{1+\eta_1}{\beta_s\delta_s}$. Intuitively, if decisions are not immediate, then the agent realizes feelings of warm glow twice. This also means that, unless the agent is substantially future-biased ($\beta_s\delta_s > 1$), generosity will always be larger when the agent decides in week 1 rather than in week 2, because then $\frac{1+\eta_1}{\beta_s\delta_s} > 1$.

Similarly, for block LateLate, the first order condition is given as:

$$\frac{u_s'(s_{t+1,\tau})}{g'(o_{t+1,\tau})} = \frac{1}{R}\frac{1+\eta_1}{\beta_s\delta_s}\left(\frac{1+\eta_2}{\delta_s(1+\eta_1)}\right)^{\mathbf{1}\{t\neq\tau\}}$$

It thus becomes apparent, that agents display present-biased generosity, as defined in Section 3, if and only if

$$\frac{1+\eta_1}{\beta_s\delta_s} > \frac{1+\eta_2}{\delta_s(1+\eta_1)} \qquad \Leftrightarrow \qquad \frac{(1+\eta_1)^2}{1+\eta_2} > \beta_s$$

This inequality highlights a number of noteworthy aspects: First, under the natural assumption that $\eta_1 \geq \eta_2$ (i.e., that warm glow (dis)utility from decisions which realize later are weighted less), the left-hand side is always larger than one. Hence, unless the agent is strongly future biased and future warm-glow (dis)utility is discounted heavily, the model predicts present-biased generosity but not the reverse behavior, i.e., "future-biased generosity".

Second, even if agents are not present-biased in own consumption, i.e., $\beta_s = 1$, agents may display present-biased generosity whenever $\eta_1 > 0$. In these cases, present-biased generosity is exclusively driven by the fact that all decisions which have consequences for another person materialize in the future yield immediate and future warm glow utility, while decisions which have only immediate consequences for another person only yield immediate warm glow.

Third, while to us it seems most natural to think about the model as one which only considers warm glow (immediately and in the future), one could also combine these ideas with our approach and include both warm glow as well as altruistic utility which is discounted by β_o and δ_o. This would make the model considerably less tractable, but since it retains the key idea of the previous two paragraphs, it can also only plausibly rationalize present-biased generosity.

C Details of the Structural Estimation

We start by describing our estimation approaches for the two intrapersonal blocks, Self and Other. After that we discuss our approach for the interpersonal choices of our dictator games.

C.1 *Intrapersonal Decisions*

FOC Approach. The first approach to structurally estimate the parameters of the agent's utility function when only own consumption is at stake broadly follows Augenblick et al. (2015) and Andreoni and Sprenger (2012) and uses the log-linearization of the first-order condition. For block Self and using the functional form $u_s(s_{t,\tau}) = s_{t,\tau}^{\gamma_s}$, the log-linearized version of equation (2) can be written as:

$$\ln\left(\frac{s_{t,\tau}+\omega}{s_{t+1,\tau}+\omega}\right) = \frac{\ln(\delta_s)}{\gamma_s-1} + \frac{\ln(\beta_s)}{\gamma_s-1}\mathbf{1}\{t=\tau\} - \frac{1}{\gamma_s-1}\ln(R) \tag{C.1}$$

Assuming that choices are made with an additive error ε which is normally distributed with mean zero, we can estimate the parameters of interest via the following regression equation:

$$\ln\left(\frac{s_{2,\tau}+\omega}{s_{3,\tau}+\omega}\right)_i = \kappa_0 + \kappa_1 D_i + \kappa_2 \ln(R)_i + \varepsilon_i \tag{C.2}$$

where

$$D = \begin{cases} 1 & \text{if} \quad \tau = 2 \\ 0 & \text{otherwise} \end{cases}$$

Identification of the three parameters is obtained as follows: variation in the rate R which denotes how "costly" it is for oneself to complete a task later rather than sooner, identifies the curvature parameter γ_s. Since each participant faces the same decision problems in $\tau = 1$ and $\tau = 2$, variation in τ (captured via D) identifies the present bias parameter β_s. The standard exponential discounting parameter δ_s is then recovered via the constant. The estimates for the parameters of interest can then be calculated as follows:

$$\hat{\gamma_s} = -\frac{1}{\hat{\kappa}_2} + 1 \qquad \hat{\delta}_s = \exp\left(\frac{-\hat{\kappa}_0}{\hat{\kappa}_2}\right) \qquad \hat{\beta}_s = \exp\left(\frac{-\hat{\kappa}_1}{\hat{\kappa}_2}\right)$$

As discussed in the main text, in this approach we set the background consumption to $\omega = 10$, which avoids the natural logarithm to be undefined for corner solutions. We use a two-limit Tobit model which takes into account corner solutions at $s_{t,\tau} = 0$ and $s_{t,\tau} = 50$.

Standard errors are calculated via the delta-method.

For block OTHER, we follow the exact same procedures to obtain estimates for $\gamma_o, \beta_o, \delta_o$, and we thus refrain from repeating this here. In the case where we jointly estimate time preference parameters for self and others using the data from both blocks (see Table A2), the regression equation is given by:

$$\ln\left(\frac{c_{2,\tau}+\omega}{c_{3,\tau}+\omega}\right)_i = \kappa_{0,s}CS_i + \kappa_{0,o}CO_i + \kappa_{1,s}DS_i + \kappa_{1,o}DO_i + \kappa_2\ln(R)_i + \varepsilon_i \tag{C.3}$$

where

$$CS = \begin{cases} 1 & \text{if} \quad \text{block SELF} \\ 0 & \text{otherwise} \end{cases} \qquad CO = \begin{cases} 1 & \text{if} \quad \text{block OTHER} \\ 0 & \text{otherwise} \end{cases}$$

$$DS = \begin{cases} 1 & \text{if} \quad \text{block SELF and } \tau = 2 \\ 0 & \text{otherwise} \end{cases} \qquad DO = \begin{cases} 1 & \text{if} \quad \text{block OTHER and } \tau = 2 \\ 0 & \text{otherwise} \end{cases}$$

and $c_{t,\tau} \in \{s_{t,\tau}, o_{t,\tau}\}$. The parameters of interest can then be recovered as:

$$\hat{\gamma} = -\frac{1}{\hat{\kappa}_2}+1 \quad \hat{\delta}_s = \exp\left(\frac{-\hat{\kappa}_{0,s}}{\hat{\kappa}_2}\right) \quad \hat{\beta}_s = \exp\left(\frac{-\hat{\kappa}_{1,s}}{\hat{\kappa}_2}\right) \quad \hat{\delta}_o = \exp\left(\frac{-\hat{\kappa}_{0,o}}{\hat{\kappa}_2}\right) \quad \hat{\beta}_o = \exp\left(\frac{-\hat{\kappa}_{1,o}}{\hat{\kappa}_2}\right)$$

CFS Approach. The second approach to structurally estimate the parameters of the agent's utility function is based on the closed form solution for $\tilde{s}_{2,\tau}$, the ratio of the number of tasks allocated to the sooner date (week 2) divided by $m = 50$, which can be written as:

$$\tilde{s}_{2,\tau} = \frac{R^{-\frac{\gamma_s}{\gamma_s-1}}\left[\beta_s^{\mathbf{1}\{\tau=2\}}\delta_s\right]^{\frac{1}{\gamma_s-1}} + \omega\left(R^{-\frac{1}{\gamma_s-1}}\left[\beta_s^{\mathbf{1}\{\tau=2\}}\delta_s\right]^{\frac{1}{\gamma_s-1}} - 1\right)}{1 + R^{-\frac{\gamma_s}{\gamma_s-1}}\left[\beta_s^{\mathbf{1}\{\tau=2\}}\delta_s\right]^{\frac{1}{\gamma_s-1}}} \equiv g(\omega, R, \tau; \gamma_s, \beta_s, \delta_s) \tag{C.4}$$

The corresponding likelihood contribution by observation i, assuming normally distributed decision errors, is given by:

$$L_i = \left[\Phi\left(\frac{0-g_i(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{\tilde{s}^i=0\}} \left[\phi\left(\frac{\tilde{s}^i_{t,\tau}-g_i(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{0<\tilde{s}^i<1\}} \left[\Phi\left(\frac{1-g_i(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{\tilde{s}^i=1\}}$$

Like in our first approach, we use two-limit Tobit maximum-likelihood estimations using STATA. We present parameter estimates for two different values of background consumption, $\omega = 0$ and $\omega = 10$.

The estimation of parameters when deciding on behalf of others (block OTHER) proceeds analogously. For the joint estimation using the data from both blocks, we base the maximum-likelihood estimation on the following expression for $\tilde{c}_{2,\tau} \in \{\tilde{s}_{2,\tau}\ ,\ \tilde{o}_{2,\tau}\}$:

$$\tilde{c}_{2,\tau} = \frac{R^{-\frac{\gamma}{\gamma-1}} Z(B) + \omega \left(R^{-\frac{1}{\gamma-1}} Z(B) - 1 \right)}{1 + R^{-\frac{\gamma}{\gamma-1}} Z(B)} \equiv g(\omega, R, \tau; \gamma, \beta_s, \delta_s, \beta_o, \delta_o) \tag{C.5}$$

where:

$$Z(B) = \begin{cases} \left(\beta_s^{\mathbf{1}\{t=\tau\}} \delta_s \right)^{\frac{1}{\gamma-1}} & \text{if} \quad B = 1 \quad (\text{SELF}) \\ \left(\beta_o^{\mathbf{1}\{t=\tau\}} \delta_o \right)^{\frac{1}{\gamma-1}} & \text{if} \quad B = 2 \quad (\text{OTHER}) \end{cases}$$

C.2 ***Interpersonal Decisions***

We now explain our structural estimation approach for the interpersonal decisions. The discussion in Section 3 in the main text made it clear that to estimate time preference parameters for these decision, we need to combine the results from all four dictator games blocks because otherwise the parameters are not fully identified. In addition, close inspection of equation (4) in the main text, which will form the basis of our structural estimation, reveals that from our dictator game data alone, we cannot separately identify the value of ρ from the atemporal utility functions $u_s(\cdot)$ and $u_o(\cdot)$. To circumvent this issue, we first proceed by making the simplifying assumption that the atemporal utility functions, or in this case the cost of effort functions, are linear, i.e., $u_s(s_{t,\tau}) = s_{t,\tau}$ and $u_o(o_{t,\tau}) = o_{t,\tau}$. In Section F below, we discuss the implications of this assumption and provide the results of a robustness check, which demonstrate that the linearity assumption does not significantly affect our estimates, neither qualitatively nor quantitatively.

FOC Approach. The econometric specification for estimating time preferences for the dictator games proceeds along very similar lines as for the intrapersonal choices. Like before, we use two different approaches. The first is based on the log-linearized first order conditions, which can be written as:

Block SoonSoon:

$$\ln\left(\frac{s_{2,\tau}+\omega}{o_{2,\tau}+\omega}\right)=\ln(A)-\sigma\ln(R)-(\sigma+1)\Big[\ln\left(\frac{\beta_s\delta_s}{\beta_o\delta_o}\right)\mathbf{1}\{\tau=1\}\Big]$$

Block LateLate:

$$\ln\left(\frac{s_{3,\tau}+\omega}{o_{3,\tau}+\omega}\right)=\ln(A)-\sigma\ln(R)-(\sigma+1)\Big[\ln\left(\frac{\beta_s\delta_s^2}{\beta_o\delta_o^2}\right)\mathbf{1}\{\tau=1\}+\ln\left(\frac{\beta_s\delta_s}{\beta_o\delta_o}\right)\mathbf{1}\{\tau=2\}\Big]$$

Block SoonLate:

$$\ln\left(\frac{s_{2,\tau}+\omega}{o_{3,\tau}+\omega}\right)=\ln(A)-\sigma\ln(R)-(\sigma+1)\Big[\ln\left(\frac{\beta_s\delta_s}{\beta_o\delta_o^2}\right)\mathbf{1}\{\tau=1\}+\ln\left(\frac{1}{\beta_o\delta_o}\right)\mathbf{1}\{\tau=2\}\Big]$$

Block LateSoon:

$$\ln\left(\frac{s_{3,\tau}+\omega}{o_{2,\tau}+\omega}\right)=\ln(A)-\sigma\ln(R)-(\sigma+1)\Big[\ln\left(\frac{\beta_s\delta_s^2}{\beta_o\delta_o}\right)\mathbf{1}\{\tau=1\}+\ln\left(\frac{\beta_s\delta_s}{1}\right)\mathbf{1}\{\tau=2\}\Big]$$

We then set up the following regression equation:

$$\ln\left(\frac{s+\omega}{o+\omega}\right)_i=\lambda_0+\lambda_1 D1_i+\lambda_2 D2_i+\lambda_3 D3_i+\lambda_4 D4_i+\lambda_5 D5_i+\lambda_6 D6_i+\lambda_7\ln(R)_i+\varepsilon_i \quad \text{(C.6)}$$

where

$$D1_i=\begin{cases}1 & \text{if } \tau=1 \text{ \& Block SoonSoon}\\ 1 & \text{if } \tau=2 \text{ \& Block LateLate}\\ 0 & \text{otherwise}\end{cases} \qquad D2_i=\begin{cases}1 & \text{if } \tau=1 \text{ \& Block LateLate}\\ 0 & \text{otherwise}\end{cases}$$

$$D3_i=\begin{cases}1 & \text{if } \tau=1 \text{ \& Block SoonLate}\\ 0 & \text{otherwise}\end{cases} \qquad D4_i=\begin{cases}1 & \text{if } \tau=1 \text{ \& Block LateSoon}\\ 0 & \text{otherwise}\end{cases}$$

$$D5_i=\begin{cases}1 & \text{if } \tau=2 \text{ \& Block SoonLate}\\ 0 & \text{otherwise}\end{cases} \qquad D6_i=\begin{cases}1 & \text{if } \tau=2 \text{ \& Block LateSoon}\\ 0 & \text{otherwise}\end{cases}$$

If we were to estimate (C.6) like this, we would have 8 estimates to identify 6 parameters, and hence the model would be overidentified. We thus impose two linear constraints as to make the model just identified. These constraints can be written as:

$$\lambda_2 - \lambda_4 = \lambda_3 - \lambda_5 - \lambda_6 \qquad \lambda_3 - \lambda_2 = \lambda_1 - \lambda_4 \tag{C.7}$$

We then estimate equation (C.6) via a two-limit tobit regression, in order to account for corner solutions at $s_{t,\tau} = 0$ and $o_{t,\tau} = 0$. As discussed in the main text, we set the background consumption $\omega = 10$. The estimates for the parameters of interest can be recovered from the coefficients as:

$$\hat{A} = \exp\left(\hat{\lambda}_0\right) \qquad \hat{\beta}_s = \exp\left(\frac{\hat{\lambda}_2 - \hat{\lambda}_1 - \hat{\lambda}_3 + \hat{\lambda}_5}{-\hat{\lambda}_7 + 1}\right) \qquad \hat{\delta}_s = \exp\left(\frac{\hat{\lambda}_3 - \hat{\lambda}_2}{-\hat{\lambda}_7 + 1}\right)$$

$$\hat{\beta}_o = \exp\left(\frac{\hat{\lambda}_1 - \hat{\lambda}_2 + \hat{\lambda}_4 - \hat{\lambda}_6}{-\hat{\lambda}_7 + 1}\right) \qquad \hat{\delta}_o = \exp\left(\frac{\hat{\lambda}_2 - \hat{\lambda}_4}{-\hat{\lambda}_7 + 1}\right) \qquad \hat{\sigma} = -\hat{\lambda}_7$$

As before, we use the delta-method to calculate the appropriate standard errors.

CFS Approach. The second approach to structurally estimate the parameters of the agent's utility function is based on the closed form solution for $s_{t,\tau}$, which can be written as:

$$\tilde{s}(B,\tau) = \frac{R^{-\sigma-1}Z(B,\tau) + \omega\left(R^{-\sigma}Z(B,\tau) - A^{-1}\right)}{A^{-1} + R^{-\sigma-1}Z(B,\tau)} \equiv g(\omega, R, B, \tau; A, \sigma, \beta_s, \delta_s, \beta_o, \delta_o) \tag{C.8}$$

Here, $\tilde{s}(B,\tau)$ denotes the number of tasks allocated to oneself, $s_{t,\tau}$, divided by the total budget m. We use $B \in \{3,4,5,6\}$ to distinguish the four different blocks, as described in Table 1 in the main text. $Z(B,\tau)$ is a "discounting function" which takes on different values depending on the block B and the week τ in which the decision was made. These values can be taken directly from the first-order conditions as presented in the main text. $Z(B,\tau)$ is given by:

$$Z(B,\tau) = \begin{cases} 1 & \text{if} \quad B=3 \quad \text{and} \quad \tau=2 \\ \left(\frac{\beta_o\delta_o}{\beta_s\delta_s}\right)^{\sigma+1} & \text{if} \quad B=3 \quad \text{and} \quad \tau=1 \\ \left(\frac{\beta_o\delta_o}{\beta_s\delta_s}\right)^{\sigma+1} & \text{if} \quad B=4 \quad \text{and} \quad \tau=2 \\ \left(\frac{\beta_o\delta_o^2}{\beta_s\delta_s^2}\right)^{\sigma+1} & \text{if} \quad B=4 \quad \text{and} \quad \tau=1 \\ \left(\frac{\beta_o\delta_o^2}{\beta_s\delta_s}\right)^{\sigma+1} & \text{if} \quad B=5 \quad \text{and} \quad \tau=1 \\ (\beta_o\delta_o)^{\sigma+1} & \text{if} \quad B=5 \quad \text{and} \quad \tau=2 \\ \left(\frac{\beta_o\delta_o}{\beta_s\delta_s^2}\right)^{\sigma+1} & \text{if} \quad B=6 \quad \text{and} \quad \tau=1 \\ \left(\frac{1}{\beta_s\delta_s}\right)^{\sigma+1} & \text{if} \quad B=6 \quad \text{and} \quad \tau=2 \end{cases}$$

Assuming normally distributed decision errors, ε, i.e., $\tilde{s}^i(B,\tau) = g_i(\boldsymbol{\theta}) + \varepsilon_i$, and taking into account the presence of corner solutions, we can define the likelihood contribution for decision i as:

$$L_i = \left[\Phi\left(\frac{0 - g_i(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{\tilde{s}^i=0\}} \left[\phi\left(\frac{\tilde{s}^i_{t,\tau} - g_i(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{0<\tilde{s}^i<1\}} \left[\Phi\left(\frac{1 - g_i(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{\tilde{s}^i=1\}} \quad \text{(C.9)}$$

which we use for standard maximum-likelihood estimation via STATA. We present parameter estimates for two different values of background consumption, $\omega = 0$ and $\omega = 10$.

D Individual-level Analysis

For the estimation of the parameters at the individual level, we use, as described in the main text, the approach based on the closed form solutions. More precisely, for the dictator game data, we estimate equation (C.8) separately for each individual, and for the data from the intrapersonal blocks we use equation (C.5). In both cases we set $\omega = 10$.

From the derivations of the first-order conditions, it becomes clear that for an interior solution to exist, the parameters ρ and γ cannot be less than one. While this poses no major problem for the aggregate analysis as these restrictions are always met, for the individual analysis this might not always hold. To avoid this problem, for the intrapersonal choices, we replace γ by $\exp\{\tilde{\gamma}\} + 1$. Here, $\tilde{\gamma}$ then is the parameter to be estimated and we obtain γ by converting it using the above expresseion. For the interpersonal choices, where the estimation is based on $\sigma = \frac{1}{\rho - 1}$, the restriction $\rho \geq 1$ corresponds to $\sigma \geq 0$, and hence we replace σ by $\exp\{\tilde{\sigma}\}$, by the same logic as above.

To obtain parametric estimates for as many participants as possible, it is further necessary to specify different sets of initial values for different participants. We always re-estimate the parameters for all individuals when using a different set of starting values to ensure that the estimates are not driven by the specific values chosen. In all but four cases (all in the interpersonal decisions), conditional on the estimation converging, we obtain exactly the same estimates. In the remaining cases, we choose the estimates for which the log-likelihood is largest. In addition, we "manually" check the data for all participants where we do not obtain convergence. For the intrapersonal choices, there are sixteen participants for which we can directly determine that $\beta_s = \beta_o = 1$. This is because there is no variation across weeks, hence behavior shows neither a future nor a present bias. The estimation does not converge because these participants always choose the cheaper account to allocate the tasks to, i.e., $X = 0$ if $R < 1$ and $X = 50$ if $R > 1$ and $X = 25$ if $R = 1$. We include these participants in our individual-level analysis in the main text. The behavior of ten additional participants can be fully rationalized with our model, but we can only obtain bounds on β_s and β_o, i.e., whether they are (weakly) larger or smaller than one, but no point identification is possible. Therefore, these participants do not appear in our individual-level analysis.

For the dictator games, we can identify bounds for one participant, while for another three participants behavior is too noisy to yield convergence. The estimated parameters for each participant are reported in Tables D1 to D9 below. These tables also contain entries for those subjects for whom we only obtain bounds on β_s and β_o.

id	δ_s	β_s	δ_o	β_o
101	1.112	0.884	1.19	0.952
102	1.212	1.46	0.873	0.911
104	1	0.498	1	0.493
105	1.035	1.04	1.014	1
106	0.556	0.75	0.407	0.8
107	1	0.996	1	0.992
108	0.807	0.613	0.825	0.668
110	0.936	0.825	1.101	1.034
111	1	0.95	1.029	0.996
112	1.08	0.949	0.878	0.861
113	0.828	0.782	0.727	0.752
114^{+}	1.443	1.301	1.799	1.761
115	1.007	1.02	0.976	0.979
116	0.997	0.99	0.998	1
117	0.631	0.842	0.971	1
119	0.888	0.841	0.888	0.841
121	1.34	0.885	0.959	0.892
122	1.003	1	1.003	1
123	0.974	0.881	1.016	0.979
125	0.897	1.02	0.789	0.947
126	1.002	1	0.956	0.955
127	1.049	1.073	1.028	1.052
129	0.995	0.853	0.956	0.879
130	0.989	1.077	0.997	1.009
202	0.911	0.943	0.975	1.334
203	0.637	1.103	0.632	1.083
204	1.081	0.964	0.908	1.031
205	1.027	1	1.027	1
206	0.987	1.007	0.976	0.988
208	0.778	0.799	0.783	0.813
209	0.647	0.867	0.98	0.993
210	0.518	0.849	0.568	0.581
211^{+}	1.326	1.914	5.812	5.191
212^{*}		1		1
213	1.017	1.021	1.017	0.956
215	1.007	1.01	0.987	0.989
216	1.058	1.09	1.097	0.848
217	1.049	0.401	0.998	0.915
218^{*}		≤ 1		1
220^{*}		≥ 1		≥ 1
221	0.838	0.786	0.981	1.133
222	0.967	0.989	0.986	0.979
223	0.901	0.784	0.992	0.991
224	0.987	0.997	0.988	0.999
226	0.855	0.912	1.07	1.251
227	1.006	1.009	0.997	1
228	1.12	1.09	1.007	0.99
229	0.987	0.99	1.016	1.007
230^{+}	1.031	1.079	0.189	0.2

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with $^{+}$ denote cases which are excluded based on a Grubb's outlier test (see footnote 19 in the main text).

Table D1: Individual parameter estimates from intrapersonal choices (id 101-230)

id	δ_s	β_s	δ_o	β_o
304	1.092	0.892	1.093	0.82
3055	1.285	1.097	1.219	1.063
306	0.886	0.923	0.992	1.016
308	1.016	1.013	1.016	1.013
309	0.955	1.19	1.063	1.065
310	1.127	1.116	0.992	0.987
311*		≥ 1		1
312	0.902	0.855	0.869	0.821
314+	0.836	0.859	1.83	1.742
315	0.506	0.162	1.38	0.991
316+	2.474	2.439	1.026	0.981
317+	0.243	0.66	1.006	1.896
319	1.328	1.336	1	1
320*		≥ 1		≤ 1
321	1.432	0.964	0.67	0.634
323	1.016	0.921	1.066	0.895
324	1.016	0.99	1.013	1.018
326	1.004	1.001	1.004	1.001
327	0.975	1.001	1.007	1.019
328	0.599	0.601	0.861	1.081
329	1.269	1.054	0.999	0.856
330	0.851	1	0.955	1.028
401	0.988	1.029	0.88	0.982
402	1.002	0.804	1.105	1.155
403	0.885	0.867	1.002	0.872
404*		1		1
405	0.875	0.867	0.963	1.245
406	0.986	0.993	0.891	1.313
408	0.987	0.807	0.979	1.113
409	0.817	0.472	0.901	1.037
410	0.961	0.982	0.873	0.885
411	1.017	1.002	1.003	1.1
413	0.993	1.009	1.025	1.003
414	1.007	1	0.939	0.932
415	0.987	0.99	0.977	0.971
417	0.961	0.734	0.857	0.961
418	1.024	1.117	1.004	0.984
419	1.058	0.587	0.905	0.88
420	0.981	0.964	1.129	1.111
421	0.996	1	0.996	1
422	0.491	0.949	0.998	1.009
423	1.208	0.732	0.919	0.815
424	0.831	0.871	0.886	0.885
425	1.007	1.009	0.997	0.991
426	0.993	1.055	0.941	0.996
427	0.961	0.993	0.996	0.985

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with + denote cases which are excluded based on a Grubb's outlier test (see footnote 19 in the main text).

Table D2: Individual parameter estimates from intrapersonal choices (id 301-430)

id	δ_s	β_s	δ_o	β_o
501	0.995	1.038	0.909	0.894
503	0.984	0.948	1.015	0.932
504	1.05	1.006	1.05	1.18
505	1.027	1.021	1.027	1.03
506	0.888	1.103	0.965	1.322
509	1.121	0.932	1.139	1.144
510	1.005	0.995	0.928	0.918
512*		≤ 1		1
514	0.954	0.83	1.025	0.917
515	0.925	1.16	0.941	1.055
517*		1		1
518*		1		1
520*		1		1
521*		1		1
523	0.867	0.91	1.083	1.148
524	0.976	1.054	0.951	0.976
525*		1		1
526	0.997	1.029	0.997	0.991
529	0.98	0.945	1.007	1.007
530	0.891	0.817	0.926	1.002
531	1.027	1	1.027	1
532	0.728	0.909	0.856	1.17
601	0.997	0.925	1.027	0.992
602*		≤ 1		1
605	0.928	1.216	0.947	1.03
606	1.001	1.1	0.909	1.012
607	0.768	0.798	1.055	1.221
608	1.187	0.961	1.22	1.275
609	0.93	0.896	0.985	0.946
610	1.017	1.042	1.007	1.054
611*		1		1
613*		1		1
615	0.581	0.624	0.696	0.747
616	1.026	0.834	1.03	0.722
617	0.972	0.72	0.98	1.012
618	0.977	1.018	1.008	1.049
620	0.969	0.983	0.969	0.992
622	1.133	1.18	1.03	0.666
624	1.026	0.824	1.011	1.049
625+	1.007	0.836	2.449	2.292
626*		1		1
627	0.766	0.584	0.743	0.565
628	1.029	1.021	0.988	1.171
629	1.07	0.936	0.99	0.948
630	0.535	1.057	0.751	0.851
631	1	1	1	1

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with + denote cases which are excluded based on a Grubb's outlier test (see footnote 19 in the main text).

Table D3: Individual parameter estimates from intrapersonal choices (id 501-631)

id	δ_s	β_s	δ_o	β_o
701	0.998	1.01	0.969	0.953
702	0.931	0.886	0.825	0.786
703	0.942	0.97	0.997	1.076
705	1.012	1.084	0.979	1.031
707	0.998	0.991	0.96	0.984
708*		1		1
710	0.956	0.991	0.979	1.136
711	1.018	1.011	0.997	0.804
715*		1		1
716	1.102	1.123	1.066	1.135
717	0.858	0.973	1.046	1.06
718	0.788	0.523	1.047	1.004
721	0.949	0.913	0.94	0.93
722	1.019	0.966	1.05	1.02
723	0.715	0.667	0.838	0.884
724	0.957	0.931	1.068	1.039
725	0.905	0.96	0.94	1.01
726	1.225	0.983	1.079	0.829
727	1.007	1	1.007	1
728	0.975	0.901	1.139	0.982
729	1.049	1.071	1.085	0.971
732	0.999	1.001	1.007	1
801	1.112	1.097	0.933	0.933
802	0.77	1.012	1.109	1.077
804	0.58	0.175	0.847	0.938
805	1.028	1.011	0.968	0.969
807*		1		1
808	0.967	0.988	0.971	0.974
809	1.015	1	0.968	1.048
811*		1		1
812*		1		1
813	0.842	0.924	1.005	1.179
814	0.969	1.009	0.969	1
816	0.78	0.753	0.906	0.855
818	1.248	1.224	1.01	1
820*		≥ 1		≤ 1
821	0.895	0.867	0.961	0.95
822$^+$	4.363	4.81	1.232	1.223
825	1.109	0.975	0.876	0.738
826	1.142	1.045	1.259	1.493
827	0.908	0.861	0.958	0.843
828	0.997	0.997	1.003	1.003
829	1.128	1.056	1.039	0.99
830*		≤ 1		≤ 1

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with $^+$ denote cases which are excluded based on a Grubb's outlier test (see footnote 19 in the main text).

Table D4: Individual parameter estimates from intrapersonal choices (id 701-830)

id	δ_s	β_s	δ_o	β_o
902*		1		1
904*		≥ 1		1
906	1.107	0.962	1.082	0.966
908	0.957	0.903	0.955	0.888
909	1.189	1.199	1.045	1.08
910	0.967	0.914	0.998	1.003
911	1	0.996	1	0.999
913	1.333	1.235	1.102	1.062
914	1.001	1.145	1.172	1.23
915	0.884	0.838	0.788	0.747
918	1.006	0.996	1.006	1.027
919	1.079	1.312	1.054	1.142
922	0.886	0.806	1.174	1.102
923	1.124	1.086	1.058	1.051
924	0.903	1	0.999	1.104
925	1.09	1.058	1.008	1
926	1.182	1.231	1.163	1.22
927+	0.993	1.283	1.007	2.03
928	0.586	0.928	0.981	1.068
929	0.911	0.92	0.94	0.969
931	1.007	1	1.007	1
932	0.852	0.847	0.862	0.899

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with + denote cases which are excluded based on a Grubb's outlier test (see footnote 19 in the main text).

Table D5: Individual parameter estimates from intrapersonal choices (id 902-932)

id	δ_s	β_s	δ_o	β_o
101	1.023	1.028	1.141	0.854
103	0.967	0.959	1.021	0.958
105	0.982	0.875	1.049	0.908
108	0.929	1.018	1.019	1.142
1118	1.083	0.943	1.074	0.912
113	0.778	1.132	0.825	1.446
114	0.831	1.365	0.971	1.118
115	1.002	0.99	0.873	1.155
117	1.327	0.748	1.022	1.167
119	0.945	0.997	1.058	1.004
122	1	1	1	1
125	0.996	0.899	0.944	0.994
126	1.065	0.944	1.018	0.971
127	1.001	1.001	0.979	1.019
129	1.163	0.8	0.86	0.931
130	1.026	0.847	1.173	0.945
202^+	12.627	0.072	0.079	13.452
203	1.686	0.674	1.425	0.742
205	1.006	0.995	1.004	0.995
208	0.954	0.987	1.082	0.963
209	1.263	0.857	1.204	0.873
210	0.999	0.989	1.165	0.87
211^+	1.786	0.628	0.532	2.429
214^*		< 1		
212	1.014	0.964	1.023	1
215	0.999	1	0.993	1.008
216	1.002	0.995	0.998	1.005
217	1.046	0.998	0.956	1.002
221	0.95	0.739	0.995	1.176
222	1	0.991	1	1
223	1.025	0.875	1.015	0.936
224	1.018	0.94	0.962	1.077
228	0.903	1.012	0.985	1.055

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with $^+$ denote cases which are excluded based on a Grubb's outlier test.

Table D6: Individual parameter estimates from interpersonal choices (id 101-230)

id	δ_s	β_s	δ_o	β_o
301	0.687	0.952	1.42	1.362
304	0.983	0.979	1.08	0.867
305	0.982	1.02	1.077	0.983
306	0.811	1.065	1.06	0.923
308	1.041	0.845	0.998	1.263
309	0.955	1.075	0.975	0.99
312	1.025	0.917	1.033	1.061
315	1.787	0.849	2.303	0.244
316	0.733	1.145	0.858	1.552
317^{+}	5803.466	0	0	10468.75
320	0.979	0.916	0.971	1.094
323	0.966	1.049	1.005	1.008
324	0.985	1.011	1.154	0.879
326	0.962	1.032	0.933	1.08
327	0.897	1.052	1.069	1.039
329	0.957	1.133	0.964	1.018
330	0.761	1.028	1.293	1.009
403	0.943	1.113	1.06	1.125
404^{*}	1	1	1	1
405	1.096	0.802	1.077	0.858
406	1.967	0.612	0.928	1.06
408	0.879	0.743	1.068	0.984
410	0.97	1.042	1	0.987
411	0.837	1.136	0.716	1.444
413	0.885	1.031	1.074	1.021
415	0.963	0.946	0.99	1.177
417	1.133	0.8	1.083	1.025
418	0.893	1.247	1.263	0.834
419	0.909	0.658	1.422	0.507
420	1.377	0.859	0.726	1.164
421	1.005	0.991	1.005	0.991
422	1.967	0.641	1.032	1.171
423	1.023	0.816	0.996	1.089
424	1.244	0.85	1.084	0.918
425	0.941	1.293	1.062	0.773
426	0.979	0.997	0.943	1.045
427	1.051	0.913	0.997	0.997

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with + denote cases which are excluded based on a Grubb's outlier test.

Table D7: Individual parameter estimates from interpersonal choices (id 301-430)

id	δ_s	β_s	δ_o	β_o
501	0.97	1.024	0.993	0.988
504	0.936	0.787	1.099	0.92
505	1.04	0.976	1.01	0.976
506	0.555	0.661	1.031	1.613
509	1.065	1.068	1.005	0.97
510	0.913	1.164	1.114	0.724
512	0.97	1.1	1.031	0.894
514	0.806	1.102	0.909	1.006
515	0.911	1.138	1.021	0.983
517	0.995	0.925	1.005	1.081
518	0.986	0.991	1.014	1.009
520	0.988	1.01	1.001	0.99
523	0.992	0.996	0.988	1.025
525	1.007	0.99	1.014	0.98
526	0.993	1	1.007	1
529	0.918	1.2	1.024	0.899
530	1.031	1.006	1.004	1.032
531	1	1	1	1
532	1.303	0.671	1.216	0.631
601	0.991	0.996	0.999	1.004
606	1.074	1.149	0.841	0.997
607	1.129	0.875	1.023	0.989
609	0.999	0.996	0.991	1.013
610	0.996	0.993	0.973	1.045
615	1.081	1.052	0.951	1.155
616	0.972	1.187	1.153	0.786
617	0.966	1.032	1.026	0.956
620	1.016	1.007	0.976	1.001
622	0.896	1.277	0.773	1.092
624	0.931	0.964	1.074	0.953
625	0.933	1.227	0.667	1.21
626	0.965	0.995	1.036	1.005
627	1.217	0.982	0.928	0.931
628	1.149	0.831	1.06	0.953
629	0.893	1.161	1.038	1.164
630	1.136	1.13	1.2	1.133
701	0.972	1.016	0.977	1.037
705	0.927	1.07	1.079	0.945
706	1.313	0.83	1.482	0.67
710	1	0.902	0.957	0.888
711	0.772	1.1	1.335	0.913
716	1.001	0.999	0.966	1.026
717	0.999	1.136	1.041	1.03
718	1.047	1.009	0.87	0.973

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with $^+$ denote cases which are excluded based on a Grubb's outlier test.

Table D8: Individual parameter estimates from interpersonal choices (id 501-718)

id	δ_s	β_s	δ_o	β_o
721	0.978	0.993	1.011	1.052
722	0.896	1.122	1.132	0.856
723	0.885	0.784	0.844	1.318
724	1.006	1.005	1.025	0.966
725	0.997	1.012	0.991	1.001
726	1.012	0.951	1.038	0.95
729	1.308	1.234	0.845	0.898
732	1.006	0.995	1.004	0.995
801	1.021	0.924	0.967	1.011
802	1.137	0.874	1.136	0.839
804^{+}	0.408	3.227	1.272	0.598
805	0.993	0.985	1.017	0.969
807	0.987	1.063	0.846	1.044
808	01.154	0.944	0.637	1.664
809	1.013	0.965	1.069	0.956
813^{+}	0.847	1.936	0.966	0.645
814	0.996	0.957	0.996	0.996
815	1.134	0.612	0.91	0.456
816	0.945	1.11	0.884	1.243
820	1.05	1.029	1.029	0.827
821	0.986	1.013	1.008	1.002
822^{+}	0.831	2.408	0.593	0.931
826	1.053	1.019	0.874	1.038
827	0.913	1.014	1.141	0.972
828	1.065	0.871	1.043	1.033
829	1.549	0.67	0.633	1.282
902	0.988	1.045	1.012	1.078
904	1.021	0.962	0.959	1.062
906	0.967	1.029	0.94	1.05
908	0.915	0.957	1.149	0.92
909	1.01	0.996	1.013	1.036
910	0.972	0.952	1.068	0.928
911	1.125	0.806	0.889	0.955
913	1.037	0.924	1.053	0.935
914	0.907	0.989	1.032	0.974
915	1.123	0.93	0.92	1.17
916	0.862	1.039	1.057	1.068
918	1.026	0.966	0.974	1.035
919	1.005	0.98	1.021	0.95
922	0.967	1.023	0.98	0.993
923^{+}	0.645	0.38	1.463	2.792
925	1.016	0.961	0.923	1.034
926	0.798	0.556	1.368	1.523
927^{+}	0.242	2.549	0.286	18.454
928	1.985	0.741	1.662	0.859
929	0.995	1.004	0.997	1.004
931	0.998	1.005	0.993	1.014
932	1.047	0.897	1.056	0.85

Note: The table reports the individual parameter estimates from the intrapersonal choices. IDs with (*) denote cases where we infer (bounds on) the values directly from the data without estimation. IDs with + denote cases which are excluded based on a Grubb's outlier test.

Table D9: Individual parameter estimates from interpersonal choices (id 721-932)

E Results split up by our original and our replication study

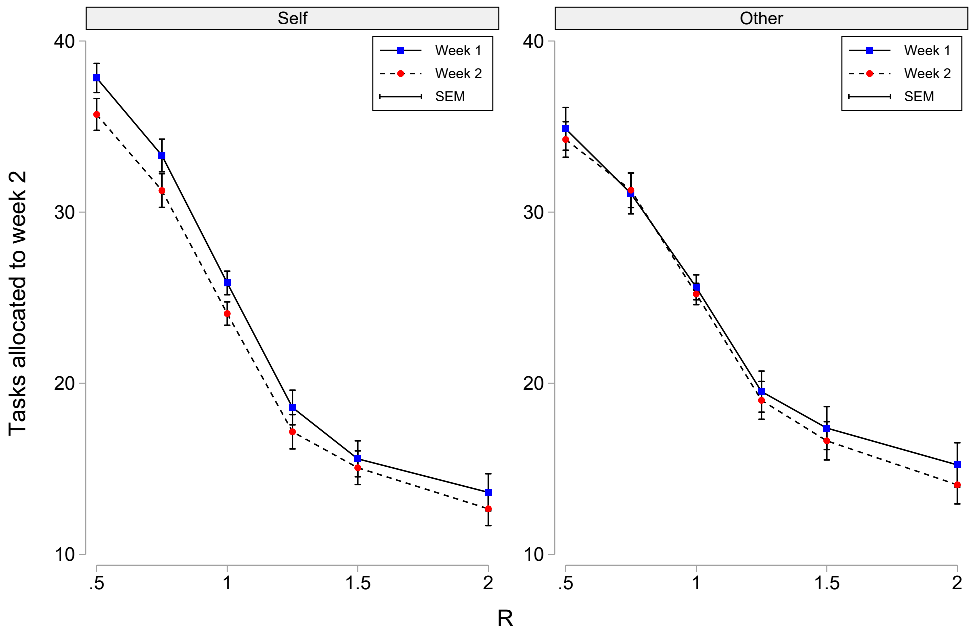


Figure E1: Effort allocations in intrapersonal decisions in the original study (Self: $n = 100$, Other: $n = 98$)

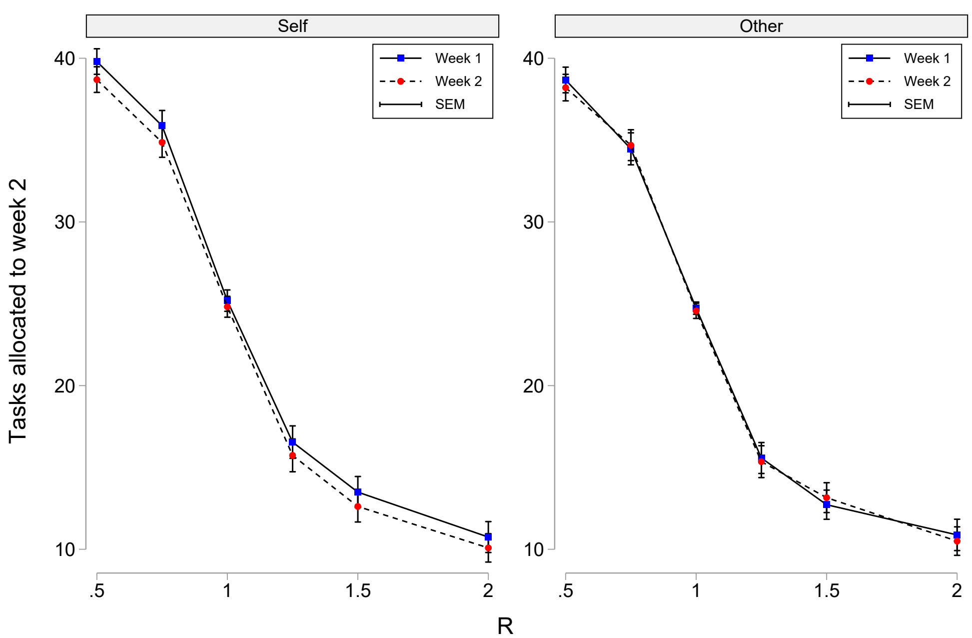


Figure E2: Effort allocations in intrapersonal decisions in the replication study (Self: $n = 115$, Other: $n = 116$)

Rate R	Self ($n = 100$)			Other ($n = 98$)		
	$\tau = 1$ Tasks soon	$\tau = 2$ Tasks soon	t-test	$\tau = 1$ Tasks soon	$\tau = 2$ Tasks soon	t-test
0.5	37.84	35.71	$p = 0.008$	34.87	34.24	$p = 0.544$
	(8.52)	(9.29)		(12.36)	(10.26)	
0.75	33.31	31.26	$p = 0.018$	31.08	31.29	$p = 0.838$
	(9.55)	(9.84)		(11.73)	(10.10)	
1	25.86	24.07	$p = 0.031$	25.60	25.21	$p = 0.608$
	(6.92)	(6.81)		(7.20)	(6.19)	
1.25	18.58	17.16	$p = 0.037$	19.51	19.00	$p = 0.581$
	(10.16)	(10.03)		(11.87)	(10.93)	
1.5	15.58	15.06	$p = 0.446$	17.38	16.63	$p = 0.356$
	(10.50)	(9.83)		(12.43)	(11.07)	
2	13.62	12.66	$p = 0.173$	15.22	14.06	$p = 0.168$
	(10.84)	(9.84)		(12.79)	(11.06)	
Overall	24.13	22.65	$p = 0.004$	23.94	23.14	$p = 0.252$
	(13.09)	(12.61)		(13.58)	(12.52)	

Note: The table denotes the number of tasks allocated to the sooner date, separately for block Self (left panel) and block Other (right panel). For each rate R, the p-value reported stems from a t-test with standard errors clustered at the individual level.

Table E1: Intrapersonal decisions: Aggregate behavior by task rate (Original study)

Rate R	Self ($n = 115$)			Other ($n = 116$)		
	$\tau = 1$ Tasks soon	$\tau = 2$ Tasks soon	t-test	$\tau = 1$ Tasks soon	$\tau = 2$ Tasks soon	t-test
0.5	39.80	38.70	$p = 0.132$	38.67	38.21	$p = 0.554$
	(8.37)	(8.39)		(8.45)	(8.73)	
0.75	35.88	34.86	$p = 0.193$	34.47	34.69	$p = 0.750$
	(9.99)	(9.79)		(10.52)	(10.14)	
1	25.19	24.82	$p = 0.647$	24.73	24.57	$p = 0.675$
	(7.00)	(6.83)		(4.04)	(5.00)	
1.25	16.55	15.74	$p = 0.326$	15.58	15.35	$p = 0.757$
	(10.63)	(10.75)		(10.19)	(10.47)	
1.5	13.50	12.62	$p = 0.185$	12.72	13.16	$p = 0.581$
	(10.20)	(10.18)		(9.60)	(9.82)	
2	10.75	10.09	$p = 0.336$	10.88	10.50	$p = 0.637$
	(10.12)	(9.30)		(10.32)	(9.37)	
Overall	23.61	22.80	$p = 0.085$	22.84	22.75	$p = 0.773$
	(14.54)	(14.34)		(14.06)	(14.01)	

Note: The table denotes the number of tasks allocated to the sooner date, separately for block Self (left panel) and block Other (right panel). For each rate R, the p-value reported stems from a t-test with standard errors clustered at the individual level.

Table E2: Intrapersonal decisions: Aggregate behavior by task rate (Replication study)

	Self ($j = s$)			Other ($j = o$)		
	(1)	(2)	(3)	(4)	(5)	(6)
	FOC	CFS	CFS	FOC	CFS	CFS
	$\omega = 10$	$\omega = 10$	$\omega = 0$	$\omega = 10$	$\omega = 10$	$\omega = 0$
γ_j	2.284	2.667	2.083	2.748	3.534	2.688
	(0.256)	(0.402)	(0.277)	(0.551)	(1.050)	(0.726)
δ_j	1.045	1.046	1.023	0.989	0.991	0.967
	(0.052)	(0.063)	(0.059)	(0.055)	(0.074)	(0.069)
β_j	0.863	0.842	0.844	0.931	0.912	0.919
	(0.045)	(0.056)	(0.055)	(0.059)	(0.078)	(0.076)
Observations	1200	1200	1200	1176	1176	1176
Participants	100	100	100	98	98	98
$H_0(\hat{\delta}_j = 1)$	$p = 0.388$	$p = 0.464$	$p = 0.692$	$p = 0.850$	$p = 0.901$	$p = 0.623$
$H_0(\hat{\beta}_j = 1)$	$p = 0.003$	$p = 0.005$	$p = 0.005$	$p = 0.245$	$p = 0.259$	$p = 0.282$

Note: The table reports the parameter estimates for the choices made in blocks Self (left panel) and Other (right panel), respectively. Columns (1) and (4) use the log-linearized first order condition, while the other columns use the closed form solution for the number of tasks allocated to the sooner date. Standard errors are clustered at the individual level and calculated via the delta method.

Table E3: Parameter estimates for blocks Self and Other (Original Study)

	Self ($j = s$)			Other ($j = o$)		
	(1)	(2)	(3)	(4)	(5)	(6)
	FOC	CFS	CFS	FOC	CFS	CFS
	$\omega = 10$	$\omega = 10$	$\omega = 0$	$\omega = 10$	$\omega = 10$	$\omega = 0$
γ_j	1.769	1.859	1.521	1.831	1.948	1.576
	(0.111)	(0.158)	(0.277)	(0.127)	(0.182)	(0.131)
δ_j	1.015	1.023	1.006	0.964	0.964	0.951
	(0.052)	(0.027)	(0.024)	(0.014)	(0.016)	(0.016)
β_j	0.946	0.946	0.951	0.984	0.989	0.990
	(0.029)	(0.031)	(0.029)	(0.024)	(0.025)	(0.021)
Observations	1380	1380	1380	1392	1392	1392
Participants	115	115	115	116	116	116
$H_0(\hat{\delta}_j = 1)$	$p = 0.550$	$p = 0.392$	$p = 0.796$	$p = 0.013$	$p = 0.026$	$p = 0.002$
$H_0(\hat{\beta}_j = 1)$	$p = 0.061$	$p = 0.080$	$p = 0.088$	$p = 0.500$	$p = 0.664$	$p = 0.653$

Note: The table reports the parameter estimates for the choices made in blocks Self (left panel) and Other (right panel), respectively. Columns (1) and (4) use the log-linearized first order condition, while the other columns use the closed form solution for the number of tasks allocated to the sooner date. Standard errors are clustered at the individual level and calculated via the delta method.

Table E4: Parameter estimates for blocks Self and Other (Replication Study)

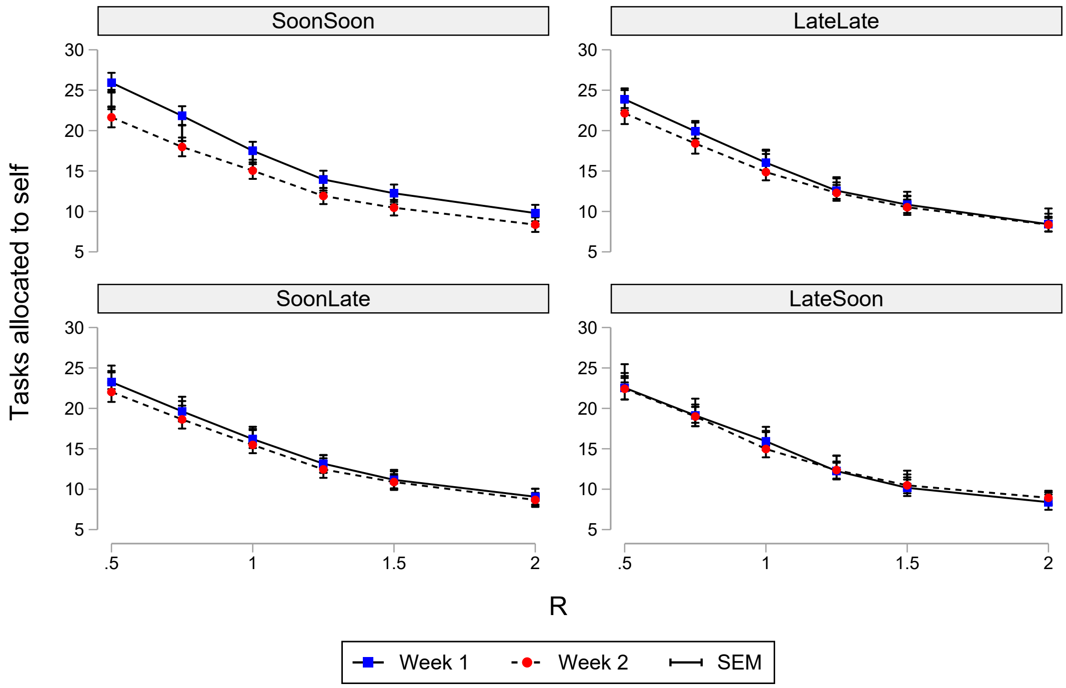


Figure E3: Effort allocations in interpersonal decisions in the original study ($n = 71$)

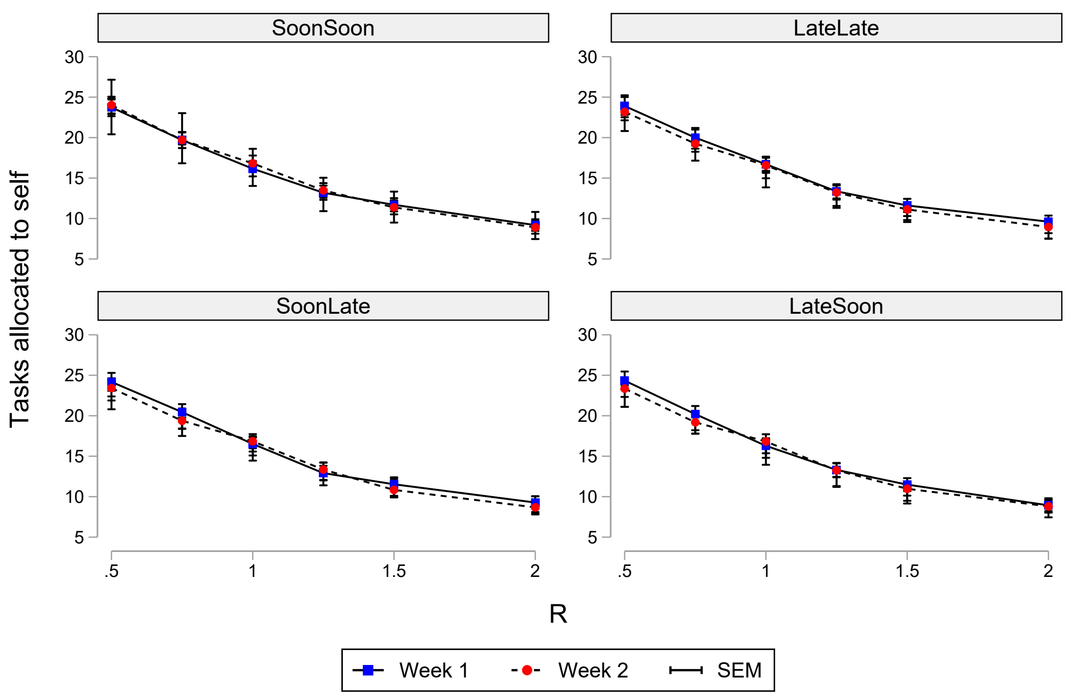


Figure E4: Effort allocations in interpersonal decisions in the replication study ($n = 94$)

Rate R	SoonSoon ($n = 71$) $\tau = 1$ Task self	$\tau = 2$ Task self	t-test	LateLate ($n = 71$) $\tau = 1$ Task self	$\tau = 2$ Task self	t-test	Diff-in-diff [t-test]
0.5	25.93	21.65	$p < 0.001$	23.87	22.14	$p = 0.035$	2.55
	(10.29)	(10.46)		(11.44)	(11.21)		[$p = 0.049$]
0.75	21.83	17.99	$p = 0.001$	19.82	18.41	$p = 0.028$	2.34
	(10.04)	(9.77)		(10.77)	(10.56)		[$p = 0.062$]
1	17.51	15.06	$p = 0.002$	16.04	14.87	$p = 0.084$	1.28
	(9.32)	(8.67)		(9.00)	(8.61)		[$p = 0.089$]
1.25	13.96	11.92	$p = 0.002$	12.58	12.30	$p = 0.626$	1.76
	(9.12)	(8.46)		(8.61)	(8.23)		[$p = 0.003$]
1.5	12.25	10.46	$p = 0.022$	10.85	10.52	$p = 0.580$	1.46
	(9.05)	(8.13)		(8.68)	(8.01)		[$p = 0.061$]
2	9.80	8.37	$p = 0.111$	8.42	8.37	$p = 0.915$	1.38
	(8.55)	(7.56)		(780)	(6.98)		[$p = 0.105$]
Overall	16.88	14.24	$p < 0.001$	15.28	14.43	$p = 0.094$	1.80
	(10.90)	(9.94)		(10.82)	(10.15)		[$p = 0.015$]

Note: The table denotes the number of tasks allocated to oneself, separately for block SoonSoon (left panel) and block LateLate (right panel). The p-values reported stem from t-tests with standard errors clustered at the individual level. The last column shows the difference-in-difference across week 1 and week 2 allocations between block SoonSoon and LateLate.

Table E5: Symmetric dictator games: Aggregate behavior by task rate (Original study)

Rate R	SoonSoon ($n = 94$) $\tau = 1$ Task self	$\tau = 2$ Task self	t-test	LateLate ($n = 94$) $\tau = 1$ Task self	$\tau = 2$ Task self	t-test	Diff-in-diff [t-test]
0.5	23.74	24.02	$p = 0.753$	23.90	23.18	$p = 0.312$	-1.00
	(10.66)	(10.00)		(10.77)	(10.12)		[$p = 0.399$]
0.75	19.70	19.70	$p = 1.000$	20.00	19.26	$p = 0.297$	-0.74
	(9.63)	(9.58)		(9.64)	(9.71)		[$p = 0.360$]
1	16.16	16.82	$p = 0.423$	16.71	16.57	$p = 0.827$	-0.80
	(9.20)	(9.37)		(9.08)	(9.03)		[$p = 0.844$]
1.25	13.19	13.48	$p = 0.664$	13.36	13.21	$p = 0.778$	-0.44
	(8.49)	(8.71)		(8.53)	(8.44)		[$p = 0.181$]
1.5	11.70	11.38	$p = 0.617$	11.62	11.13	$p = 0.315$	-0.17
	(7.93)	(8.38)		(7.92)	(7.97)		[$p = 0.190$]
2	9.19	8.91	$p = 0.635$	9.62	8.96	$p = 0.124$	-0.38
	(6.96)	(7.61)		(7.31)	(7.41)		[$p = 0.401$]
Overall	15.62	15.72	$p = 0.864$	15.87	15.38	$p = 0.319$	-0.59
	(10.13)	(10.29)		(10.18)	(10.05)		[$p = 0.089$]

Note: The table denotes the number of tasks allocated to oneself, separately for block SoonSoon (left panel) and block LateLate (right panel). The p-values reported stem from t-tests with standard errors clustered at the individual level. The last column shows the difference-in-difference across week 1 and week 2 allocations between block SoonSoon and LateLate.

Table E6: Symmetric dictator games: Aggregate behavior by task rate (Replication study)

Rate R	SoonLate ($n = 71$) $\tau = 1$ Task self	$\tau = 2$ Task self	t-test	LateSoon ($n = 71$) $\tau = 1$ Task self	$\tau = 2$ Task self	t-test
0.5	23.25	22.03	$p = 0.252$	22.55	22.42	$p = 0.884$
	(11.57)	(10.38)		(12.18)	(11.26)	
0.75	19.63	18.63	$p = 0.302$	19.13	19.00	$p = 0.874$
	(10.68)	(9.57)		(11.38)	(10.08)	
1	16.20	15.48	$p = 0.367$	15.93	14.97	$p = 0.213$
	(9.37)	(8.66)		(9.51)	(8.69)	
1.25	13.15	12.45	$p = 0.305$	12.24	12.37	$p = 0.864$
	(9.09)	(8.84)		(8.69)	(8.91)	
1.5	11.15	10.89	$p = 0.668$	10.17	10.48	$p = 0.647$
	(8.85)	(8.27)		(8.57)	(8.31)	
2	9.07	8.68	$p = 0.550$	8.39	8.93	$p = 0.424$
	(8.11)	(7.35)		(7.96)	(7.36)	
Overall	15.41	14.69	$p = 0.272$	14.73	14.69	$p = 0.945$
	(10.80)	(9.96)		(10.98)	(10.29)	

Note: The table denotes the number of tasks allocated to oneself, separately for block SoonLate (left panel) and block LateSoon (right panel). The p-values reported stem from t-tests with standard errors clustered at the individual level. Contrary to the table for the symmetric dictator games, here we do not report the difference-in-difference results because, as explained in Section 3 in the main text, they do not have a straightforward interpretation.

Table E7: Asymmetric dictator games: Aggregate behavior by task rate (Original study)

Rate R	SoonLate ($n = 94$) $\tau = 1$ Task self	$\tau = 2$ Task self	t-test	LateSoon ($n = 94$) $\tau = 1$ Task self	$\tau = 2$ Task self	t-test
0.5	24.18	23.41	$p = 0.329$	24.34	23.35	$p = 0.267$
	(10.78)	(9.97)		(10.90)	(9.93)	
0.75	20.45	19.38	$p = 0.141$	20.20	19.19	$p = 0.229$
	(9.57)	(9.26)		(9.68)	(9.52)	
1	16.52	16.85	$p = 0.680$	16.29	16.82	$p = 0.497$
	(9.15)	(8.46)		(8.96)	(8.64)	
1.25	12.91	13.35	$p = 0.525$	13.31	13.26	$p = 0.932$
	(8.77)	(8.15)		(8.35)	(8.39)	
1.5	11.53	10.84	$p = 0.277$	11.49	10.98	$p = 0.380$
	(8.34)	(8.07)		(7.80)	(8.24)	
2	9.28	8.68	$p = 0.294$	8.94	8.81	$p = 0.818$
	(7.64)	(7.25)		(6.90)	(7.57)	
Overall	15.81	15.42	$p = 0.509$	15.76	15.40	$p = 0.566$
	(10.44)	(9.91)		(10.26)	(10.02)	

Note: The table denotes the number of tasks allocated to oneself, separately for block SoonLate (left panel) and block LateSoon (right panel). The p-values reported stem from t-tests with standard errors clustered at the individual level. Contrary to the table for the symmetric dictator games, here we do not report the difference-in-difference results because, as explained in Section 3 in the main text, they do not have a straightforward interpretation.

Table E8: Asymmetric dictator games: Aggregate behavior by task rate (Replication study)

	Original study			Replication study		
	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$	(4) FOC $\omega = 10$	(5) CFS $\omega = 10$	(6) CFS $\omega = 0$
$\sigma = \frac{1}{\rho - 1}$	0.067	0.000	0.185	0.062	0.005	0.175
	(0.088)	(0.030)	(0.125)	(0.060)	(0.052)	(0.085)
$A = \left(\frac{1-a}{a}\right)^{\frac{1}{\rho-1}}$	0.486	0.509	0.365	0.529	0.558	0.425
	(0.038)	(0.038)	(0.045)	(0.038)	(0.037)	(0.045)
δ_s	1.048	1.046	1.056	0.980	0.982	0.976
	(0.031)	(0.031)	(0.041)	(0.022)	(0.021)	(0.027)
β_s	0.910	0.910	0.883	1.026	1.024	1.034
	(0.027)	(0.027)	(0.036)	(0.020)	(0.021)	(0.027)
δ_o	1.001	1.005	1.006	1.017	1.020	1.027
	(0.027)	(0.027)	(0.035)	(0.021)	(0.020)	(0.027)
β_o	1.044	1.043	1.060	0.990	0.983	0.977
	(0.040)	(0.040)	(0.053)	(0.018)	(0.018)	(0.024)
Observations	3408	3408	3408	4512	4512	4512
Participants	71	71	71	94	94	94
$H_0(\hat{\beta}_s = 1)$	$p < 0.001$	$p = 0.001$	$p = 0.001$	$p = 0.206$	$p = 0.239$	$p = 0.213$
$H_0(\hat{\beta}_o = 1)$	$p = 0.272$	$p = 0.276$	$p = 0.258$	$p = 0.585$	$p = 0.360$	$p = 0.348$
$H_0(\hat{\beta}_s = \hat{\beta}_o)$	$p = 0.013$	$p = 0.014$	$p = 0.015$	$p = 0.131$	$p = 0.090$	$p = 0.074$

Note: The table reports the parameter estimates from all dictator games (blocks SoonSoon, SoonLate, LateLate, and LateSoon). Columns (1) and (4) use the log-linearized first order condition, while columns (2), (3), (5), and (6) use the closed form solution for the number of tasks allocated to oneself. Standard errors are clustered at the individual level and calculated via the delta method.

Table E9: Parameter estimates from all dictator games

F Testing the robustness of the structural estimates

In this section, we report the results of a robustness check for our structural estimates based on the dictator game choices. In our theoretical framework we proposed a functional form which allows us to capture intertemporal social preferences (compare Section 3). When estimating time preferences for the dictator games, however, we made a simplifying assumption and constrained the curvature of the atemporal utility/cost-of-effort function to be linear. The upside of this assumption was that it allowed us to estimate time preferences for own and others' consumption for the interpersonal and the intrapersonal decisions in isolation, and, thus, test the stability of time preferences across these two decision contexts. The potential downside of this approach, however, is that by essentially neglecting the fact that there is substantial curvature in the cost-of-effort function – as is evident from the estimates for γ obtained above (compare Table 4 in the main text and Table A2 in Appendix A) – we may have produced biased estimates in the dictator games. To test for this possibility, in the following we provide the details and results from an estimation approach that estimates time preferences using data from all decision blocks, without imposing any linearity restriction on the utility function.

F.1 ***Details on the Structural Estimation***

FOC Approach. In order to estimate the general specification of time preferences in our dictator games accounting for convexity in the cost-of-effort function, we can derive the (log-linearized) first-order conditions for the interpersonal choices as follows:

Block SoonSoon:

$$\ln\left(\frac{s_{2,\tau}+\omega}{o_{2,\tau}+\omega}\right) = \ln(A) - \frac{1}{\gamma\rho-1}\ln(R) - \frac{\rho}{\gamma\rho-1}\Big[\ln\left(\frac{\beta_s\delta_s}{\beta_o\delta_o}\right)\mathbf{1}\{\tau=1\}\Big]$$

Block LateLate:

$$\ln\left(\frac{s_{3,\tau}+\omega}{o_{3,\tau}+\omega}\right) = \ln(A) - \frac{1}{\gamma\rho-1}\ln(R) - \frac{\rho}{\gamma\rho-1}\Big[\ln\left(\frac{\beta_s\delta_s^2}{\beta_o\delta_o^2}\right)\mathbf{1}\{\tau=1\} + \ln\left(\frac{\beta_s\delta_s}{\beta_o\delta_o}\right)\mathbf{1}\{\tau=2\}\Big]$$

Block SoonLate:

$$\ln\left(\frac{s_{2,\tau}+\omega}{o_{3,\tau}+\omega}\right) = \ln(A) - \frac{1}{\gamma\rho-1}\ln(R) - \frac{\rho}{\gamma\rho-1}\Big[\ln\left(\frac{\beta_s\delta_s}{\beta_o\delta_o^2}\right)\mathbf{1}\{\tau=1\} + \ln\left(\frac{1}{\beta_o\delta_o}\right)\mathbf{1}\{\tau=2\}\Big]$$

Block LateSoon:

$$\ln\left(\frac{s_{3,\tau}+\omega}{o_{2,\tau}+\omega}\right) = \ln(A) - \frac{1}{\gamma\rho-1}\ln(R) - \frac{\rho}{\gamma\rho-1}\Big[\ln\left(\frac{\beta_s\delta_s^2}{\beta_o\delta_o}\right)\mathbf{1}\{\tau=1\} + \ln\left(\frac{\beta_s\delta_s}{1}\right)\mathbf{1}\{\tau=2\}\Big]$$

The first-order conditions for the intrapersonal decisions, remain unchanged, and is given by (C.2). Using the same approach as in (C.6) and (C.3), we estimate the preference parameters via the following equation:

$$
\begin{aligned}
&\ln\left(x(B)\right)_i = \kappa_{0,s} CS_i + \kappa_{0,o} CO_i + \kappa_{1,s} DS_i + \kappa_{1,o} DO_i + \kappa_2 \ln(R)_i \times IA_i \\
&+ \lambda_0 IE_i + \lambda_1 D1_i + \lambda_2 D2_i + \lambda_3 D3_i + \lambda_4 D4_i + \lambda_5 D5_i + \lambda_6 D6_i + \lambda_7 \ln(R)_i \times IE_i + \varepsilon_i
\end{aligned}
\tag{F.1}
$$

where

$$
x(B) = \begin{cases} \frac{s_{2,\tau}+\omega}{s_{3,\tau}+\omega} & \text{if} \quad B = 1 \quad (\textsc{Self}) \\ \frac{o_{2,\tau}+\omega}{o_{3,\tau}+\omega} & \text{if} \quad B = 2 \quad (\textsc{Other}) \\ \frac{s_{t,\tau}+\omega}{o_{t,\tau}+\omega} & \text{if} \quad B \geq 3 \quad (\textsc{Dictator Games}) \end{cases}
$$

IA is a dummy variable indicating a decision from blocks 1 or 2 (intrapersonal decisions) and IE is a dummy variable indicating a decision is from blocks 3 to 6 (interpersonal decisions). All other independent variables are defined as before, and we impose the constraints from (C.7), as before. The estimates for the parameters of interest can be recovered from the coefficients as:

$$
\hat{\beta}_s^{Inter} = \exp\left(\frac{\hat{\lambda}_2 - \hat{\lambda}_1 - \hat{\lambda}_3 + \hat{\lambda}_5}{-\hat{\lambda}_7 + 1} \frac{\hat{\kappa}_2 - 1}{\hat{\kappa}_2}\right) \qquad \hat{\delta}_s^{Inter} = \exp\left(\frac{\hat{\lambda}_3 - \hat{\lambda}_2}{-\hat{\lambda}_7 + 1} \frac{\hat{\kappa}_2 - 1}{\hat{\kappa}_2}\right)
$$

$$
\hat{\beta}_o^{Inter} = \exp\left(\frac{\hat{\lambda}_1 - \hat{\lambda}_2 + \hat{\lambda}_4 - \hat{\lambda}_6}{-\hat{\lambda}_7 + 1} \frac{\hat{\kappa}_2 - 1}{\hat{\kappa}_2}\right) \qquad \hat{\delta}_o^{Inter} = \exp\left(\frac{\hat{\lambda}_2 - \hat{\lambda}_4}{-\hat{\lambda}_7 + 1} \frac{\hat{\kappa}_2 - 1}{\hat{\kappa}_2}\right)
$$

$$
\hat{\delta}_s^{Intra} = \exp\left(\frac{-\hat{\kappa}_{0,s}}{\hat{\kappa}_2}\right) \quad \hat{\beta}_s^{Intra} = \exp\left(\frac{-\hat{\kappa}_{1,s}}{\hat{\kappa}_2}\right) \quad \hat{\delta}_o^{Intra} = \exp\left(\frac{-\hat{\kappa}_{0,o}}{\hat{\kappa}_2}\right) \quad \hat{\beta}_o^{Intra} = \exp\left(\frac{-\hat{\kappa}_{1,o}}{\hat{\kappa}_2}\right)
$$

$$
\hat{\hat{A}} = \exp\left(\hat{\lambda}_0\right) \qquad \hat{\sigma} = \frac{\hat{\kappa}_2 \hat{\lambda}_7 - \hat{\lambda}_7}{\hat{\lambda}_7 - \hat{\kappa}_2} \qquad \hat{\gamma} = -\frac{1}{\hat{\kappa}_2} + 1
$$

CFS Approach. When using the closed-form solution approach instead, we can write the equivalent of equation (C.8) as:

$$\tilde{s}(B,\tau) = \frac{R^{-\xi-1}Z(B,\tau) + \omega\left(R^{-\xi}Z(B,\tau) - \tilde{A}^{-1}\right)}{\tilde{A}^{-1} + R^{-\xi-1}Z(B,\tau)} \equiv g^{Inter}(\omega, R, B, \tau; \tilde{A}, \sigma, \gamma, \beta_s, \delta_s, \beta_o, \delta_o) \tag{F.2}$$

Here, $\tilde{A} = \left(\frac{1-a}{a}\right)^{\frac{1}{\gamma\rho-1}}$ and $\xi = \frac{1}{\gamma\left(\frac{1}{\sigma}+1\right)-1}$. $Z(B,\tau)$ is given by:

$$Z(B,\tau) = \begin{cases} 1 & \text{if} \quad B=3 \quad \text{and} \quad \tau=2 \\ \left(\frac{\beta_o\delta_o}{\beta_s\delta_s}\right)^{\xi\left(\frac{1}{\sigma}+1\right)} & \text{if} \quad B=3 \quad \text{and} \quad \tau=1 \\ \left(\frac{\beta_o\delta_o}{\beta_s\delta_s}\right)^{\xi\left(\frac{1}{\sigma}+1\right)} & \text{if} \quad B=4 \quad \text{and} \quad \tau=2 \\ \left(\frac{\beta_o\delta_o^2}{\beta_s\delta_s^2}\right)^{\xi\left(\frac{1}{\sigma}+1\right)} & \text{if} \quad B=4 \quad \text{and} \quad \tau=1 \\ \left(\frac{\beta_o\delta_o^2}{\beta_s\delta_s}\right)^{\xi\left(\frac{1}{\sigma}+1\right)} & \text{if} \quad B=5 \quad \text{and} \quad \tau=1 \\ (\beta_o\delta_o)^{\xi\left(\frac{1}{\sigma}+1\right)} & \text{if} \quad B=5 \quad \text{and} \quad \tau=2 \\ \left(\frac{\beta_o\delta_o}{\beta_s\delta_s^2}\right)^{\xi\left(\frac{1}{\sigma}+1\right)} & \text{if} \quad B=6 \quad \text{and} \quad \tau=1 \\ \left(\frac{1}{\beta_s\delta_s}\right)^{\xi\left(\frac{1}{\sigma}+1\right)} & \text{if} \quad B=6 \quad \text{and} \quad \tau=2 \end{cases}$$

For blocks $B=1$ (Self) and $B=2$ (Other), the specification remains unchanged and $g^{Intra}(\boldsymbol{\theta})$ is defined as in equation (C.5). The overall likelihood contribution for decision i is then given by:

$$L_i = \begin{cases} \left[\Phi\left(\frac{0-g_i^{Intra}(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{\tilde{c}_2^i=0\}} \left[\phi\left(\frac{\tilde{c}_{2,\tau}^i-g_i^{Intra}(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{0<\tilde{c}_2^i<1\}} \left[\Phi\left(\frac{1-g_i^{Intra}(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{\tilde{c}_2^i=1\}} & \text{if} \quad B<3 \\ \left[\Phi\left(\frac{0-g_i^{Inter}(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{\tilde{s}^i=0\}} \left[\phi\left(\frac{\tilde{s}_{t,\tau}^i-g_i^{Inter}(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{0<\tilde{s}^i<1\}} \left[\Phi\left(\frac{1-g_i^{Inter}(\boldsymbol{\theta})}{\sigma}\right)\right]^{\mathbf{1}\{\tilde{s}^i=1\}} & \text{if} \quad B\geq 3 \end{cases}$$

F.2 ***Results***

The results from this estimation are shown in Table F1. As can be seen, the results echo the ones reported in the main text, indicating that any bias from considering the two types of decisions separately is negligible. In particular, for the intrapersonal choices we find a significant present bias for own consumption but not for the consumption of others; the estimates for β_s^{Intra} range between 0.891 and 0.903 (all $p < 0.014$) and the ones for β_o^{Intra} range between 0.989 and 0.995 (all $p > 0.703$). The difference between β_s^{Intra} and β_o^{Intra} is thereby statistically significant in all cases (all $p < 0.032$). For the interpersonal choices, in

	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$
$\sigma = \frac{1}{\rho - 1}$	0.163	0.006	0.408
	(0.123)	(0.109)	(0.166)
$\tilde{A} = \left(\frac{1-a}{a}\right)^{\frac{1}{\gamma\rho - 1}}$	0.525	0.552	0.417
	(0.028)	(0.027)	(0.032)
γ	2.254	2.552	2.007
	(0.177)	(0.259)	(0.181)
δ_s^{Inter}	0.998	0.998	0.995
	(0.037)	(0.041)	(0.042)
δ_s^{Intra}	1.027	1.027	1.005
	(0.035)	(0.040)	(0.038)
β_s^{Inter}	0.979	0.974	0.978
	(0.034)	(0.039)	(0.040)
β_s^{Intra}	0.903	0.891	0.895
	(0.037)	(0.043)	(0.042)
δ_o^{Inter}	1.009	1.018	1.019
	(0.035)	(0.039)	(0.040)
δ_o^{Intra}	0.960	0.952	0.934
	(0.022)	(0.025)	(0.024)
β_o^{Inter}	1.028	1.025	1.027
	(0.043)	(0.049)	(0.050)
β_o^{Intra}	0.989	0.995	0.994
	(0.029)	(0.034)	(0.031)
Observations	11376	11376	11376
Participants	158	158	158
$H_0(\hat{\beta}_s^{Inter} = 1)$	$p = 0.526$	$p = 0.502$	$p = 0.586$
$H_0(\hat{\beta}_s^{Intra} = 1)$	$p = 0.009$	$p = 0.012$	$p = 0.013$
$H_0(\hat{\beta}_o^{Inter} = 1)$	$p = 0.522$	$p = 0.609$	$p = 0.589$
$H_0(\hat{\beta}_o^{Intra} = 1)$	$p = 0.704$	$p = 0.874$	$p = 0.857$
$H_0(\hat{\beta}_s^{Intra} = \hat{\beta}_o^{Intra})$	$p = 0.003$	$p = 0.029$	$p = 0.031$
$H_0(\hat{\beta}_s^{Inter} = \hat{\beta}_o^{Inter})$	$p = 0.394$	$p = 0.444$	$p = 0.478$
$H_0(\hat{\beta}_s^{Inter} = \hat{\beta}_s^{Intra})$	$p = 0.095$	$p = 0.109$	$p = 0.110$
$H_0(\hat{\beta}_o^{Inter} = \hat{\beta}_o^{Intra})$	$p = 0.377$	$p = 0.544$	$p = 0.509$

Note: The table reports the parameter estimates from all the blocks, using the utility specification introduced in equation (3). We use data from those subjects who have sufficient variation in block SELF and block OTHER, as well as in the dictator game choices). Column (1) uses the approach via the log-linearized first order condition, all others use the closed form solution. Standard errors are clustered at the individual level and calculated via the delta method.

Table F1: Parameter estimates from all blocks

contrast, we find no evidence for a significant present bias, neither for own nor for others consumption (all $p > 0.501$). In line with the results reported in the main text, the difference between β_s^{Inter} and β_o^{Inter} is not statistically significant (all $p > 0.393$).

G Additional Robustness Checks

	Self ($j = s$)			Other ($j = o$)		
	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$	(4) FOC $\omega = 10$	(5) CFS $\omega = 10$	(6) CFS $\omega = 0$
γ_j	2.228	2.544	2.005	2.252	2.569	2.017
	(0.183)	(0.273)	(0.191)	(0.193)	(0.285)	(0.198)
δ_j	1.037	1.043	1.019	0.958	0.950	0.933
	(0.037)	(0.043)	(0.040)	(0.022)	(0.026)	(0.024)
β_j	0.901	0.889	0.892	0.987	0.992	0.992
	(0.037)	(0.044)	(0.042)	(0.029)	(0.033)	(0.031)
Observations	1932	1932	1932	1932	1932	1932
Participants	161	161	161	161	161	161
$H_0(\hat{\delta}_j = 1)$	$p = 0.319$	$p = 0.320$	$p = 0.631$	$p = 0.055$	$p = 0.055$	$p = 0.006$
$H_0(\hat{\beta}_j = 1)$	$p = 0.007$	$p = 0.011$	$p = 0.011$	$p = 0.651$	$p = 0.814$	$p = 0.801$

Note: The table reports the parameter estimates for the choices made in blocks Self (left panel) and Other (right panel), respectively. Columns (1) and (4) use the log-linearized first order condition, while the other columns use the closed form solution for the number of tasks allocated to the sooner date. Standard errors are clustered at the individual level and calculated via the delta method.

Table G1: Parameter estimates for blocks Self and Other, excluding types with too little variation in the interpersonal choices

	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$
γ	2.227	2.533	1.994
	(0.183)	(0.269)	(0.188)
δ_s	1.025	1.027	1.005
	(0.035)	(0.040)	(0.038)
β_s	0.904	0.892	0.896
	(0.037)	(0.043)	(0.042)
δ_o	0.959	0.952	0.934
	(0.021)	(0.025)	(0.024)
β_o	0.989	0.995	0.994
	(0.029)	(0.033)	(0.031)
Observations	3792	3792	3792
Participants	158	158	158
$H_0(\hat{\beta}_s = 1)$	$p = 0.009$	$p = 0.013$	$p = 0.013$
$H_0(\hat{\beta}_o = 1)$	$p = 0.698$	$p = 0.870$	$p = 0.853$
$H_0(\hat{\beta}_s = \hat{\beta}_o)$	$p = 0.033$	$p = 0.030$	$p = 0.032$

Note: The table reports the parameter estimates for the choices made in blocks Self and Other under the restriction that $\gamma_s = \gamma_o = \gamma$. Column (1) uses the log-linearized first order condition, while the columns (2) and (3) use the closed form solution for the number of tasks allocated to the sooner date. Standard errors are clustered at the individual level and calculated via the delta method.

Table G2: Parameter estimates for blocks Self and Other combined, excluding types with too little variation in the interpersonal choices

	Self ($j = s$)			Other ($j = o$)		
	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$	(4) FOC $\omega = 10$	(5) CFS $\omega = 10$	(6) CFS $\omega = 0$
γ_j	1.959	2.153	1.727	2.152	2.463	1.940
	(0.112)	(0.167)	(0.117)	(0.168)	(0.266)	(0.186)
δ_j	0.998	0.999	0.982	0.963	0.959	0.941
	(0.031)	(0.034)	(0.031)	(0.025)	(0.029)	(0.028)
β_j	0.920	0.915	0.917	0.977	0.978	0.981
	(0.032)	(0.035)	(0.033)	(0.034)	(0.039)	(0.037)
Observations	2676	2676	2676	2676	2676	2676
Participants	223	223	223	223	223	223
$H_0(\hat{\delta}_j = 1)$	$p = 0.940$	$p = 0.987$	$p = 0.559$	$p = 0.145$	$p = 0.164$	$p = 0.033$
$H_0(\hat{\beta}_j = 1)$	$p = 0.011$	$p = 0.014$	$p = 0.013$	$p = 0.502$	$p = 0.578$	$p = 0.618$

Note: The table reports the parameter estimates for the choices made in blocks Self (left panel) and Other (right panel), respectively. Columns (1) and (4) use the log-linearized first order condition, while the other columns use the closed form solution for the number of tasks allocated to the sooner date. Standard errors are clustered at the individual level and calculated via the delta method.

Table G3: Parameter estimates for blocks Self and Other using data from all participants

	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$
γ	2.047	2.291	1.822
	(0.130)	(0.199)	(0.140)
δ_s	0.992	0.987	0.973
	(0.033)	(0.036)	(0.033)
β_s	0.913	0.906	0.910
	(0.035)	(0.038)	(0.037)
δ_o	0.971	0.975	0.954
	(0.023)	(0.028)	(0.026)
β_o	0.979	0.981	0.984
	(0.031)	(0.035)	(0.034)
Observations	5352	5352	5352
Participants	223	223	223
$H_0(\hat{\beta}_s = 1)$	$p = 0.012$	$p = 0.014$	$p = 0.014$
$H_0(\hat{\beta}_o = 1)$	$p = 0.502$	$p = 0.581$	$p = 0.624$
$H_0(\hat{\beta}_s = \hat{\beta}_o)$	$p = 0.122$	$p = 0.122$	$p = 0.109$

Note: The table reports the parameter estimates for the choices made in blocks Self and Other under the restriction that $\gamma_s = \gamma_o = \gamma$. Column (1) uses the log-linearized first order condition, while the columns (2) and (3) use the closed form solution for the number of tasks allocated to the sooner date. Standard errors are clustered at the individual level and calculated via the delta method.

Table G4: Parameter estimates for blocks Self and Other combined using data from all participants

	(1) FOC $\omega = 10$	(2) CFS $\omega = 10$	(3) CFS $\omega = 0$
$\sigma = \frac{1}{\rho - 1}$	0.037	0.000	0.227
	(0.050)	(.)	(0.080)
$A = \left(\frac{1-a}{a}\right)^{\frac{1}{\rho-1}}$	0.442	0.473	0.324
	(0.027)	(0.028)	(0.032)
δ_s	1.008	1.008	1.008
	(0.019)	(0.019)	(0.024)
β_s	0.981	0.980	0.977
	(0.018)	(0.018)	(0.024)
δ_o	1.013	1.016	1.021
	(0.018)	(0.018)	(0.023)
β_o	1.003	0.999	0.998
	(0.020)	(0.020)	(0.026)
Observations	8928	8928	8928
Participants	186	186	186
$H_0(\hat{\beta}_s = 1)$	$p = 0.284$	$p = 0.271$	$p = 0.335$
$H_0(\hat{\beta}_o = 1)$	$p = 0.895$	$p = 0.967$	$p = 0.936$
$H_0(\hat{\beta}_s = \hat{\beta}_o)$	$p = 0.450$	$p = 0.514$	$p = 0.586$

Note: The table reports the parameter estimates from all dictator games (blocks SoonSoon, SoonLate, LateLate, and LateSoon). Column (1) uses the log-linearized first order condition, while columns (2) and (3) use the closed form solution for the number of tasks allocated to oneself. Standard errors are clustered at the individual level and calculated via the delta method.

Table G5: Parameter estimates from all dictator games, excluding only those participants who behave completely selfish in both weeks

H Experimental Instructions

H.1 Experimental Instructions (Week 1)

Welcome to our Experiment!

Prerequisites for participation

In order to participate in this study, you must be able to attend three laboratory sessions in three consecutive weeks. These sessions always take place at the same day of the week and the same time of the day. In the following, we refer to these three sessions as week 1 (today), week 2 (next week) and week 3 (the week after next). The average duration of the sessions is about one hour but may vary between 15 and 90 minutes. You also have to be willing to receive your total payment as a one-time payment at the end of the third experimental session. If you are not able to meet one or more of these requirements, please raise your hand now. In that case, you unfortunately cannot participate in our study.

Anonymity

Your anonymity in this study is guaranteed, i.e., no participant will learn about the identity of those who made a certain decision. Also, the experimenters will never connect your name with your decisions.

Rules of conduct

The results of this experiment will be used for a research project. It is therefore important that all participants follow certain rules of conduct. During the experiment, you are not allowed to communicate with other participants of the experiment or other people outside the laboratory. All mobile devices need to be switched off. In case you have any questions about the instructions or the study, please raise your hand at any time – we will answer your question individually at your desk. Non-compliance with these rules will lead to exclusion from the experiment and all payments.

Payment

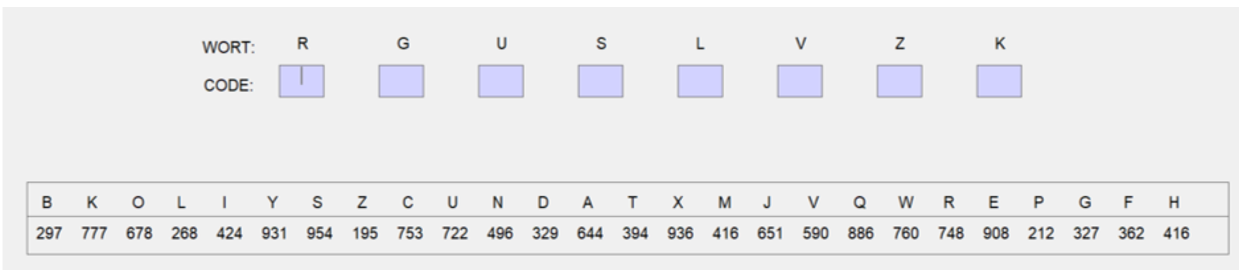
If you show up at all three experimental sessions, you will receive a completion payment of €40. The payments will be made in cash at the end of the third session. If you drop out earlier or fail to show up to one or more sessions, you will receive a compensation payment of €4. You have to collect this payment in cash at the end of the third session.

General description

Your task in this study is to solve a series of encryption tasks. You have to work on these encryption tasks in each of the three sessions. How many tasks you have to solve in each session depends on your decisions and chance. Yet, in each of the three laboratory sessions you have to solve a minimum requirement of tasks. It is therefore necessary that you show up for all three laboratory sessions.

Encryption task

The encryption task consists of a combination of eight letters (a "word"), which need to be converted into a number. For this, you will be shown a table with all letters of the alphabet, where each letter is assigned a three-digit number. Your task is to find for each letter of the displayed "word" the corresponding numeric code and to type it into the corresponding blue textbox. This becomes immediately clear by looking at the following screenshot:



In this example, you would have to type in the number 748 into the first textbox for "R". For the next textbox you would have to type the number 327 for "G", and for the third number 722 for "U", and so on. As soon as you enter the numbers for all letters correctly, a new word appears, again consisting of eight randomly generated letters. You will also see a new table in which the position of the letters is reshuffled. Furthermore, in this new table each letter is assigned a new randomly generated three-digit number.

<u>Incorrect Entries</u>: In case you make a mistake in one of the entries and press the "submit" button, a note will be shown on your screen. In this case, you have to encrypt the whole world again. However, the table of the letters and numbers stays the same in this case.

Information: To switch between the textboxes you may use the "Tab key" on the keyboard.

What you have to do in the three laboratory sessions

After you have become familiar with the encryption task, in the following we explain the details of the study.

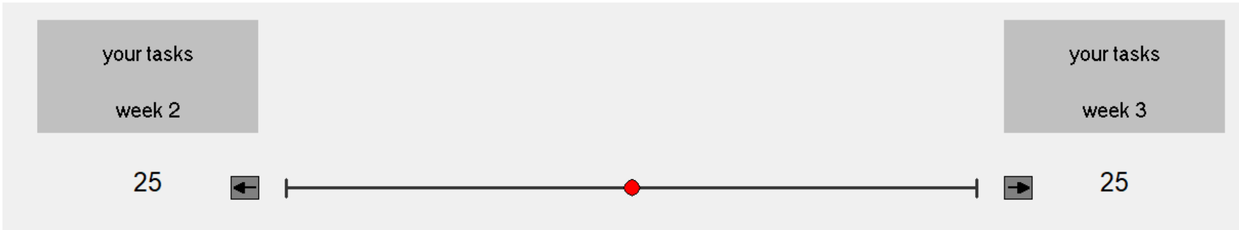
Important: At the beginning of each of the three laboratory sessions, every participant has to correctly solve a minimum number of 10 encryption tasks.

Week 1 (today)

After all participants have correctly solved the minimum requirement of encryption tasks, the task of today's laboratory session is to make a **series of allocation decisions** for different situations. In each decision, you have to decide about the **allocation of a certain number of encryption tasks**. There are **six different situations** in total. They differ in terms of who is affected by the allocation of the tasks. Some decisions affect **only you**, other decisions affect **only another person**, and in some decisions both you and the other person are affected. In week 2 one of these situations will be randomly selected (for more on this see below). This choice determines how many tasks you and the other person have to solve in week 2 and week 3. The different situations are classified into six blocks. The blocks are as follows:

> **Block 1**: In block 1 you have to decide how many tasks **you** want to solve in **week 2** and how many tasks **you** want to solve in **week 3**.
>
> **Block 2**: In block 2 you have to decide how many tasks **another randomly selected participant** needs to solve in **week 2** and how many tasks **this person** needs to solve in **week 3**.
>
> **Block 3**: In block 3 you have to decide how many tasks **you** want to solve in **week 2** and how many tasks **another randomly selected participant** needs to solve in **week 2**.
>
> **Block 4**: In block 4 you have to decide how many tasks **you** want to solve in **week 3** and how many tasks **another randomly selected participan**t needs to solve in **week 3**.
>
> **Block 5**: In block 5 you have to decide how many tasks **you** want to solve in **week 2** and how many tasks **another randomly selected participant** needs to solve in **week 3**.
>
> **Block 6**: In block 6 you have to decide how many tasks **you** want to solve in **week 3** and how many tasks **another randomly selected participant** needs to solve in **week 2**.

For each decision there will be shown a slider on the screen, which you can use to allocate the tasks. For example, in block 1 the screen looks as follows:



You can move the slider along the black line to the left or to the right, either by a mouse click or by clicking on the arrow keys next to it. In this example, the left and right numbers next to the slider show how many tasks you want to solve in week 2 and how many you want so solve in week 3. Please note that these numbers do not include the 10 tasks every participant needs to complete at the beginning of each laboratory session.

Decisions within a block

Within each block, you have to make a total of six decisions. The decisions differ in terms of **the rate by which you can allocate tasks between week 2 and week 3 or between yourself and another randomly selected participant**. There are six rates in total: 1:0.5; 1:0.75; 1:1; 1:1.25; 1:1.5 and 1:2.

To illustrate these task rates, we stick to the example of block 1. As shown in the screenshot above, you can place the slider further to the left to complete more tasks in week 2, or further to the right to complete more tasks in week 3. The task rate defines by how much the number of tasks you need to solve in week 2 is reduced when you move the slider further to the right:

- A rate of 1:1 means that every task you solve in week 3 reduces the number of tasks you need to solve in week 2 by 1.
- A rate of 1:2 means, that every task you solve in week 3 reduces the number of tasks you need to solve in week 2 by 2.

Now take as an example block 5, in which you have to decide how many tasks **you** want to solve in **week 2** and how many tasks **one other randomly selected participant** needs to solve in **week 3**. In this case the task rate defines by how much the number of tasks you need to solve in week 2 is reduced when you move the slider further to the right:

- A rate of 1:1 then means that every task another person needs to solve in week 3 reduces the number of the tasks you need to solve in week 2 by 1.
- A rate of 1:0.5 then means that every task another person needs to solve in week 3 reduces the number of the tasks you need to solve in week 2 by 0.5.

The same logic applies for the other blocks and rates. You have the opportunity to familiarize yourself with the slider and the different task rates at the beginning of the experiment. We will also ask you some control questions to ensure that the procedure is clear for everybody. After that, you will make your decisions. The six blocks will be shown to you in a random order. After all participants have made their decisions, a brief questionnaire will follow. After that, the laboratory session for week 1 is over.

Week 2 (one week from today)

The second laboratory session will take place in exactly one week at the same weekday and at the same time of the day, here in the laboratory. We will send you an email reminder about the dates beforehand. Please bring along the card you drew at the beginning of today's session. On the back of the card you will see once more the dates of the second and third laboratory session. The procedure of week 2 is as follows:

- At the beginning of the second laboratory session, you first have to correctly solve the minimum requirement of 10 encryption tasks.
- After that you will be again asked to make a series of allocation decisions, as in week 1.
- Then, one decision, either from week 1 or week 2, will be randomly implemented. In the following, we will refer to the randomly chosen decision as the "allocation that counts". Below, we will explain how exactly the "allocation that counts" is chosen.
- After that, you will be informed on screen how many tasks you need to solve based on the "allocation that counts". Subsequently, you have to correctly solve the number of tasks allocated to you for week 2.
- Once you have solved all tasks correctly, you may leave your desk and the laboratory. That is, you do not have to wait until all participants have finished solving their allocated tasks. If you leave the laboratory before you have solved all tasks correctly, this will count as dropping out of the study and you only receive a compensation payment of €4.

Week 3 (two weeks from today)

The third (and last) laboratory session will take place exactly in two weeks at the same week day at the same time of the day, here in the laboratory. We will send you an email reminder about the dates beforehand. Please bring along the card you drew at the beginning of today's session. The procedure of week 2 is as follows:

- At the beginning of the third laboratory session you first have to correctly solve the minimum requirement of 10 encryption tasks.
- Then, you have to correctly solve the number of tasks that have been allocated to you based on the "allocation that counts" for week 3.
- Once you have solved all the tasks correctly, we will come to your desk and you will receive your completion payment of €40. Then, you may leave your desk and the laboratory. That is, you do not have to wait until all participants have finished solving their allocated tasks. If you leave the laboratory before you have solved all tasks, this will count as dropping out of the study and you only receive a compensation of €4.

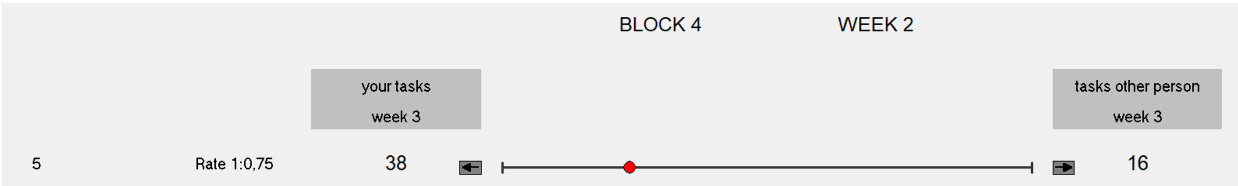
Determination of the "Allocation That Counts"

In week 1 and week 2 you will make a total of 36 allocation decisions, respectively. After having made all decisions, one decision will be randomly chosen in week 2. This "allocation that counts" defines how many tasks you and one other person need to solve in week 2 and week 3. The determination of the "allocation that counts" is as follows:

1. First, we divide all participants into two groups, red and blue. To do this, each participant has to draw a colored card from a bag. The bag contains the same number of red and blue cards.
2. Then, each blue participant will be randomly allocated to a red participant. The decisions of the red participants determine how many tasks the red and the blue participant need to solve in week 2 and week 3.
3. First, it will be randomly and with equal probability determined, whether red's decisions from week 1 or from week 2 will be relevant.
4. After that, it will determined which decision within the randomly chosen week will be relevant. To this end, first one of the six blocks will be randomly selected with equal probabilities. Then, one of the 6 decisions within the selected block will be chosen randomly and with equal probabilities.

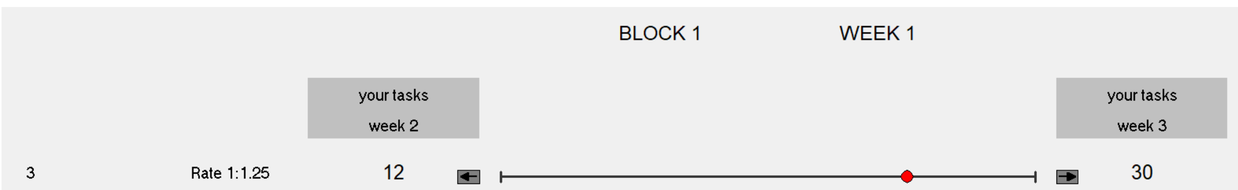
This decision will then be the "allocation that counts". The allocation of encryption tasks from this decision then determines how many tasks you and the other person need to solve in week 2 and week 3. This procedure ensures that every decision has the same probability to be chosen. Furthermore, it means, that the "allocation that counts" may be different from participant to participant.

Example: For a red participant decision 5 from week 2 and block 4 is chosen:



This means that the red participant needs to solve 38 tasks in week 3 and the blue participant (the "other person") needs to solve 16 tasks in week 3, in addition to the minimum requirement of 10 tasks each. This further means that in week 2 the red and the blue participant only need to solve the minimum requirement of 10 tasks.

For another red participant decision 3 from week 1 and block 1 is chosen:



This means, that the red participant needs to solve 12 tasks in week 2 and 30 tasks in week 2, in addition to the minimum requirement of 10 tasks each. This further means that the blue participant only needs to solve the minimum requirement of 10 tasks in weeks 2 and 3.

Information: Please note that every decision you make may be the "allocation that counts". So please take every decision as if it would be the one determining your task.

Summary

- You are participating in a two-week study with a total of three laboratory sessions.
- If you show up to all sessions and solve all your tasks, you will receive a completion payment of €40 in cash at the end of the third session.
- If you miss one or more laboratory sessions and/or fail to solve all your tasks, you will receive a compensation payment of €4, which you have to collect in cash at the end of the third session.
- You make various allocation decisions in week 1 and week 2. In week 2, one decision will be randomly chosen and implemented. This "allocation that counts" determines how many tasks you and one other person need to solve in week 2 and week 3.

- All participants need to solve a minimum requirement of 10 tasks independently from "the allocation that counts" in each of the three weeks.
- As soon as you solved your tasks in week 2 and week 3, you may leave the laboratory. You do not have to wait for the other participants.

In case you have any question please raise your hand. We will answer your question in private at your desk. After that, the experiment starts with the test screen and the control questions.

H.2 Experimental Instructions (Week 2)

Welcome to our experiment!

Reminder:

Today's laboratory session is the second ("week 2") of three sessions in total. The third session takes place in exactly one week ("week 3"), at the same week day and at the same time of the day. If you also show up then and finish the study, you receive your full payment of €40.

What you have to do today

Today's session consists of three parts:

- First, you need to solve the minimum requirement of 10 encryption tasks, which you already know from the previous week.
- Afterwards, you again need to make a series of allocation decisions for different situations. In each decision, you have to decide about the allocation of a certain number of encryption tasks. There are six different situations, exactly like in week 1. They differ in terms of who is affected by the allocation of the tasks. Some decisions affect only you, other decisions affect only another person, and in some decisions both you and the other person are affected.
- Then, one decision, either from last week ("week 1") or from today ("week 2"), will be randomly chosen as the "allocation that counts". This decision then determines how many tasks you need to solve today and next week. As a reminder, we will again explain below how exactly the "allocation that counts" is chosen.

Once you have solved all tasks correctly, you may leave your desk and the laboratory. **That is, you do not have to wait until all participants have finished solving their allocated tasks**. If you leave the laboratory before you have solved all tasks, this will count as dropping out of the study and you only receive a compensation payment of €4.

You have the opportunity to familiarize yourself with the slider and the different task rates at the beginning of the experiment. We will also ask you again some control questions to ensure that the procedure is clear for everybody.

Determination of the "Allocation That Counts"

1. First, we divide all participants into two groups, red and blue. To do this, each participant has to draw a colored card from a bag. The bag contains the same number of red and blue cards.

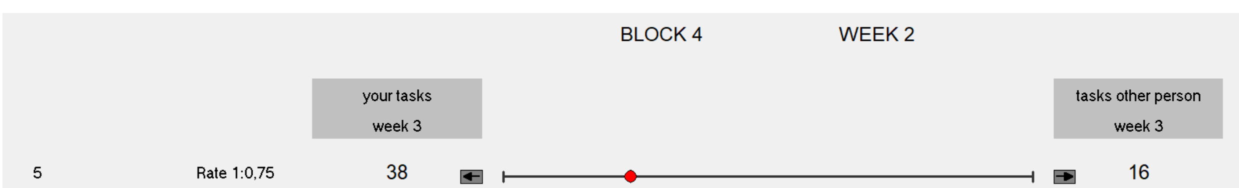
2. Then, each blue participant will be randomly allocated to a red participant. The decisions of the red participants determine how many tasks the red and the blue participant need to solve in week 2 and week 3.

3. First, it will be randomly and with equal probability determined, whether red's decisions from week 1 or from week 2 will be relevant.

4. After that, it will determined which decision within the randomly chosen week will be relevant. To this end, first one of the six blocks will be randomly selected with equal probabilities. Then, one of the 6 decisions within the selected block will be chosen randomly and with equal probabilities.

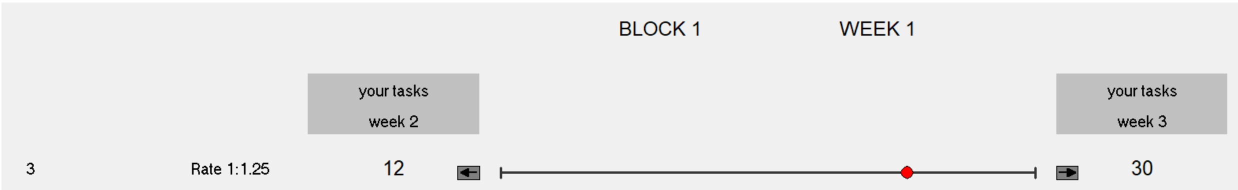
This decision will then be the "allocation that counts". The allocation of encryption tasks from this decision then determines how many tasks you and the other person need to solve today (week 2) and next week (week 3). This procedure ensures that every decision has the same probability to be chosen. Furthermore, it means, that the "allocation that counts" may be different from participant to participant.

Example 1: For a red participant decision 5 from week 2 and block 4 is chosen:



This means that the red participant needs to solve 38 tasks in week 3 and the blue participant (the "other person") needs to solve 16 tasks in week 3, in addition to the minimum requirement of 10 tasks each. This further means that today the red and the blue participant only need to solve the minimum requirement of 10 tasks.

Example 2: For another red participant decision 3 from week 1 and block 1 is chosen:



This means, that the red participant needs to solve 12 tasks today and 30 tasks in week 2, in addition to the minimum requirement of 10 tasks each. This further means that the blue participant only needs to solve the minimum requirement of 10 tasks today and in week 3.

Information: Please note that every decision you make may be the "allocation that counts". So please take every decision as if it would be the one determining your task.

H.3 Experimental Instructions (Week 3)

Welcome to our experiment!

Reminder:

Today's laboratory session is the third ("week 3") of three sessions in total.

What you have to do today

Today's session consists of two parts:

- First, you need to solve the minimum requirement of 10 encryption tasks, which you already know from the previous two weeks.
- Afterwards you need to solve the numbers of tasks that have been allocated to you based on the "**allocation that counts**".

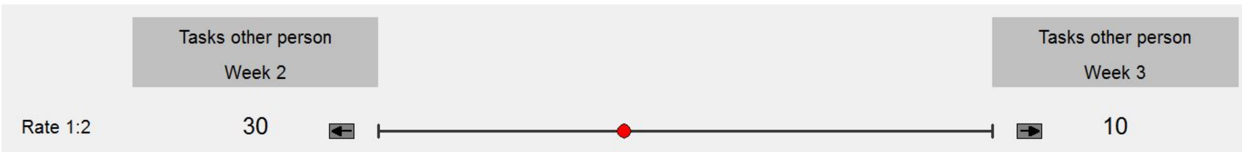
Once you have solved all the tasks, we will come to your desk and you will receive your completion payment of €40. Then, you may leave your desk and the laboratory. **That is, you do not have to wait until all participants have finished solving their allocated tasks**. If you leave the laboratory before you have solved all tasks, this will count as dropping out of the study. In this case you will only receive a compensation payment of €4.

H.4 Control Questions

Here we provide the control questions that participants were asked before they made decisions in weeks 1 and 2. The same questions were asked in both weeks. Numbers in brackets indicate the correct answer.

Example 1

Imagine that you were selected to be the red participant and that the following of your decisions was selected as the "decision that counts":



Please answer the following questions about the example given above:

- How many tasks do you need to solve in week 2 (in addition to the minimum work of 10 tasks)? *[0]*
- How many tasks does the other person need to solve in week 2 (in addition to the minimum work of 10 tasks)? *[30]*
- How many tasks do you need to solve in week 3 (in addition to the minimum work of 10 tasks)? *[30]*
- How many tasks does the other person need to solve in week 3 (in addition to the minimum work of 10 tasks)? *[10]*

Example 2

Imagine that you were selected to be the red participant and that the following of your decisions was selected as the "decision that counts":

Please answer the following questions about the example given above:

- How many tasks do you need to solve in week 2 (in addition to the minimum work of 10 tasks)? *[38]*

- How many tasks does the other person need to solve in week 2 (in addition to the minimum work of 10 tasks)? *[16]*
- How many tasks do you need to solve in week 3 (in addition to the minimum work of 10 tasks)? *[0]*
- How many tasks does the other person need to solve in week 3 (in addition to the minimum work of 10 tasks)? *[0]*

Example 3

Imagine that you were selected to be the red participant and that the following of your decisions was selected as the "decision that counts":

Please answer the following questions about the example given above:

- How many tasks do you need to solve in week 2 (in addition to the minimum work of 10 tasks)? *[0]*
- How many tasks does the other person need to solve in week 2 (in addition to the minimum work of 10 tasks)? *[28]*
- How many tasks do you need to solve in week 3 (in addition to the minimum work of 10 tasks)? *[15]*
- How many tasks does the other person need to solve in week 3 (in addition to the minimum work of 10 tasks)? *[0]*

Please answer the following questions to make sure you understand all the procedures of the experiment.

If within a session you correctly solved all tasks that were assigned to you, then

- the experiment for this week is over and you are allowed to leave the laboratory. *[✓]*
- you have to wait until all participants have finished their tasks.

If you show up to all laboratory sessions and correctly solve all tasks that were assigned to you, then

- you earn €4.
- you earn €40. *[✓]*

References

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