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Concentration Bias in Intertemporal Choice

Abstract

Many intertemporal trade-offs are unbalanced: while the advantages of options are concentrated in a few periods, the disadvantages are dispersed over numerous periods. We provide novel experimental evidence for "concentration bias", the tendency to overweight advantages that are concentrated in time. Subjects commit to too much overtime work that is dispersed over multiple days in exchange for a bonus that is concentrated in time: concentration bias increases subjects' willingness to work by 22.4% beyond what standard discounting models could account for. In additional conditions and a complementary experiment involving monetary payments, we study the mechanisms behind concentration bias and demonstrate the robustness of our findings.

JEL-Codes: D010.

Keywords: attention, focusing, bounded rationality, intertemporal choice, future bias, present bias, framing.

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1 Introduction

Trade-offs in multiattribute choice are often unbalanced: the advantage of an option relative to alternatives is concentrated in fewer attributes, while its disadvantage is dispersed over numerous other attributes. In such unbalanced choices, the "focusing model" by Kőszegi and Szeidl (2013) posits that a decision maker focuses disproportionately on, and hence overweights, the advantages of options that are concentrated in fewer attributes. Consequently, the decision maker is too prone to choose options with concentrated advantages: she exhibits "concentration bias". While direct empirical evidence on concentration bias is lacking, potentially important theoretical implications of concentration bias have been identified in, for instance, industrial organisation (Dertwinkel-Kalt et al., 2019; Apffelstaedt and Mechtenberg, 2020), political economy (Gallice and Grillo, 2020; Nunnari and Zápal, 2020), bargaining (Canidio and Karle, 2021), and intertemporal choice (Kőszegi and Szeidl, 2013). In intertemporal choice, concentration bias implies systematic distortions between a decision maker's time preferences and her choices. Depending on the temporal position of a concentrated advantage, the decision maker may act either too patiently or too impatiently. For instance, the decision maker may overcommit to working overtime each day of the year in order to receive a large concentrated bonus at the end of the year. At the same time, she may be too prone to avoiding the concentrated hassle of going to the gym on one day at the cost of marginally poorer health on future days.

In this paper, we identify and quantify the causal effect of concentration bias on intertemporal choice based on two laboratory experiments. Our main experiment is preregistered and involves dated consumption events. We demonstrate that concentration bias can cause subjects to be too patient: subjects overcommit to dispersed effort provision in exchange for redeeming a concentrated restaurant voucher at a later point in time. In additional conditions of this experiment, we study the mechanisms behind concentration bias and demonstrate the robustness of our results. In our second experiment—which involves monetary payments—we corroborate these findings and add evidence that concentration bias can cause subjects to be too impatient as well.

In Section 2, we provide our main evidence for concentration bias. In the Main-Treatment condition of the consumption experiment, we provide a nonparametric measure of concentration bias on the individual level which allows us to account for heterogeneity in subjects' time preferences and their per-period utility functions. In a set of unbalanced choices, we elicit subjects' willingness to complete real-effort tasks on *multiple* workdays in exchange for a restaurant voucher. The voucher is valid on a *single* day, the "restaurant day", after the last workday. This trade-off is unbalanced because the real-effort tasks involve a utility cost that is dispersed over multiple workdays, while the restaurant voucher involves a subsequent utility benefit that is concentrated on the restaurant day. The focusing model predicts that subjects overweight the concentrated restaurant voucher and, thus, commit to completing more tasks than their preferences can account for. To identify this effect of concentration bias, we contrast subjects' unbalanced choices with balanced choices. The balanced choices consist of balanced trade-offs between real-effort tasks concentrated on a *single* workday and a restaurant voucher concentrated on the restaurant day. In these balanced trade-offs, focusing predicts that subjects' preferences entirely determine their intertemporal choices. The balanced choices thus provide a *within-subjects* benchmark of subjects' preferences. We find a statistically significant ($p < 0.001$) and large concentration bias in the unbalanced choices. Concentration bias causes subjects to be too patient: they are willing to complete 22.4% more tasks per workday than their preferences, as measured via the balanced choices, can account for.

In Section 3, we investigate the mechanisms behind concentration bias. Concentration bias could be caused by concentration in time—as the focusing model posits—or by a concentrated versus dispersed *framing* of utility outcomes. In Main-Treatment, the degree of temporal concentration and the corresponding type of framing coincide: additional work is not only dispersed over all workdays but it is also displayed in a dispersed framing, that is, per workday; the restaurant voucher is concentrated in time and also displayed in a concentrated framing, that is, its value is depicted in its entirety. If overweighting is at least partially caused by a concentrated framing, both temporal concentration and framing may cause the observed concentration bias. To disentangle these mechanisms, we designed the MECHANISM-TREATMENT condition. In comparison to MAIN-TREATMENT, we only change the framing of the restaurant voucher in MECHANISM-TREATMENT. If concentration bias were exclusively caused by concentration in time, the same degree of concentration bias would hence be predicted in both conditions. In MECHANISM-TREATMENT, we frame the restaurant voucher in a dispersed way. Instead of depicting the value in its entirety, we depict the value as the sum of multiple parts. If framing contributes to concentration bias, there should be a less pronounced concentration bias in MECHANISM-TREATMENT than in MAIN-TREATMENT. We find that both concentration in time and framing contribute to concentration bias. MECHANISM-TREATMENT yields a statistically significant concentration bias that is significantly smaller than the degree of concentration bias estimated in MAIN-TREATMENT. This provides evidence for both the assumption of the focusing model that generates the prediction of concentration bias and the unmodeled intuition of Kőszegi and Szeidl (2013, p. 68–69) that framing may additionally affect concentration bias.

In Section 4, we provide robustness checks. We show that noise in subjects' choices cannot explain our findings, and we replicate the evidence for concentration bias by using a charitable donation instead of the restaurant voucher. In addition, we discuss the main assumptions behind our TREATMENT conditions. We assume that subjects' preferences are stable over the course of the experiment (since our measurement of concentration bias is within-subjects) and that subjects anticipate that their per-workday disutility is independent of the number of tasks on other workdays. We discuss violations of these assumptions that would confound our identification of concentration bias and show that these violations are implausible.

In Section 5, we report further evidence for concentration bias. In our money experiment, we test concentration bias over monetary payments. We find that subjects overweight payments concentrated in time relative to payments dispersed over time. Again, both concentration in time and framing contribute to concentration bias. Depending on the temporal position of the concentrated payment, concentration bias causes subjects to be either too patient or too impatient. Finally, concentration bias is more pronounced for a greater degree of dispersion over time.

We conclude in Section 6 with a discussion of related models of attention-based decision making (Rubinstein, 2003; Bordalo et al., 2012; Bushong et al., 2021) and of potential policy implications of our findings.

We make the following additional contributions: (i) Recent lab studies find little evidence for present bias (e.g., Andreoni and Sprenger, 2012), while field studies typically find substantial present bias (e.g., DellaVigna and Malmendier, 2006; Paserman, 2008; Laibson et al., 2018). The use of monetary payments in the lab and real consumption in the field may partially explain this disparity (Augenblick et al., 2015). Concentration bias, however, may also contribute: choices have been exclusively balanced in the lab and often unbalanced in the field, with costs of present-biased behaviour being dispersed over many periods. For instance, not searching for a job prolongs unemployment for many more days. (ii) Our findings inform the literature on debt repayment. Kettle et al. (2016) find that the repayment of debt that is accumulated on multiple accounts is greater if repayment is framed to be account-wise than if it is framed as a uniform reduction across all accounts. While this finding is consistent with a motivation to break even, it also relates to our effects of a concentrated versus a dispersed framing. (iii) Goals often draw attention to productive actions. This may come at the cost of intangible adverse consequences, such as unethical behaviours (Ordóñez et al., 2009). Our results suggest that such adverse consequences could be made more tangible by increasing their degree of concentration (in time or framing). (iv) Concentration bias may contribute to the annuity puzzle. This puzzle refers to people's tendency to prefer a concentrated lump-sum payment over an annuity—a dispersed payment stream—even when the annuity has a substantially greater expected present value (e.g., Yaari, 1965; Davidoff et al., 2005). (v) More broadly, we contribute to the recent experimental literature on the bounded rationality of behavioural biases (e.g., Enke and Zimmermann, 2019; Esponda and Vespa, 2019; Frydman and Jin, 2020; Enke and Graeber, 2021).

2 Experimental Evidence for Concentration Bias

In *unbalanced* intertemporal trade-offs, the focusing model (Kőszegi and Szeidl, 2013) predicts concentration bias: distortions between a decision maker's preferences and her choices. In *balanced* trade-offs, by contrast, focusing predicts no distortions. We derive these predictions in Section 2.1 and explain how balanced and unbalanced trade-offs allow us to identify concentration bias in Section 2.2. In the MAIN-TREATMENT condition of our consumption experiment, we implement these trade-offs and find evidence for concentration bias (see sections 2.3 and 2.4).

2.1 The focusing model

2.1.1. Unbalanced trade-off. Consider a choice between two work plans. Each work plan $\boldsymbol{c}$ consists of consequences, c_t, on nine fixed future days, $t \in \{1, \ldots, 9\}$, that affect utility negatively (work) or positively (compensation). The baseline work plan $\boldsymbol{c}^{\mathrm{b}}$ consists of strictly positive numbers of real-effort tasks, $(e_1, \ldots, e_8)$, on eight workdays, $t \in \{1, \ldots, 8\}$, and a compensation. The compensation is a non-cashable and personalised restaurant voucher of value $v > 0$. The voucher is valid on the restaurant day at $t = 9$. The overtime work plan $\boldsymbol{c}^{\mathrm{o}}$ consists of a raise $R > 0$ in the value of the restaurant voucher in exchange for additional tasks on all eight workdays, $\tilde{\boldsymbol{x}} = (\tilde{x}_1, \ldots, \tilde{x}_8)$, with all $\tilde{x}_t > 0$. Let C be the choice set of this choice which includes only

$$
\begin{aligned}
\boldsymbol{c}^{\mathrm{b}} &= (-e_1, \quad -e_2, \quad -e_3, \quad \ldots, -e_8, \quad v) \quad \text{and} \\
\boldsymbol{c}^{\mathrm{o}} &= (-e_1 - \tilde{x}_1, -e_2 - \tilde{x}_2, -e_3 - \tilde{x}_3, \ldots, -e_8 - \tilde{x}_8, v + R).
\end{aligned}
$$

This choice implies an *unbalanced* trade-off: choosing either work plan amounts to an *unequal* number of periods with utility costs and utility benefits, relative to the rejected work plan. Choosing the overtime work plan $\boldsymbol{c}^{\mathrm{o}}$, for instance, involves the utility cost of completing additional tasks on all *eight* workdays and a utility benefit in the form of a more valuable voucher on the restaurant day. In short, while the utility cost is *dispersed over time*, the utility benefit is *concentrated in time*.

Consider a decision maker whose time preferences and per-period utility functions make her indifferent between the two work plans in the absence of focusing. Her discounted utility benefit of the raise R is equal to her discounted utility cost of the additional tasks $\tilde{\boldsymbol{x}}$:

$$
D(9)\,(u(v+R) - u(v)) = \sum_{t=1}^{8} D(t)\,(u(-e_t) - u(-e_t - \tilde{x}_t)), \tag{1}
$$

with discount factor $D(t)$ allowing for any (including exponential and hyperbolic) discounting. This assumes that aggregate utility $U(\boldsymbol{c})$ is additively separable in per-period utility $u(c_t)$.

According to the focusing model (Kőszegi and Szeidl, 2013), the decision maker does not choose by considering the discounted utilities of the available work plans, but by comparing the *focus-weighted* discounted utilities. Focusing augments the discounted utility with period-t focus weights $g_t(C)$ that scale discounted per-period utility $D(t)\,u(c_t)$:

$$\tilde{U}(\boldsymbol{c}, C) := \sum_{t=1}^{9} g_t(C)\, D(t)\, u(c_t). \tag{2}$$

The central assumption of focusing is that per-period utility differences attract attention: the greater the utility difference between alternatives is at a certain point in time, the greater is the decision weight that is assigned to the utility consequences at this point in time. Formally, focus weights g_t are generated by a function g that strictly increases in the difference between the maximum and the minimum utility in period t across all elements of the choice set C:

$$g_t(C) := g[\Delta_t(C)] \quad \text{with} \quad \Delta_t(C) := \max_{c \in C} D(t)\, u(c_t) - \min_{c \in C} D(t)\, u(c_t) \quad \text{and} \quad g'[\cdot] > 0. \tag{3}$$

In unbalanced trade-offs, alternatives with a concentrated utility benefit and a dispersed utility cost tend to "profit" from focus weighting relative to alternatives with a dispersed utility benefit and a concentrated utility cost. In the choice between the baseline work plan $\boldsymbol{c}^{\mathrm{b}}$ and the overtime work plan $\boldsymbol{c}^{\mathrm{o}}$, the latter profits from focus weighting because of its concentrated utility benefit (the raise on the restaurant day) and its dispersed utility cost (the additional tasks on eight workdays).

To illustrate this formally, note that the argument of g is the per-period utility difference between the two work plans. From the perspective of the overtime work plan $\boldsymbol{c}^{\mathrm{o}}$, the utility difference is the *per-workday* utility cost of additional work, $D(t)\,(u(-e_t) - u(-e_t - \tilde{x}_t))$, on each workday t and the utility benefit of the raise, $D(9)\,(u(v+R) - u(v))$, on the restaurant day. Since the decision maker's preferences satisfy indifference between $\boldsymbol{c}^{\mathrm{b}}$ and $\boldsymbol{c}^{\mathrm{o}}$, see equation (1), the utility difference on the restaurant day exceeds the utility difference on any individual workday. And, since g is strictly increasing, the focus weight assigned to the restaurant day is larger than for any workday:

$$g[D(9)\,(u(v+R) - u(v))] \;>\; g[D(t)\,(u(-e_t) - u(-e_t - \tilde{x}_t))], \quad t \in \{1, \ldots, 8\}. \tag{4}$$

Focus weighting hence leads to an overweighting of the utility benefit of choosing the overtime work plan $\boldsymbol{c}^{\mathrm{o}}$ relative to its utility cost:

$$D(9)\,(u(v+R) - u(v)) > \sum_{t=1}^{8} \left(\underbrace{\frac{g[D(t)\,(u(-e_t) - u(-e_t - \tilde{x}_t))]}{g[D(9)\,(u(v+R) - u(v))]}}_{<\,1} D(t)\,(u(-e_t) - u(-e_t - \tilde{x}_t)) \right). \tag{5}$$

Consequently, focusing causes *concentration bias* in the unbalanced choice between the two work plans: in order to obtain the concentrated raise in the value of the voucher, the decision maker is willing to complete *more* tasks on the workdays than her preferences—which are captured in $\tilde{\boldsymbol{x}}$—can account for. Concentration bias causes the decision maker to be too patient in her choice: she overweights the delayed raise in the value of the voucher at the cost of additional work earlier.[1]

2.1.2. Balanced trade-off. The focusing model does not predict concentration bias in binary choices with *balanced* trade-offs. Consider a choice between the following two work plans:

$$\begin{aligned}
\boldsymbol{c}^{\mathrm{b},j} &= (-e_1, \ldots, -e_{j-1}, -e_j, \qquad -e_{j+1}, \ldots, -e_8, v^j) && \text{and} \\
\boldsymbol{c}^{\mathrm{o},j} &= (-e_1, \ldots, -e_{j-1}, -e_j - \tilde{x}_j, -e_{j+1}, \ldots, -e_8, v^j + r^j) && \text{with} \quad j \in \{1, \ldots, 8\}.
\end{aligned}$$

This choice implies a balanced trade-off: the utility benefit and utility cost of accepting either work plan are *both* concentrated in time. Accepting overtime work plan $\boldsymbol{c}^{\mathrm{o},j}$, for instance, involves the

1. When the concentrated utility benefit of an action precedes its dispersed utility cost, concentration bias may cause too impatient choices. We study such unbalanced choices in our money experiment, see Section 5.

Table 1
The balanced and unbalanced trade-offs of MAIN-TREATMENT

Options	Baseline, $\boldsymbol{e}$, and additional, $\boldsymbol{x}$ and d, real-effort tasks								Value of the voucher
	$t=1$	$t=2$	$t=3$	$t=4$	$t=5$	$t=6$	$t=7$	$t=8$	$t=9$
Balanced trade-offs									
$\mathbf{c}^{b,1}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	$v^1=v$
$\mathbf{c}^{o,1}$	$-e_1-x_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	$v^1+r^1=v^2$
$\mathbf{c}^{b,2}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	v^2
$\mathbf{c}^{o,2}$	$-e_1$	$-e_2-x_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	$v^2+r^2=v^3$
$\mathbf{c}^{b,3}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	v^3
$\mathbf{c}^{o,3}$	$-e_1$	$-e_2$	$-e_3-x_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	$v^3+r^3=v^4$
$\mathbf{c}^{b,4}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	v^4
$\mathbf{c}^{o,4}$	$-e_1$	$-e_2$	$-e_3$	$-e_4-x_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	$v^4+r^4=v^5$
$\mathbf{c}^{b,5}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	v^5
$\mathbf{c}^{o,5}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5-x_5$	$-e_6$	$-e_7$	$-e_8$	$v^5+r^5=v^6$
$\mathbf{c}^{b,6}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	v^6
$\mathbf{c}^{o,6}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6-x_6$	$-e_7$	$-e_8$	$v^6+r^6=v^7$
$\mathbf{c}^{b,7}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	v^7
$\mathbf{c}^{o,7}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7-x_7$	$-e_8$	$v^7+r^7=v^8$
$\mathbf{c}^{b,8}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	v^8
$\mathbf{c}^{o,8}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8-x_8$	$v^8+r^8=$ $v^1+r^1+\dots+r^8$
Unbalanced trade-off									
$\mathbf{c}^{b}$	$-e_1$	$-e_2$	$-e_3$	$-e_4$	$-e_5$	$-e_6$	$-e_7$	$-e_8$	$v=v^1$
$\mathbf{c}^{o}$	$-e_1-\tilde{x}_1-d$	$-e_2-\tilde{x}_2-d$	$-e_3-\tilde{x}_3-d$	$-e_4-\tilde{x}_4-d$	$-e_5-\tilde{x}_5-d$	$-e_6-\tilde{x}_6-d$	$-e_7-\tilde{x}_7-d$	$-e_8-\tilde{x}_8-d$	$v+R=$ $v^1+r^1+\dots+r^8$

Notes: In each balanced trade-off j, the decision maker states her willingness to work $\tilde{x}_j$. In the unbalanced trade-off, the decision maker states her willingness to work more ($\tilde{d}>0$) or fewer ($\tilde{d}<0$) additional tasks than $\tilde{\boldsymbol{x}}=(\tilde{x}_1,\dots,\tilde{x}_8)$, or equally many ($\tilde{d}=0$). While discounted utility predicts $\tilde{d}=0$, the focusing model predicts $\tilde{d}>0$.

utility cost of additional tasks only on one workday, $t=j$, and a utility benefit in the form of the raise r^j on the restaurant day.

Assume that the time preferences and per-period utility functions of the decision maker make her indifferent between $\boldsymbol{c}^{b,j}$ and $\boldsymbol{c}^{o,j}$ in the absence of focusing, that is,

$$D(9)(u(v^j+r^j)-u(v^j)) \;=\; D(t)(u(-e_j)-u(-e_j-\tilde{x}_j)). \tag{6}$$

Because of equation (6), the focus weights of the utility benefit of the overtime work plan and its utility cost are identical. Thus, focusing also predicts indifference between $\boldsymbol{c}^{b,j}$ and $\boldsymbol{c}^{o,j}$:

$$D(9)[u(v^j+r^j)-u(v^j)] \;=\; \underbrace{\frac{g[D(j)(u(-e_j)-u_j(-e_j-\tilde{x}_j))]}{g[D(9)(u(v^j+r^j)-u_9(v^j))]}}_{=1} D(j)[u(-e_j)-u_j(-e_j-\tilde{x}_j)]. \tag{7}$$

In binary choices with balanced trade-offs, the predictions of the focusing model and discounted utility coincide: the decision maker's preferences determine her choices entirely.

2.2 Identifying concentration bias

We identify concentration bias via the unbalanced and balanced trade-offs summarised in Table 1. In the unbalanced trade-off, focusing predicts concentration bias: a decision maker is willing to complete *more* tasks on the workdays than her preferences can account for (see Section 2.1.1). To identify concentration bias, we hence need to know what the decision maker's willingness to work would be if it were exclusively based on her preferences. When looking at an unbalanced trade-off in isolation, this information is not observable and hence amounts to the main identification challenge of concentration bias. To overcome this challenge, we employ the following approach. First, since focusing predicts no concentration bias in balanced trade-offs (see Section 2.1.2), we design our balanced trade-offs to reveal sufficient information about the decision maker's preferences. Second, the design of our balanced and unbalanced trade-offs is interconnected in order to allow us to provide a nonparametric measure of concentration bias. This means that our measure does not rely on any assumptions regarding the decision maker's time preferences and per-period utility functions. In the following, we first discuss the balanced and unbalanced trade-offs separately and then discuss their interconnected design.

At first, the decision maker faces all eight balanced trade-offs of Table 1. In each balanced trade-off $j \in \{1, \ldots, 8\}$, the decision maker states the number of additional tasks $\tilde{x}_j$ that make her indifferent between the baseline work plan $\boldsymbol{c}^{\mathrm{b},j}$ and overtime work plan $\boldsymbol{c}^{\mathrm{o},j}$. Each $\tilde{x}_j$ quantifies the maximal number of additional tasks on workday $t = j$ that the decision maker is willing to work in exchange for the raise r^j. Importantly, since focusing predicts no distortions in these balanced trade-offs (see Section 2.1.2), the decision maker's eight indifference points, $(\tilde{x}_1, \ldots, \tilde{x}_8) = \tilde{\boldsymbol{x}}$, are determined entirely by her time preferences and per-period utility functions.

Thereafter, the decision maker faces the unbalanced trade-off of Table 1. The decision maker states the value of extra tasks $\tilde{d}$ that make her indifferent between the baseline work plan $\boldsymbol{c}^{\mathrm{b}}$ and overtime work plan $\boldsymbol{c}^{\mathrm{o}}$. Since $\boldsymbol{c}^{\mathrm{o}}$ requires the decision maker to complete $(\tilde{x}_1 + d, \tilde{x}_2 + d, \ldots, \tilde{x}_7 + d, \tilde{x}_8 + d)$ additional tasks on the eight workdays, $\tilde{d}$ quantifies how many tasks per workday she is willing to work beyond the number of additional tasks $\tilde{\boldsymbol{x}}$. Consequently, the decision maker is willing to complete *no* more than $\tilde{\boldsymbol{x}}$ additional tasks when stating $\tilde{d} = 0$, $\tilde{d}$ *fewer* tasks per workday than $\tilde{\boldsymbol{x}}$ when stating $\tilde{d} < 0$, and $\tilde{d}$ *more* tasks per workday than $\tilde{\boldsymbol{x}}$ when stating $\tilde{d} > 0$.

The decision maker's value of $\tilde{d}$ identifies the effect of concentration bias on her willingness to work in the unbalanced trade-off. This follows from the interconnected design of the trade-offs: (i) The overtime work plan $\boldsymbol{c}^{\mathrm{o}}$ in the unbalanced trade-off includes the decision maker's own indifference points from the balanced trade-offs, $\tilde{\boldsymbol{x}}$. (ii) The structure of the voucher values (see Table 1) implies that the utility benefit in the unbalanced trade-off and the sum of utility benefits of the balanced trade-offs coincide:

$$D(9)\,(u(v+R) - u(v)) \;=\; D(9)\sum_{j=1}^{8}(u(v^j + r^j) - u(v^j)). \tag{8}$$

Design features (i) and (ii) individualise the unbalanced trade-off with respect to the decision maker's preferences. This individualisation has an important consequence in the absence of focusing. At $\tilde{\boldsymbol{x}}$, the discounted utility cost of choosing the overtime work plan in the unbalanced trade-off is equal to the sum of discounted utility costs of choosing the overtime work plans of the balanced trade-offs:

$$\sum_{t=1}^{8} D(t)\,(u(-e_t) - u(-e_t - \tilde{x}_t - \tilde{d})) \;=\; \sum_{t=1}^{8} D(t)\,(u(-e_t) - u(-e_t - \tilde{x}_t)). \tag{9}$$

Equation 9 can only hold for $\tilde{d} = 0$. This implies that the preferences of the decision maker require her to be willing to complete the same number of additional tasks in the unbalanced trade-off as in all balanced trade-offs. Consequently, she states a value of $\tilde{d} = 0$.

The focusing model, by contrast, predicts that concentration bias causes the decision maker to be willing to complete more tasks than her preferences can account for. Thus, she states a value of $\tilde{d} > 0$ (see Section 2.1.1). Moreover, her value of $\tilde{d}$ is the larger, the larger the effect of concentration bias is (that is, the steeper g is). We henceforth refer to $\tilde{d}$ as the absolute effect of concentration bias, it measures the decision maker's deviation from discounted utility.

2.3 Experimental implementation

2.3.1. General set-up. We identify concentration bias in MAIN-TREATMENT by applying the procedure outlined in Section 2.2. Each subject first completes the eight balanced trade-offs of Table 1 in random order and thereafter completes the unbalanced trade-off of Table 1. In the balanced trade-offs, we elicit from each subject their indifference points $(\tilde{x}_1, \ldots, \tilde{x}_8)$ which we then include in their respective unbalanced trade-off. In the unbalanced trade-off, we elicit from each subject their indifference point $\tilde{d}$, which identifies concentration bias on the subject level.

2.3.2. Protocol. Before subjects face the intertemporal trade-offs, they receive information about the real-effort task and the restaurant for which the voucher is valid. The real-effort task require subjects to transcribe a sequence of six numbers into a sequence of six letters, see Figure A.2 in Online Appendix A. The mapping of numbers to letters is randomly determined for each individual task, making each task equally challenging. Subjects complete 10 real-effort tasks to gain experience before they face the trade-offs. Subjects learn that they will complete the workload on each future workday online. Thereafter, subjects choose their eight workdays from a set of 20 dates and the restaurant day, $t = 9$, from a set of seven dates.[2] This allows subjects to accommodate their individual schedules and allows us to emphasize the intertemporal dimension of the trade-offs.

We implement the following parametrisation of the trade-offs. The mandatory number of real-effort tasks $\boldsymbol{e}$ require subjects to complete between 115 and 143 real-effort tasks per workday (which took them 28 minutes on average).[3] We employ a substantial number of mandatory tasks to keep transaction costs between workdays fairly constant and to reproduce an overtime work setting. The smallest attainable value of the voucher $v = v^1$ is €7.50 and ensures that subjects can consume a main course at the restaurant. Each raise in the value of the voucher r^j is between €2.70 and €3.20 and ensures a greater range of items on the menu that they can choose from.[4] We introduce the slight variation in r^j and e_t with the objective that the intertemporal trade-offs vary enough to induce subjects to cognitively engage with each choice.

Following the procedure outlined in Section 2.2, we elicit indifference point $\tilde{x}_j$ in each balanced trade-off $j \in \{1, \ldots, 8\}$. Thereafter, we elicit $\tilde{d}$ in the unbalanced trade-off. We explain our elicitation procedure in Section 2.3.3.

After subjects complete the intertemporal trade-offs, we elicit proxies for their cognitive abilities—Raven Matrices (Raven, 1941), Cognitive Reflection Test (Frederick, 2005), and an incentivised arithmetic test—to investigate potential heterogeneity in concentration bias, as well as demographic information that we use as control variables in our regression analyses.

All subjects receive €10 in cash, plus their earnings from the arithmetic task (on average, €0.94). Following Attema et al. (2016), three subjects are randomly selected at the end of each session (27–32 participants) to implement the work plan associated with their payoff-relevant choice. After completion of their entire work plan, these subjects receive their respective restaurant voucher as well as an additional lump-sum compensation of €100. All selected subjects completed their work plans.

Experimental sessions lasted up to 75 minutes. Subjects were invited using ORSEE (Greiner, 2015), and the experiment was programmed in oTree (Chen et al., 2016).

2.3.3. Elicitation of indifference points. All indifference points are elicited from subjects via a series of binary choices. In each balanced trade-off j, subjects repeatedly choose between the

2. The available workdays comprised all business days during the four weeks following the date of the experiment. The average first workday was 6.65 days after the experiment. The average distance between workdays, including weekends, was 1.84 days. The average restaurant day was 15.73 days after the last workday.

3. $\boldsymbol{e} = (e_1, \ldots, e_8) = (129, 132, 143, 115, 121, 117, 135, 127)$.

4. $\boldsymbol{r} = (r^1, \ldots, r^8) = (€2.90, €2.70, €3.10, €2.70, €3.20, €2.90, €3.10, €3.20)$.

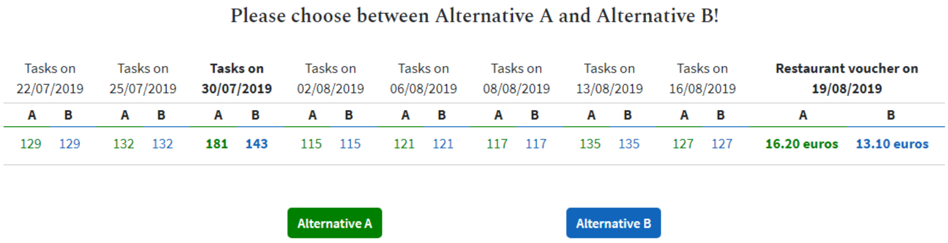

Figure 1
Screenshot of a balanced choice in MAIN-TREATMENT.

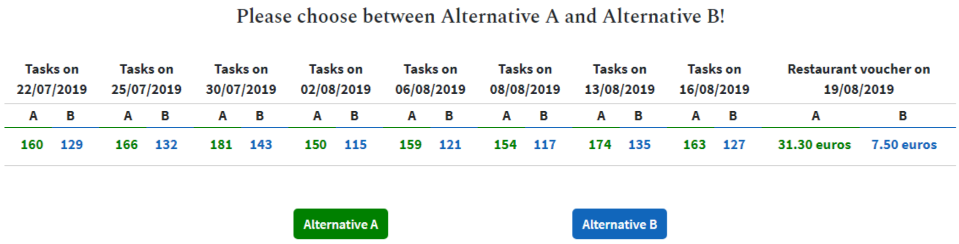

Figure 2
Screenshot of an unbalanced choice in MAIN-TREATMENT.

baseline work plan $\mathbf{c}^{\mathrm{b},j}$ and overtime work plan $\mathbf{c}^{\mathrm{o},j}$. Figure 1 depicts an exemplary balanced choice of trade-off $j = 3$. Between individual choices within each balanced trade-off j, we vary the number of additional tasks $x_t \in \{0, 1, 2, \ldots, 125\}$, with $t = j$, while keeping all remaining parameters ($\boldsymbol{e}$, v^j, r^j) constant. Likewise in the unbalanced trade-off, subjects repeatedly choose between the baseline work plan $\mathbf{c}^{\mathrm{b}}$ and overtime work plan $\mathbf{c}^{\mathrm{o}}$. Figure 2 depicts an exemplary unbalanced choice. Between unbalanced choices, we only vary the number of extra tasks $d \in \{-63, -62, \ldots, 0, \ldots, 61, 62\}$, while keeping the remaining parameters ($\boldsymbol{e}$, v, R, $\tilde{\boldsymbol{x}}$) constant.

Each trade-off hence consists of 126 pairwise choices. In each trade-off, however, subjects make only up to nine choices *directly*. The remaining choices are made *indirectly* according to a procedure that is logically equivalent to making choices in a choice list. In contrast to a choice list, however, our procedure avoids a potential tendency to switch close to the middle of the list. We explain the procedure in the following paragraph and provide an example in Online Appendix B.

The procedure works as follows: The first choice in each trade-off is made directly. To that end, the computer randomly chooses a number of additional tasks from all 126 possible values, that is, x_t from $\{0, 1, \ldots, 125\}$ in balanced trade-off $j = t$ or d from $\{-63, \ldots, 62\}$ in the unbalanced trade-off. If a subject agrees to complete this particular number of additional tasks for the given raise, then she *indirectly* also agrees to complete any lower number of tasks. Conversely, if a subject chooses not to complete this particular number of additional tasks and thereby forgoes the given raise, she then *indirectly* also chooses not to complete any larger number of additional tasks. Each next direct choice is then determined dynamically so that the expected number of direct choices that a subject has to make until she reaches her indifference point is minimised. This is achieved by selecting as the next direct choice the midpoint of the implied interval of additional tasks that has not been covered by direct and indirect choices yet. The procedure stops when all choices have been made (directly or indirectly). We then conservatively define a subject's indifference point as the largest number of additional tasks that she agrees to complete in exchange for the respective raise. Subjects' comprehension of this procedure was ensured by computerised instructions, control questions, and practice choices.

The procedure has the following implications: we elicit indifference points with a low number of direct choices—which prevents subjects from getting bored and tired—despite being able

Table 2
Absolute and relative concentration bias in MAIN-TREATMENT

	OLS			Tobit
	Lower bound (1)	Midpoint (2)	Upper bound (3)	(4)
$\tilde{d}$ in MAIN-TREATMENT	31.640*** (2.685)	37.610*** (3.575)	43.580*** (4.683)	37.094*** (3.658)
$\tilde{d}^{\text{rel}}$ in MAIN-TREATMENT	0.190*** (0.016)	0.224*** (0.021)	0.259*** (0.027)	
Observations	100	100	100	100

Notes: This table presents estimates of the average absolute and relative measure of concentration bias, $\tilde{d}$ and $\tilde{d}^{\text{rel}}$, respectively. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT. $^{*}\ p < 0.1$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

to cover a wide range of additional tasks;[5] and the method ensures single switching so that indifference is unambiguously determined for subjects without corner choices. A subject makes a corner choice whenever she always chooses the same option in a trade-off.

The procedure incentivises subjects to report their indifference points truthfully: (i) Each direct and indirect choice has the same probability of being randomly selected to be payoff-relevant in the end. (ii) As in Halevy et al. (2018), subjects are not made aware that balanced and unbalanced trade-offs are interconnected.

2.3.4. Corner choices in balanced trade-offs. By construction, our measurement of concentration bias in the unbalanced trade-off relies on subjects stating indifference points in the balanced trade-offs. We hence preregistered to exclude all subjects with at least one corner choice in the set of balanced trade-offs from our main analyses. In Online Appendix A.4, we show that this exclusion restriction does not affect our results.

2.3.5. Corner choice in the unbalanced trade-off. Our measure of concentration bias is subjects' indifference point $\tilde{d}$ in the unbalanced trade-off. Subjects with upper corner choices—always choosing the overtime work plan—only reveal a lower bound of their indifference point: the upper bound of the interval from which d is chosen (that is, 62). In an un-incentivised way, we ask these subjects to state their indifference point $\tilde{d}$ manually (see Figure A.3 in Online Appendix A for a screenshot). We henceforth use the following three values to proxy their $\tilde{d}$: (i) the lower bound ($\tilde{d} = 62$); (ii) the upper bound (their manually stated indifference point); (iii) the midpoint between (i) and (ii).

2.3.6. Preregistration. After presenting the results of MAIN-TREATMENT, we discuss further conditions of our consumption experiment in sections 3 and 4. All conditions were preregistered, including their design, sample size, exclusion restrictions, and a pre-analysis plan.[6]

2.4 Results

Our measure of concentration bias is $\tilde{d}$: the number of per-workday tasks that subjects are willing to complete in the unbalanced trade-off beyond what their preferences could account for, that is,

5. Our subjects make between 53 and 70 binary decisions. In comparison, subjects make 45 decisions in Andreoni and Sprenger (2012), 100 in Andersen et al. (2008), and 523 in Attema et al. (2016).

6. See https://doi.org/10.1257/rct.4446 for MAIN-TREATMENT, MECHANISM-TREATMENT, MAIN-CONTROL, and MECHANISM-CONTROL and https://doi.org/10.1257/rct.4341 for DONATION-TREATMENT and DONATION-CONTROL.

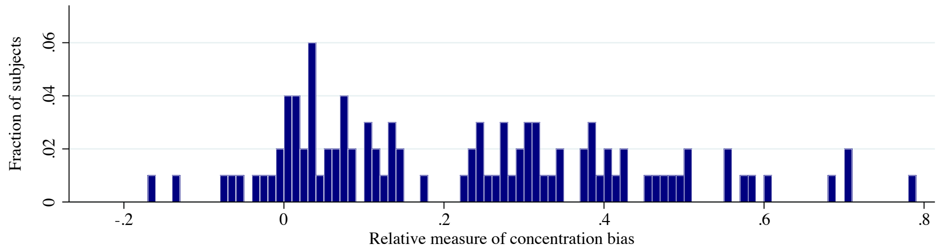

Figure 3
Distribution of the relative measure of concentration bias ($\tilde{d}^{\text{rel}}$) in MAIN-TREATMENT.

beyond $\tilde{\boldsymbol{x}}$. We elicit $\tilde{\boldsymbol{x}}$ from each subject in the balanced trade-offs and find, on average, that subjects' preferences allow them to complete between 33 and 43 additional tasks per workday:[7]

$$\tilde{\boldsymbol{x}} = (39.2, 34.9, 33.6, 41.5, 41.8, 42.2, 35.9, 38.3). \tag{10}$$

In the unbalanced trade-off, we find evidence for concentration bias: subjects are willing to complete more overtime work than is implied by $\tilde{\boldsymbol{x}}$. While we directly measure $\tilde{d}$ from subjects with an interior indifference point (73% of all subjects in MAIN-TREATMENT), subjects with an upper corner choice (see Section 2.3.5) yield a lower bound, midpoint, and upper bound estimate of $\tilde{d}$. These estimates yield three across-subjects averages of $\tilde{d}$ that are between 31.6 and 43.6 tasks, see columns 1, 2, and 3 of Table 2. A Tobit regression based on the lower bound yields an estimate of concentration bias in similar magnitude, see column 4 of Table 2. All estimates of $\tilde{d}$ are statistically significantly greater than zero ($p < 0.001$). Taking the midpoint estimate, subjects' willingness to work in the unbalanced trade-off is, on average,

$$\tilde{\boldsymbol{x}} + \tilde{d} \cdot \mathbf{1} = (76.8, 72.5, 71.2, 79.1, 79.4, 79.8, 73.5, 75.9), \tag{11}$$

and hence exceeds $\tilde{\boldsymbol{x}}$ substantially. In relative terms, concentration bias causes subjects, on average, to increase their per-period willingness to work by $\tilde{d}^{\text{rel}} = 22.4\%$ ($p < 0.001$), where

$$\tilde{d}^{\text{rel}} := \frac{1}{8} \sum_{t=1}^{8} \frac{\tilde{d}}{e_t + \tilde{x}_t}. \tag{12}$$

Figure 3 depicts the distribution of $\tilde{d}^{\text{rel}}$ and shows that a large fraction of subjects (88%) exhibit concentration bias ($\tilde{d}^{\text{rel}} > 0$).[8] Row 2 of Table 2 reports $\tilde{d}^{\text{rel}}$ also based on the two other estimates of $\tilde{d}$; both values are significantly greater than zero. In the following, we refer to $\tilde{d}^{\text{rel}}$, based on the midpoint estimate for subjects with corner choices, as our measure of concentration bias.[9]

Result 1. *Concentration bias causes subjects to increase their willingness to work by 22.4%.*

3 Mechanism

Having identified concentration bias in intertemporal choice, we now investigate its mechanisms. According to the focusing model, the observed concentration bias in MAIN-TREATMENT is the res-

7. Between-workdays differences in $\tilde{\boldsymbol{x}}$ may reflect, for instance, nonlinear costs from effort and between-workdays differences in the number of mandatory real-effort tasks, the temporal distance of the date on which the tasks have to be completed, and the possibility that subjects have more time to work on some dates than on others.

8. Figure 3 reveals substantial heterogeneity in the degree of concentration bias. In Online Appendix A.3, we investigate preregistered correlates of this heterogeneity. While cognitive skills are not correlated with concentration bias, response times in unbalanced choices are significantly and negatively correlated with concentration bias.

9. All results based on our relative measure of concentration bias are robust to using a different specification of the relative measure (see Online Appendix A.5) and to using the absolute measure (see Online Appendix A.6).

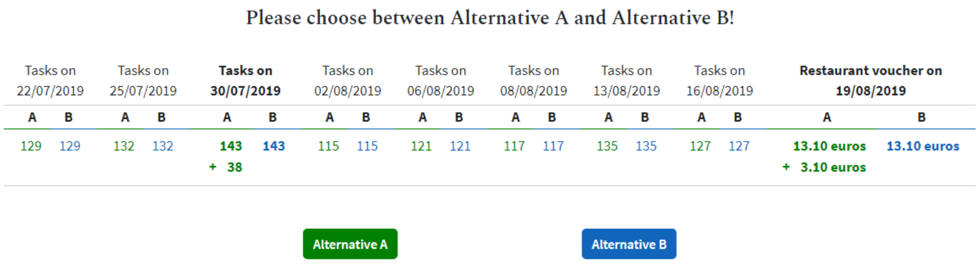

Figure 4
Screenshot of a decision screen for a balanced choice in the Mechanism-Treatment condition.

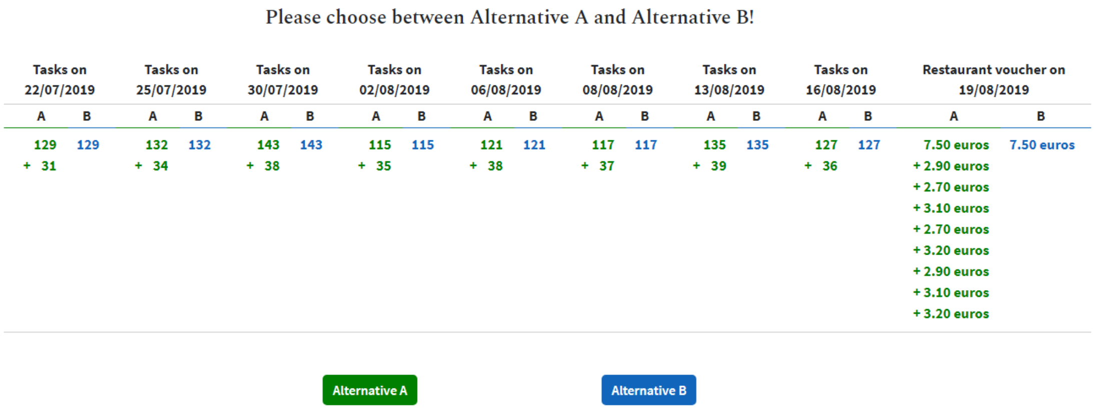

Figure 5
Screenshot of a decision screen for an unbalanced choice in the Mechanism-Treatment condition.

ult of differential concentration in time. Alternatively, concentration bias may be driven by the *framing* of utility consequences. A utility consequence is framed in a concentrated way when the entirety of the consequence is directly observable and, hence, tangible. A utility consequence, by contrast, is framed in a dispersed way when the entirety of the consequence is not directly observable and rather difficult to grasp. Framing could contribute to concentration bias if a greater degree of tangibility increases how much attention is paid to a utility consequence. In the domain of belief updating, Enke (2020) provides evidence for such a link between tangibility, attention, and overweighting: subjects overweight signals that are directly observable and underweight signals that are merely (yet easily) inferable.

To illustrate how framing could explain our observed concentration bias, we consider the utility benefit and utility cost of choosing the overtime work plan in Main-Treatment. The raise in the value of the voucher is not only concentrated in time, it is also displayed in a concentrated framing: the raised value of the restaurant voucher is depicted in its entirety and hence framed in a tangible way. The additional tasks, in contrast, are dispersed over time and are also framed in a dispersed way: they are depicted for each workday separately. The aggregated utility cost of additional tasks on all workdays is hence not tangible. An effortful mental aggregation is required before a cognitive representation is available (Kahneman, 2003). If subjects' decision weights of utility consequences are affected by how tangible they are, the concentrated utility benefit of the overtime work plan may be overweighted relative to the dispersed utility cost of additional tasks on all workdays. Consequently, the concentrated framing of the utility benefit and the dispersed framing of the utility cost may contribute to (if not entirely explain) our evidence for concentration bias.

Based on these deliberations, we designed the Mechanism-Treatment condition to make the following contributions. First, we test whether temporal concentration drives concentration bias, as predicted by the focusing model. Second, we investigate the role of framing in concentration bias. Third, we discuss how our findings in MECHANISM-TREATMENT relate to the focusing model. (In Section 6, we briefly discuss potential policy implications.)

MECHANISM-TREATMENT consists of the same balanced and unbalanced choices as MAIN-TREATMENT. In the unbalanced trade-off of both conditions, the raise in the value of the voucher is equally concentrated on the restaurant day, and the additional tasks equally arise on all eight workdays. If concentration in time causes concentration bias, subjects also exhibit concentration bias in MECHANISM-TREATMENT. If framing also contributes to concentration bias, we predict, however, a smaller degree of concentration bias in MECHANISM-TREATMENT than in MAIN-TREATMENT. This follows from employing a dispersed framing of the raise in the restaurant voucher in MECHANISM-TREATMENT: instead of depicting the raise in its entirety as in MAIN-TREATMENT,

€31.30 (see Figure 2),

we depict the raise as the sum of multiple parts in MECHANISM-TREATMENT,

€7.50 + €2.90 + €2.70 + €3.10 + €2.70 + €3.20 + €2.90 + €3.10 + €3.20 (see Figure 5).

Consequently, the utility benefit of completing additional tasks is no longer tangible in MECHANISM-TREATMENT. At the same time, we maintain a dispersed framing of the dispersed-over-time additional tasks in MECHANISM-TREATMENT.[10] Thus, concentration bias should be less pronounced in MECHANISM-TREATMENT if framing plays a role.

Conditions MECHANISM-TREATMENT and MAIN-TREATMENT were conducted (i) at the same laboratory, (ii) based on the same subject pool, and (iii) on the same dates (in alternating sessions). In fact, subjects' average balanced choices do not differ significantly between MECHANISM-TREATMENT and MAIN-TREATMENT, as would be expected given that the balanced choices should be determined entirely by subjects' preferences.

Turning to the results of the unbalanced trade-off in MECHANISM-TREATMENT, we find that concentration bias causes subjects to be willing to complete 7.5% more tasks than their preferences could account for. This relative measure of concentration bias is significantly greater than zero ($p < 0.001$), yet significantly smaller than in MAIN-TREATMENT ($p < 0.001$), see column 1 of Table 3. In particular, significantly fewer subjects display concentration bias in MECHANISM-TREATMENT than in MAIN-TREATMENT (70% versus 88%, $p < 0.01$), and the magnitude of concentration bias conditional on concentration bias being present is significantly smaller in MECHANISM-TREATMENT than in MAIN-TREATMENT (16% versus 26%, $p < 0.001$). We conclude:

Result 2. *Both concentration in time as well as framing contribute to concentration bias.*

According to the focusing model, focusing weights are applied to the exogenous attributes of choice options. In intertemporal choice, Kőszegi and Szeidl (2013) assume that attributes are time periods. Consequently, *per-period* differences in utility consequences attract attention according to the focusing model. Result 2 supports this view of time period-based attributes: subjects disproportionately focus on the large utility difference on the restaurant day (even when the utility benefit is framed in a dispersed way). Result 2 indicates that the framing of utility consequences matters as well: concentration bias is more pronounced when the utility benefit that is concentrated in time is also framed in a concentrated manner.

10. The dispersed framing of the raise in the restaurant voucher in MECHANISM-TREATMENT involves "+" signs to ensure that subjects understand that they would receive the sum. In order to maintain the same display of differences in the available choice options between utility benefits and costs, the display of per-workday additional tasks was also adapted to include "+" signs (see Figures 4 and 5). While this minor display change allows us to maintain a dispersed framing of the dispersed-over-time additional tasks, we can rule out that it affects intertemporal choices in isolation: all balanced choices in MECHANISM-TREATMENT also employ this minor display change and subjects' average choices in the balanced trade-offs do not differ between MECHANISM-TREATMENT and MAIN-TREATMENT.

Table 3
Average relative measure of concentration bias ($\tilde{d}^{\text{rel}}$) across conditions

	OLS			
	(1)	(2)	(3)	(4)
$\tilde{d}^{\text{rel}}$ in MECHANISM-TREATMENT	0.075***			
	(0.019)			
Difference:	0.149***			
MAIN-TREATMENT − MECHANISM-TREATMENT	(0.028)			
$\tilde{d}^{\text{rel}}$ in DONATION-TREATMENT		0.135***		
		(0.021)		
Difference:		0.158***		
DONATION-TREATMENT − DONATION-CONTROL		(0.022)		
$\tilde{d}^{\text{rel}}$ in MAIN-CONTROL			−0.023***	
			(0.008)	
Difference:			0.247***	
MAIN-TREATMENT − MAIN-CONTROL			(0.023)	
$\tilde{d}^{\text{rel}}$ in MECHANISM-CONTROL				−0.014
				(0.010)
Difference:				0.090***
MECHANISM-TREATMENT − MECHANISM-CONTROL				(0.021)
Observations	200	200	200	200

Notes: Robust standard errors are in parentheses. The sample includes all observations from the mentioned (between-subjects) conditions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

While framing is not part of the focusing model, Kőszegi and Szeidl (2013) discuss how framing relates to the focusing model: Framing may affect what constitutes attributes in a given choice. To illustrate this, consider a consumer who contemplates buying a consumption product via a financing option that disperses the overall cost into monthly payments of $39 over two years. Since the payments are dispersed over time, the focusing model predicts that the financing option makes purchasing relatively attractive. The seller may even advertise the product by framing the payments in terms of $1.28 a day; a friend, by contrast, may frame her advice in terms of the total payment of $936. Kőszegi and Szeidl (2013, p. 68–69) argue that

> the first type of framing can make the consumer think about the daily costs as attributes, so that she finds the financing option even more attractive [...], whereas the second type of framing can make her think about the total cost as an attribute, so that she finds the financing option less attractive [...].

Result 2 provides support for this. By employing a dispersed framing of the utility benefit in MECHANISM-TREATMENT, subjects may tend to treat the individual parts of the benefit as separate attributes, focusing less on the utility benefit, and hence exhibiting less concentration bias.

4 Robustness

Noise in subjects' choices. Our measure of concentration bias is robust to potential noise in subjects' choices. Since we vary d in the unbalanced trade-off between −63 and +62, we symmetrically allow positive and negative values of $\tilde{d}$. This ensures that noise cannot simply produce our finding of an average value of $\tilde{d} > 0$. In addition, 88% of subjects in MAIN-TREATMENT stated values of $\tilde{d} > 0$, which is significantly greater than 50% ($p < 0.001$).

Donation-Treatment. In the Donation-Treatment condition of our consumption experiment, we test the robustness of our main evidence for concentration bias. Donation-Treatment is a replica of Main-Treatment, apart from one difference: the utility benefit of working is a donation to a good cause[11] instead of a restaurant voucher. In Donation-Treatment, we find a concentration bias of 13.5% ($p < 0.001$), see column 2 in Table 3. Since we use greater values for the restaurant vouchers in Main-Treatment than the donation values in Donation-Treatment, our pre-registration does not include a comparison between these conditions.[12]

Result 3. *Concentration bias is robust to using a donation instead of a voucher.*

Main assumptions. Our design of the consumption experiment is based on two assumptions. In the following, we discuss which violations of these assumptions would confound our identification of concentration bias. We provide specific evidence against each type of violation below.

Preference stability. We assume that a decision maker's preferences are stable across the balanced choices and the subsequent unbalanced choices of our Treatment conditions. If the decision maker's preferences change and suddenly allowed her to tolerate more additional real-effort tasks in the final choices, our measurement of concentration bias could be confounded.[13] We designed the Control conditions (Main-Control, Mechanism-Control, and Donation-Control) to test this and conducted them alongside their respective Treatment conditions.[14] In all Control conditions, subjects first complete the balanced trade-offs in random order like in their respective Treatment conditions. Instead of completing the unbalanced trade-off thereafter, however, subjects complete balanced trade-off $j = 1$, $j = 4$, or $j = 8$ for a second time. In this repetition, we vary the number of additional real-effort tasks in the same way as in the unbalanced trade-off of the Treatment conditions: subjects can deviate from their indifference point $\tilde{x}_t$ (that was elicited in balanced trade-off $j = t$) in both directions, by integers in $[-63, +62]$. In contrast to the Treatment conditions, the final trade-off hence consists of balanced choices in the Control conditions. This implies that now the focusing model does not predict that subjects will deviate from their earlier choices. In particular, $\tilde{d}^{\text{rel}}$ should not be greater than zero. If, however, subjects' preferences suddenly tolerated more work in the final choices, $\tilde{d}^{\text{rel}}$ should also be greater than zero in the Control conditions. We do not find such evidence—see columns 3 and 4 of Table 3: subjects do not tolerate more work in the final trade-off in the Control conditions.[15] In addition, all estimates of concentration bias from the Treatment conditions are significantly ($p < 0.001$) larger than the "placebo" deviations in the respective Control conditions. Our evidence for concentration bias is hence not confounded by preference instability.

Additive separability. We assume that a decision maker expects that her disutility from completing real-effort tasks is additively separable over time. If instead the decision maker expects her disutility from work to decrease strongly in the number of previously completed real-effort tasks, she could, in principle, be more willing to work in an unbalanced trade-off than in balanced trade-offs. In light of our experimental design and subjects' observed intertemporal choices, such

11. The donation was made to LichtBlick Seniorenhilfe (https://seniorenhilfe-lichtblick.de), a German nonprofit organisation that provides financial and administrative assistance to old-age citizens in financial distress.

12. Greater values in Main-Treatment ensure that subjects can purchase dishes solely based on the voucher.

13. Since the unbalanced trade-off is preceded by eight balanced trade-offs, fatigue might be a factor.

14. Assignment to a condition within the three pairs of conditions Main, Mechanism, and Donation was randomised within-session under the following restriction: We stratify the assignment based on the average number of tasks chosen in the balanced choice blocks. All participants who fulfil the inclusion criterion, see Section 2.3.4, are ranked according to the average number of tasks chosen in the balanced trade-offs, and pairs of neighbouring ranks are formed for whom conditions are randomly and mutually exclusively assigned. This is done to make subjects as comparable as possible between the conditions of each condition pair.

15. If anything, we find that subjects tolerate slightly fewer tasks in the last trade-off. While this effect is small in magnitude, it is statistically significant only in Main-Control and Donation-Control when considering subjects' average values of $\tilde{d}$ and $\tilde{d}^{\text{rel}}$. Turning to medians instead yields no significant differences from zero.

a violation of time separability is implausible. First, the real-effort task was designed to be dull and repetitive, and to prevent learning beyond mere familiarity effects. The latter follows from two features of the task: each trial was equally demanding (due to a novel letter–number mapping in each trial), and the general structure of the task was not complex enough to give subjects room to figure out more efficient executions after sufficient exposure. Since subjects practiced the real-effort tasks prior to facing the intertemporal choices, they experienced how dull and repetitive the task is, and that little if any learning is possible. In light of these design features, we think that it is not plausible that subjects anticipate a violation of additive separability.[16] Second, an anticipation of such a violation would affect subjects' willingness to work not only in the unbalanced trade-off, but also in the balanced trade-offs. We show in a calibration exercise in Online Appendix D that a violation that is extreme enough to explain the observed willingness to work in the unbalanced trade-off would imply one of the following: a pattern of indifference points in the balanced trade-offs should be observed that is at odds with subjects' actual behaviour; or an absurd level of anti-discounting is required.[17]

Result 4. *Unstable preferences and a violation of additive separability cannot explain our evidence for concentration bias.*

5 Money Experiment

In an additional experiment, we test for concentration bias over dated money transfers to subjects' bank accounts. The money experiment also exploits comparisons between balanced and unbalanced intertemporal choices. It differs from the consumption experiment in the following aspects: (i) We investigate two different comparisons between balanced and unbalanced choices to test whether concentration bias causes subjects to be too patient as well as too impatient. (ii) We vary the degree of dispersion among the unbalanced choices so that we can investigate whether a greater degree of dispersion leads to stronger concentration bias. (iii) The design relies on the assumption of nonconvex per-period utility.[18] (iv) While also using a within-subjects design, we did not individualise the unbalanced choices in our money experiment. Instead, all subjects faced exactly the same balanced and unbalanced choices. A consequence of this design feature is that we estimate concentration bias conservatively in the money experiment, as we discuss below.

The money experiment consists of two conditions, Money-Main ($n = 185$) and Money-Mechanism (additional $n = 189$). Money-Main tests for concentration bias that is potentially generated by both concentration in time and a concentrated framing, while Money-Mechanism focuses on the latter channel by inducing a dispersed framing through a change in the presentation of the concentrated payoff. We summarise the money experiment in the following and we provide a detailed description in Online Appendix C. Our results corroborate and extend our evidence for concentration bias from the consumption experiment:

Result 5.

(i) Concentration bias is also present in purely monetary trade-offs and when using a different elicitation method.

16. If anything, we anticipated that our real-effort task might be tiring over time (which could not confound our estimation of concentration bias). To circumvent such potential exhaustion/fatigue effects, we limited the within-workdays scope of the tasks and allowed subjects to choose their own workdays in advance to accommodate their own personal schedules as well as their potential need to rest between workdays.

17. A daily discount factor of 1.24 would be required. Recent studies estimate daily discount factors very close to unity. For instance, Augenblick et al. (2015) estimate a weekly (daily) discount factor of 0.999 (0.9999) and Augenblick and Rabin (2019) a daily discount factor between 1.003 and 1.005.

18. Previous findings in experiments using monetary payments, for instance, Andreoni and Sprenger (2012) and Augenblick et al. (2015), estimate that utility is close to linear but concave.

(ii) *Depending on the temporal structure of the dispersed and concentrated consequences of the available alternatives, concentration bias can cause subjects to be too patient or too impatient, as predicted by the focusing model.*
(iii) *The strength of concentration bias is monotonically increasing in the degree to which the dispersed outcomes are spread over time, as predicted by the focusing model.*
(iv) *Also in this setting, framing contributes to concentration bias.*

Money-Main. In Money-Main, each subject repeatedly allocates a monetary budget to an earlier and a later payoff (Andreoni and Sprenger, 2012). Our variable of interest is subjects' "savings rate", that is, the share of the budget that they allocate to the later payoff. We implement an intertemporal budget constraint with a strictly positive nominal interest rate. Thus, the sum total of the payoffs is the greater, the more subjects save. To identify concentration bias, we compare subjects' savings rate between balanced and unbalanced decisions. In balanced decisions, both payoffs are concentrated on a single payment date. In unbalanced decisions, either the *earlier* or the *later* payoff is dispersed over two, four, or eight payment dates.[19] Importantly, the discounted utility of each dispersed payoff is weakly greater than that of its concentrated counterpart: all payments of the dispersed payoffs occur no later than the corresponding concentrated payment, and they sum up to the concentrated payoff. In the absence of concentration bias, subjects hence should save more when the *later* payoff is dispersed and should save less when the *earlier* payoff is dispersed. The focusing model, by contrast, predicts that subjects overweight concentrated payoffs relative to dispersed payoffs: compared to the balanced choices, a sufficiently strong concentration bias leads subjects to save less when the later payoff is dispersed and to save more when the earlier payoff is dispersed.[20]

The results of Money-Main provide evidence for concentration bias. Subjects allocate, on average, 5.7 percentage points (p.p.) less money to later payment dates when the later payoff is dispersed rather than concentrated. They also allocate, on average, 6.8 p.p. more money to the later payment dates when the earlier payoff is dispersed rather than concentrated. Both differences are significantly different from zero ($p < 0.01$). This demonstrates that concentration bias can cause subjects to be too impatient as well as too patient, consistent with the central prediction of the focusing model.

In addition, we find that the size of concentration bias depends on the degree to which the dispersed payoff is spread over time. Our measure of concentration bias is 8.1 p.p. for a dispersion over eight payment dates, 6.6 p.p. for four payment dates, and 3.7 p.p. for two payment dates. While all three measures are significantly different from zero ($p < 0.001$ for eight and four dates and $p < 0.05$ for 2 dates), concentration bias is significantly greater ($p < 0.01$) when payoffs are dispersed over four or eight payment dates than when they are dispersed over two payment dates.

Money-Mechanism. Like in Section 3, we investigate whether framing contributes to the concentration bias observed in our money experiment. To that end, we compare Money-Main with the between-subjects condition Money-Mechanism. Relative to Money-Main, subjects face exactly the same balanced decisions as in Money-Mechanism, but slightly different unbalanced decisions. The dispersed payoffs in these decisions are not dispersed over time, but are "dispersed within a day": they are presented to subjects in a dispersed framing although occurring on a single day. More precisely, in the dispersed-within-a-day decisions either the earlier or the later payoff

19. To equalise transaction costs between balanced and unbalanced decisions, we hold the number of transfers constant across conditions. To this end, subjects receive an additional fixed amount of €1 on each of 9 payment dates. That is, each budget set gives rise to 9 money transfers to subjects' bank accounts at given dates in the future.

20. Since the discounted utility of each dispersed payoff is weakly greater than its concentrated counterpart, our measure of concentration bias is conservative. For some subjects, the greater discounted utility of the dispersed payoff may dominate the overvaluation of the concentrated payoff due to concentration bias and for others it may just reduce the effect of concentration bias.

is displayed as the sum of two, four, or eight smaller payments that all occur on a single day. This implies that in all decisions of Money-Mechanism the payoffs are concentrated in time and subjects face only balanced trade-offs.[21]

If both framing and concentration in time contribute to concentration bias, we should also observe concentration bias in Money-Mechanism but in a smaller size than in Money-Main. This is what we find: On average, subjects allocate 2.6 p.p. more of their budget to concentrated than to "dispersed-within-a-day" payoffs ($p < 0.01$). A difference-in-differences analysis reveals that our measure of concentration bias from Money-Main is significantly greater than the effect in Money-Mechanism ($p < 0.01$). Like in our consumption experiment, both framing and concentration in time contribute to concentration bias.

6 Conclusion

By providing clean and robust evidence for concentration bias, our findings relate to other recent theories of attention-based decision making. Relative thinking (Bushong et al., 2021) is built on the opposite assumption of focusing (Kőszegi and Szeidl, 2013) and hence predicts a reversed-concentration bias effect in our unbalanced trade-off, which is inconsistent with our evidence. Since similarity-based decision making (Rubinstein, 2003) and salience theory (Bordalo et al., 2012) assume, like focusing, that differences in an attribute attract attention, these models are broadly consistent with our findings. They differ from focusing, however, in several aspects. Consequently, they do not unambiguously predict concentration bias: (i) According to similarity-based decision making, the more often a utility difference is repeated over time, the more attention it attracts (see Experiment II in Rubinstein, 2003). In unbalanced trade-offs, a dispersed disadvantage could hence be overweighted, and concentration bias may not be predicted. (ii) Salience theory also assumes diminishing sensitivity, which counteracts the behavioural forces implied by the focusing assumption. The relative importance of these assumptions for behaviour is not fully determined. If diminishing sensitivity dominates, concentration bias is not predicted by salience theory.

Our findings have implications for policy interventions that try to mitigate the adverse consequences of too impatient (present-biased) intertemporal choice. First, the attention-grabbing advantages of undesirable behaviours which are typically concentrated in the present could be reduced through substantially higher purchasing prices. Second, the dispersed-over-time costs of undesirable behaviours which are typically easy-to-neglect could be made more tangible. For instance, pension savings may be increased if the total value of the retirement savings at the time of entering retirement is reported as a lump sum instead of being reported as an annuity. Similarly, public health campaigns that promote healthy life styles could focus on quantifying the consequences of unhealthy behaviours in terms of the total treatment costs over one's entire life. For instance, a campaign aimed at reducing excessive consumption of sugar could highlight the total cost of treating chronic diseases such as diabetes. Alternative approaches may involve presenting adverse health consequences in a concentrated framing. For instance, mandating pictures on cigarette packs that illustrate the severe health consequences of smoking may make the costs of smoking more tangible and thereby reduce smoking.

21. Importantly, subjects see exactly the same numbers in their unbalanced decisions in Money-Main and Money-Mechanism. The only difference is that the dispersed payoffs are dispersed over time and in display in Money-Main, while they are merely dispersed in display in Money-Mechanism.

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Online Appendix

A Supplementary Material

A.1 Screenshots of the real-effort task

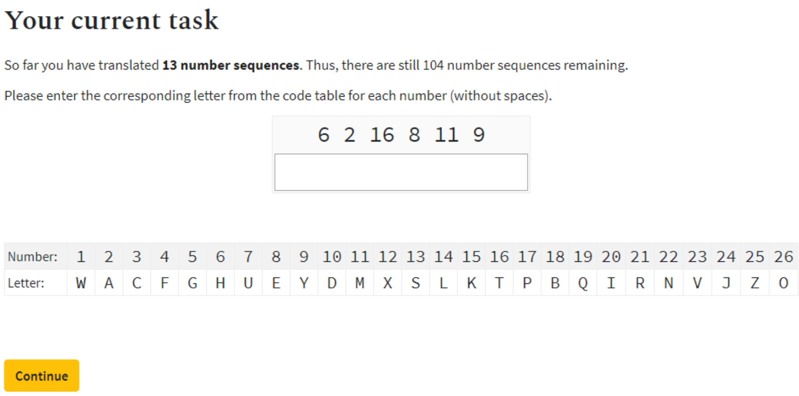


Figure A.1
An example screenshot of the real-effort task. The solution is “HATEMY” in this case.

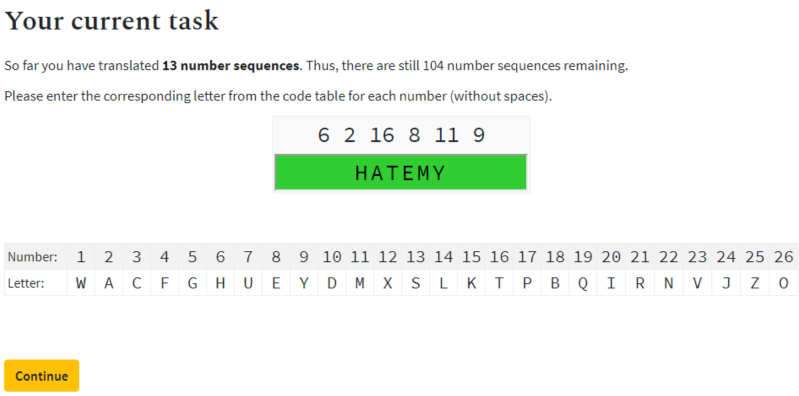


Figure A.2
An example screenshot of the real-effort task when the correct solution has been entered.

A.2 Screenshots of the manual elicitation of indifference points

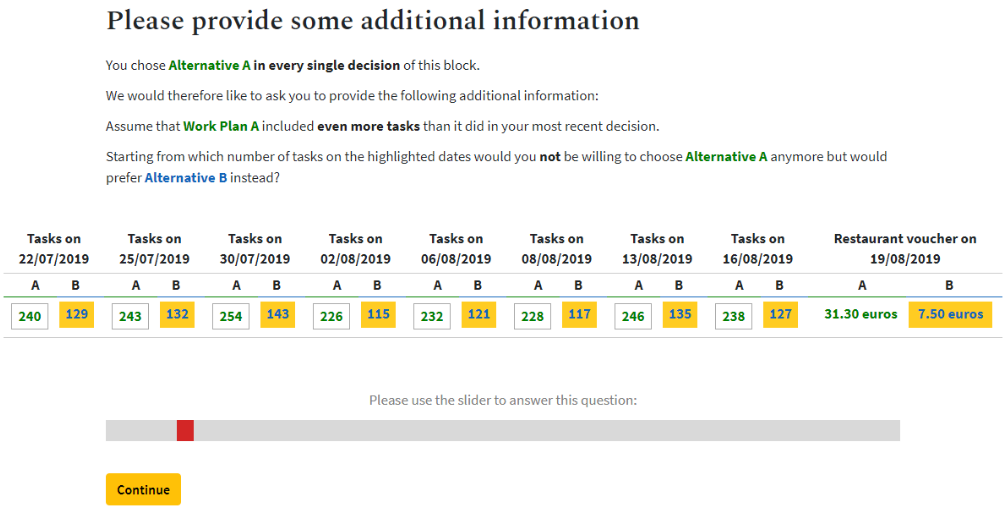


Figure A.3

A screenshot of a screen for manually entering an indifference point for the unbalanced trade-off.

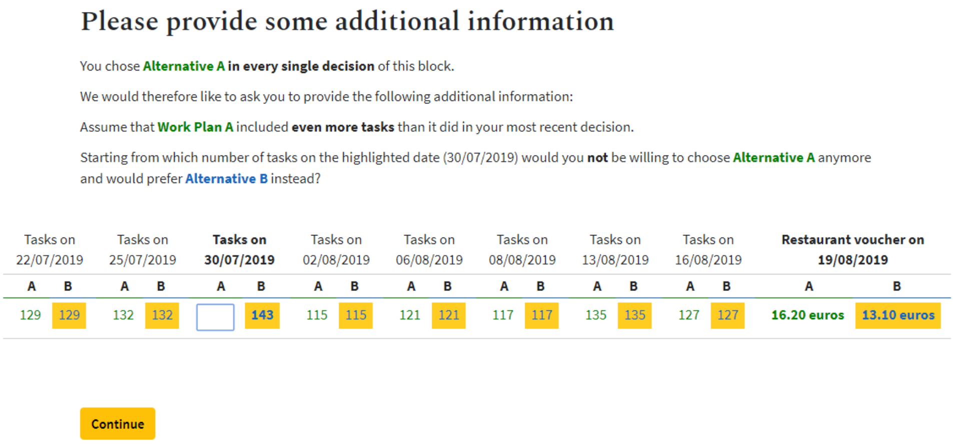


Figure A.4

A screenshot of a screen for manually entering an indifference point for a balanced trade-off.

A.3 Heterogeneity

Figure 3 depicts the distribution of the relative measure of concentration bias in Main-Treatment. While nearly all subjects (88%) exhibit concentration bias ($\tilde{d} > 0$ and $\tilde{d}^{\text{rel}} > 0$), there is substantial heterogeneity in the degree of concentration bias.

To investigate the correlates of this heterogeneity, we conduct four preregistered heterogeneity analyses:

(i) subjects' average response time in the unbalanced choice block;
(ii) performance in an arithmetic task (that asks subjects to calculate the sum of 8 distinct numbers repeatedly in 20 trials with a time constraint);
(iii) performance in the Cognitive Reflection Test (Frederick, 2005);
(iv) performance in a Raven Progressive Matrices IQ test (Raven, 1941).

As reported in Table A.1, we find:

(i) Longer response times in the unbalanced choice block go along with less pronounced concentration bias ($p < 0.01$).
(ii) Performance in the cognitive ability measures is not correlated with the degree of concentration bias.

Taken together, a potential interpretation of these findings is that concentration bias is likely to arise when a seemingly compelling representation of the decision problem at hand comes to mind rapidly.

Table A.1

Relative measure of concentration bias in Main-Treatment and response time and cognitive skills

	OLS					
	(1)	(2)	(3)	(4)	(5)	(6)
$\tilde{d}^{\text{rel}}$ in Main-Treatment	0.224***	0.224***	0.224***	0.224***	0.224***	0.224***
	(0.020)	(0.019)	(0.021)	(0.021)	(0.021)	(0.019)
RT in unbalanced choice		−0.070***				−0.074***
		(0.024)				(0.025)
Math score			−0.009			−0.020
			(0.028)			(0.028)
CRT score				−0.015		−0.009
				(0.021)		(0.024)
Raven score					0.008	0.025
					(0.022)	(0.024)
Age, gender, day, time-of-day FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	100	100	100	100	100	100

Notes: This table presents OLS estimates of the relative measure of concentration bias, $\tilde{d}^{\text{rel}}$, in Main-Treatment, including fixed effects for age, gender, day, and time of day. Columns 2–5 report coefficients for the average response time in the unbalanced choice and measures of math ability, the Cognitive Reflection Test, and the Raven Progressive Matrices test, respectively. Column 6 includes all measures simultaneously. Robust standard errors are included in parentheses. All control variables are standardised. $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

A.4 Extended Sample

By construction, our measurement of concentration bias in the unbalanced trade-off relies on subjects stating indifference points in the balanced trade-offs. We hence preregistered to exclude all subjects with at least one corner choice in the set of balanced trade-offs from our main analyses.

To show that this exclusion restriction does not affect our results, we ask subjects with upper corner choices—always choosing the overtime work plan—to state their indifference point $\tilde{x}_t$ manually (see Figure A.4 in Online Appendix A for a screenshot).[22] We include these subjects in our analyses based on their manually stated indifference points.

- In Table A.2, we report a statistically significant and substantial concentration bias for all estimates of $\tilde{d}$ and $\tilde{d}^{\text{rel}}$.
- In Figure A.5, we depict the distribution of $\tilde{d}^{\text{rel}}$ for our extended sample, which shows that a large fraction of subjects (88.2%) exhibits concentration bias ($\tilde{d}^{\text{rel}} > 0$).
- In Table A.3, we replicate our analyses reported in Section 3 and Section 4 and find similar evidence as reported in Results 2, 3, and 4.

Table A.2
Absolute and relative per-workday deviation from discounted utility in MAIN-TREATMENT

	OLS			Tobit
	Lower bound	Midpoint	Upper bound	
	(1)	(2)	(3)	(4)
$\tilde{d}$ in MAIN-TREATMENT	36.430***	45.120***	53.810***	48.170***
	(2.515)	(3.425)	(4.593)	(4.241)
$\tilde{d}^{\text{rel}}$ in MAIN-TREATMENT	0.190***	0.231***	0.271***	
	(0.013)	(0.017)	(0.022)	
Observations	136	136	136	136

Notes: This table replicates Table 2 including subjects that have been excluded in the main sample (see Section 2.3.4). This table presents estimates of the average absolute and relative per-workday deviation from discounted utility, $\tilde{d}$ and $\tilde{d}^{\text{rel}}$, respectively. Robust standard errors are included in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

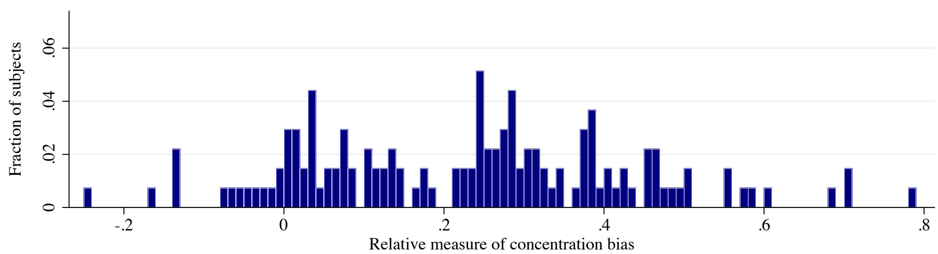


Figure A.5
Distribution of the relative effect of concentration bias ($\tilde{d}^{\text{rel}}$) in MAIN-TREATMENT. This is a replication of Figure 3 but includes those subjects that have been excluded in the main sample (see Section 2.3.4).

22. Note that our test of concentration bias cannot be applied to subjects with lower corner choices (5% of overall subjects), who prefer not to receive a raise even if no additional tasks are required ($x_t = 0$).

Table A.3
Average relative per-workday deviation from standard discounting ($\tilde{d}^{\text{rel}}$) across conditions

	OLS			
	(1)	(2)	(3)	(4)
$\tilde{d}^{\text{rel}}$ in MECHANISM-TREATMENT	0.080***			
	(0.017)			
Difference:	0.151***			
MAIN-TREATMENT − MECHANISM-TREATMENT	(0.024)			
$\tilde{d}^{\text{rel}}$ in DONATION-TREATMENT		0.148***		
		(0.018)		
Difference:		0.158***		
DONATION-TREATMENT − DONATION-CONTROL		(0.020)		
$\tilde{d}^{\text{rel}}$ in MAIN-CONTROL			−0.001	
			(0.010)	
Difference:			0.232***	
MAIN-TREATMENT − MAIN-CONTROL			(0.020)	
$\tilde{d}^{\text{rel}}$ in MECHANISM-CONTROL				−0.013
				(0.010)
Difference:				0.093***
MECHANISM-TREATMENT − MECHANISM-CONTROL				(0.020)
Observations	259	250	271	248

Notes: This table replicates Table 3 including subjects that have been excluded in the main sample (see Section 2.3.4). Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.5 Alternative Relative Measure

The results based on our relative measure of concentration bias as stated in Table 2, Figure 3, and Table 3 are robust to using a different specification (which was also preregistered) of the relative measure. The different specification is:

$$\tilde{d}^{\text{rel, alt}} := \frac{8\tilde{d}}{\sum_{t=1}^{8} e_t + \tilde{x}_t}. \tag{A.1}$$

- In Table A.4, we report substantial and statistically significant concentration bias based on $\tilde{d}^{\text{rel, alt}}$ for Main-Treatment.
- In Figure A.6, we depict the distribution of $\tilde{d}^{\text{rel, alt}}$ for Main-Treatment.
- In Table A.5, we show the results on $\tilde{d}^{\text{rel, alt}}$ across the other conditions of our consumption experiment.

Our findings based on $\tilde{d}^{\text{rel, alt}}$ yield the same conclusions regarding concentration bias and its mechanisms and robustness as our results based on $\tilde{d}^{\text{rel}}$.

Table A.4

Relative measure of concentration bias in Main-Treatment (based on alternative measure, $\tilde{d}^{\text{rel, alt}}$)

	OLS		
	(1) Lower bound	(2) Midpoint	(3) Upper bound
$\tilde{d}^{\text{rel,alt}}$ in Main-Treatment	0.188*** (0.016)	0.223*** (0.021)	0.257*** (0.027)
Observations	100	100	100

Notes: This table presents estimates of the average relative per-workday deviation from discounted utility based on the alternative measure, $\tilde{d}^{\text{rel, alt}}$. Robust standard errors are in parentheses. The sample includes all observations from Main-Treatment. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

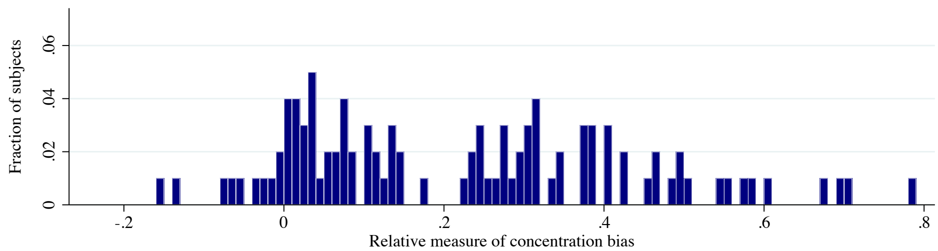


Figure A.6

Distribution of the relative measure of concentration bias ($\tilde{d}^{\text{rel, alt}}$) in Main-Treatment. This is a replication of Figure 3 but uses the alternative measure $\tilde{d}^{\text{rel, alt}}$, see equation (A.1), instead of $\tilde{d}^{\text{rel}}$.

Table A.5

Average relative measure of concentration bias across conditions (based on alternative measure, $\tilde{d}^{\text{rel, alt}}$)

	OLS			
	(1)	(2)	(3)	(4)
$\tilde{d}^{\text{rel, alt}}$ in MECHANISM-TREATMENT	0.075***			
	(0.019)			
Difference:	0.148***			
MAIN-TREATMENT − MECHANISM-TREATMENT	(0.028)			
$\tilde{d}^{\text{rel, alt}}$ in DONATION-TREATMENT		0.134***		
		(0.021)		
Difference:		0.158***		
DONATION-TREATMENT − DONATION-CONTROL		(0.022)		
$\tilde{d}^{\text{rel, alt}}$ in MAIN-CONTROL			−0.025***	
			(0.008)	
Difference:			0.248***	
MAIN-TREATMENT − MAIN-CONTROL			(0.023)	
$\tilde{d}^{\text{rel, alt}}$ in MECHANISM-CONTROL				−0.016
				(0.010)
Difference:				0.091***
MECHANISM-TREATMENT − MECHANISM-CONTROL				(0.021)
Observations	200	200	200	200

Notes: Robust standard errors are in parentheses. The sample includes all observations from the mentioned (between-subjects) conditions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.6 Results Based on Absolute Measure

The results based on our relative measure of concentration bias as stated in Figure 3 and Table 3 are robust to using our absolute measure of concentration bias, $\tilde{d}$:

- In Figure A.7, we depict the distribution of $\tilde{d}$ for MAIN-TREATMENT.
- When using our absolute measure and employing OLS (see Table A.6) and Tobit (see Table A.7) regressions, we replicate our analyses reported in Section 3 and Section 4 and find similar evidence as reported in Results 2, 3, and 4.

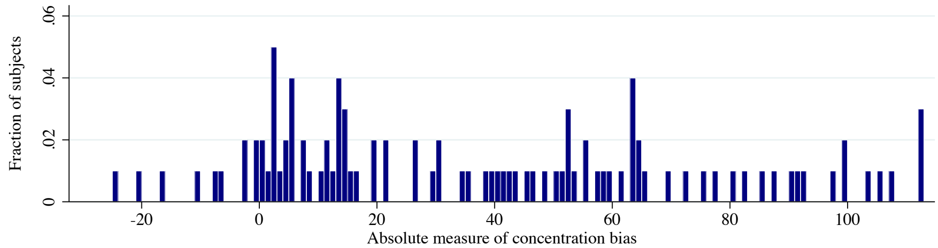


Figure A.7
Distribution of the absolute measure of concentration bias ($\tilde{d}$) in MAIN-TREATMENT. This is a replication of Figure 3 but uses the absolute and not the relative measure of concentration bias. We use our midpoint measure of $\tilde{d}$ (see Section 2.3.5).

Table A.6
Average absolute measure of concentration bias across conditions (based on OLS regressions)

	OLS			
	(1)	(2)	(3)	(4)
$\tilde{d}$ in MECHANISM-TREATMENT	13.83***			
	(3.28)			
Difference:	23.79***			
MAIN-TREATMENT − MECHANISM-TREATMENT	(4.85)			
$\tilde{d}$ in DONATION-TREATMENT		23.71***		
		(3.64)		
Difference:		28.25***		
DONATION-TREATMENT − DONATION-CONTROL		(3.89)		
$\tilde{d}$ in MAIN-CONTROL			−4.54***	
			(1.50)	
Difference:			42.15***	
MAIN-TREATMENT − MAIN-CONTROL			(3.88)	
$\tilde{d}$ in MECHANISM-CONTROL				−2.64
				(1.74)
Difference:				16.47***
MECHANISM-TREATMENT − MECHANISM-CONTROL				(3.71)
Observations	200	200	200	200

Notes: We use our midpoint measure of $\tilde{d}$ (see Section 2.3.5). Robust standard errors are in parentheses. The sample includes all observations from the mentioned (between-subjects) conditions. $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Table A.7
Absolute measure of concentration bias across conditions (based on Tobit regressions)

	Tobit			
	(1)	(2)	(3)	(4)
$\tilde{d}$ in Mechanism-Treatment	13.10^{***}			
	(3.37)			
Difference:	23.72^{***}			
Main-Treatment − Mechanism-Treatment	(4.85)			
$\tilde{d}$ in Donation-Treatment		22.22^{***}		
		(2.66)		
Difference:		26.85^{***}		
Donation-Treatment − Donation-Control		(3.72)		
$\tilde{d}$ in Main-Control			-4.62^{**}	
			(2.47)	
Difference:			39.58^{***}	
Main-Treatment − Main-Control			(3.54)	
$\tilde{d}$ in Mechanism-Control				-2.73
				(2.56)
Difference:				15.65^{***}
Mechanism-Treatment − Mechanism-Control				(3.63)
Observations	200	200	200	200

Notes: We use our lower bound measure of $\tilde{d}$ (see Section 2.3.5). Robust standard errors are in parentheses. The sample includes all observations from the mentioned (between-subjects) conditions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Elicitation of Indifference Points: Examples

In order to illustrate the procedure described in Section 2.3.3, this section describes the steps of eliciting the indifference points in the balanced trade-offs and in the unbalanced trade-off in detail, via two numerical examples.

Balanced trade-offs. Each indifference point of a balanced trade-off j is elicited via a series of binary choices between a baseline work plan $\boldsymbol{c}^{\mathrm{b},j}$ and an overtime work plan $\boldsymbol{c}^{\mathrm{o},j}$ that varies from choice to choice. The two work plans are presented to subjects as Alternative B and Alternative A, respectively, see Figure 1. Each individual choice features a number of additional tasks x_t by which the overtime work plan exceeds the baseline work plan on one workday $t = j$, where $x_t \in \{0, 1, 2, \dots, 125\}$. This increase in the number of tasks is accompanied by a raise r^j by which the value of the restaurant voucher in $\boldsymbol{c}^{\mathrm{o},j}$ exceeds the value v^j in $\boldsymbol{c}^{\mathrm{b},j}$:

$$\begin{aligned}
\boldsymbol{c}^{\mathrm{b},j} &= (-e_1, \dots, -e_{j-1}, -e_j, \qquad -e_{j+1}, \dots, -e_8, v^j) && \text{and} \\
\boldsymbol{c}^{\mathrm{o},j} &= (-e_1, \dots, -e_{j-1}, -e_j - x_j, -e_{j+1}, \dots, -e_8, v^j + r^j) && \text{with} \quad j \in \{1, \dots, 8\}.
\end{aligned}$$

To minimise the number of individual choices, we apply an iterative procedure and distinguish *direct* from *indirect* choices. Direct choices are actively made by subjects, whereas indirect choices result from these direct choices: If a subject agrees to work an additional number of tasks x_t in a direct choice, she indirectly also agrees to work any additional number of tasks less than x_t. Analogously, if a subjects declines to work an additional number of tasks x_t in a direct choice, she also indirectly declines to work any larger number of tasks. For the first of the direct binary choices, we randomly draw an integer x_t from the interval $[0, 125]$. Once the subject has made the first direct choice, the interval for the next direct choice is adjusted accordingly: if the subject accepted the additional workload, the lower bound is increased to x_t such that the new interval is $[x_t, 125]$; if the additional workload was declined, the upper bound is set to $x_t - 1$ such that the new interval is $[0, x_t - 1]$. In the next direct choice, the midpoint of the new interval is presented to the subject as the additional number x_t of tasks included in the overtime work plan. This procedure of nested intervals reduces the range covered by the interval from direct choice to direct choice and is repeated until the interval consists of a single integer, which is the elicited indifference point $\tilde{x}_t$.

Numerical example. In the following, we describe each step of an exemplary balanced choice to illustrate this procedure. Assume that the subject is indifferent between working $e_3 + \tilde{x}_3 = 181$ tasks on day 3 and receiving a restaurant voucher of $v^3 + r^2 = 16.20$ euros, and working $e_3 = 143$ tasks on day 3 and receiving a restaurant voucher of $v^3 = 13.10$ euros. That is, she is willing to work an additional number of $\tilde{x}_3 = 38$ tasks in exchange for the increase in the value of the voucher.

For the first direct choice, we randomly draw an integer between 0 and 125. Assume this random draw to be 12, which yields a total of 155 tasks for Alternative A. The exemplary subject is willing to work this additional number of tasks ($155 < 181$) and chooses Alternative A. This first direct choice implies that the subject also prefers any number of additional tasks smaller than 12 and, hence, *indirectly* chooses Alternative A for these numbers as well. We now select the midpoint of the remaining interval $[12, 125]$, which is 68, to be the number of additional tasks for the next direct choice. Having the choice between 211 (> 181) or 143 tasks on day 3, the exemplary subject selects the latter, Alternative B. With this direct choice, any number of additional tasks larger than 68 is declined as well, reducing the remaining interval to $[12, 67]$. Applying the same procedure, the next direct choice is between 183 (> 181) and 143 tasks on day 3, where the subject again chooses Alternative B, reducing the interval to $[12, 39]$. In the next direct choice, the subject faces Alternative A with 169 (< 181) tasks which she selects, and the interval shrinks to $[26, 39]$. In the next two choices, the subject chooses Alternative A with 176 and 179 tasks, reducing the interval to $[33, 39]$ and $[36, 39]$. In the seventh choice, Alternative A requires 181 tasks, which is the assumed indifference point that we defined to be the highest number of additional tasks that a subject is willing to work. Therefore, Alternative A is chosen, reducing the interval to $[38, 39]$. The final choice displays 182 tasks for Alternative A, which is declined, yielding an elicited indifference point of $e_3 + \tilde{x}_3 = 181$ total or $\tilde{x}_3 = 38$ additional tasks on day 3.

Unbalanced trade-off. Like the balanced trade-offs, the unbalanced trade-off consists of a series of binary choices between a baseline work plan, $\boldsymbol{c}^{\mathrm{b}}$, and an overtime work plan, $\boldsymbol{c}^{\mathrm{o}}$. In the unbalanced trade-off, a number of tasks $d \in \{-63, -62, \ldots, -1, 0, 1, \ldots, 61, 62\}$ is added to the indifference points $\tilde{\boldsymbol{x}} = (\tilde{x}_1, \ldots, \tilde{x}_8)$ that have been elicited in the balanced trade-offs (and are predicted by standard discounting, see Section 2.2). This is accompanied by a raise $R = \sum_{j=1}^{8} r^j$, by which the value of the restaurant voucher in $\boldsymbol{c}^{\mathrm{o}}$ exceeds its value v in $\boldsymbol{c}^{\mathrm{b}}$:

$$\begin{aligned}
\boldsymbol{c}^{\mathrm{b}} &= (-e_1, \qquad -e_2, \qquad \ldots, -e_7, \qquad -e_8, \qquad v) \qquad \text{and} \\
\boldsymbol{c}^{\mathrm{o}} &= (-e_1 - \tilde{x}_1 - d, -e_2 - \tilde{x}_2 - d, \ldots, -e_7 - \tilde{x}_7 - d, -e_8 - \tilde{x}_8 - d, v + R).
\end{aligned}$$

Numerical example. For the unbalanced trade-off, assume that the exemplary subject is willing to work $\tilde{d} = 37$ additional tasks on each of the eight work days on top of $\tilde{\boldsymbol{x}}$ when being offered a voucher of value $v + R = 31.30$ versus a voucher of value $v = 7.50$ in the baseline work plan.

In analogue to the elicitation in the balanced trade-offs, we randomly draw a number from the initial interval, here $[-63, 62]$, for the first direct choice. Assume for this example that the random draw is $d = -36$. This number is added to the predicted indifference point of standard discounting. Using the elicited indifference point $\tilde{x}_3 = 38$ tasks from the example balanced trade-off, Alternative A would display $181 - 36 = 145$ tasks on day 3, compared to 141 tasks for Alternative B. Since $-36 < 37$, the subject chooses Alternative A. This reduces the interval to $[-36, 62]$ for the next direct choice. Again, we take the midpoint, 13, of the new interval, in order to minimize the number of choices that need to be made directly, and set $d = 13$ for the second direct choice. Since $13 < 37$, the subject again prefers Alternative A, resulting in the interval $[13, 62]$ for the third interval. This interval's midpoint is 37. This equals the exemplary subject's willingness to work, $\tilde{d}$, so that she chooses Alternative A. The interval thereby shrinks to $[37, 62]$ for the next choice. From here on, 37 will be the lower bound of the intervals. Therefore, in all subsequent direct choices, the midpoint is greater than 37, and the subject always chooses Alternative B. These choices lower the upper bound of the interval step by step. More precisely, the remaining midpoints are 49 additional tasks, giving rise to a new interval of $[37, 48]$; 43 additional tasks, giving rise to a new interval of $[37, 42]$; and 40 additional tasks, giving rise to a new interval of $[37, 39]$. In the final choice, the subject declines 38 additional tasks, so that 37 is the highest number of additional tasks for which the subject chooses Alternative A. This number is thus considered the indifference point $\tilde{d} = 37$ for the unbalanced trade-off.

C Convex Time Budgets: Details

In this appendix, we describe and discuss the conditions Money-Main and Money-Mechanism of our money experiment in detail. We first present the experimental design in Section C.1 and derive behavioural predictions from discounted utility and from the focusing model (Kőszegi and Szeidl, 2013) in C.2. We report and discuss the findings for Money-Main in Section C.3 and for Money-Mechanism in C.4.

Compared to our brief discussion of the money experiment in Section 5, we use a more nuanced terminology in this appendix. In Section 5, we only use the terms "balanced" and "unbalanced". The latter term, however, refers to two different types of trade-offs in the context of our money experiment: it can be either the earlier or the later payoff that is dispersed, while the respective other payoff is concentrated. To distinguish between the two types of unbalanced trade-offs, we introduce the labels Disp-Conc and Conc-Disp. The corresponding balanced trade-offs are denoted by Conc-Conc.

C.1 Design of the experiment

C.1.1. Intertemporal decisions. This experiment investigates intertemporal decisions that involve monetary consequences on multiple dates. Each subject repeatedly allocates monetary budgets between an earlier and a later payoff. One of the allocations is randomly chosen to be payoff-relevant. The trade-offs that subjects face in their decisions are of different types: either both payoffs are concentrated on a single payment date ("balanced": Conc-Conc), or one of the two payoffs is dispersed over multiple (2, 4, or 8) dates ("unbalanced": Conc-Disp, Disp-Conc). Figures C.1 and C.2 illustrate the budget sets.

We identify concentration bias by comparing which share of their budgets subjects allocate to the later payoff in the balanced and unbalanced decisions. Subjects decide for each budget set by how much they decrease earlier payments at the benefit of increasing later payments. The sum total is the greater, the more money subjects allocate to later payment dates. Put differently, we implement an intertemporal budget constraint with a strictly positive nominal interest rate, r. Each earnings sequence specified by an allocation from a budget set consists of 9 money transfers to the subject's bank account on 9 given dates in the future. In doing so, we extend the "Convex Time Budget" approach (Andreoni and Sprenger, 2012) to settings in which subjects face more than two payment dates.

Across trials, we vary within-subject the characteristics of the intertemporal budget constraints. We thereby implement the decision types Conc-Conc, Conc-Disp, and Disp-Conc. Irrespective of their decision, subjects receive a fixed amount of €1 on each of the 9 payment dates, in order to hold the number of transfers constant across conditions.[23] On top of these fixed payments, subjects allocate a budget B between several payment dates. In Conc-Conc, the allocation is between exactly two payment dates; the intertemporal allocation thus involves payoffs each of which is concentrated on a single payment date. Decreasing a payoff increases a payoff on exactly *one* other date. By contrast, in Conc-Disp and Disp-Conc, one payoff is concentrated on a single date, while the other payoff is dispersed over multiple dates. Decreasing (increasing) the concentrated payoff increases (decreases) the payments on *several* (2, 4, or 8) other dates. To give an example, the earnings sequences in a balanced condition Conc-Conc are

$$\Big(1+B(1-x),\quad 1,\quad 1,\quad 1,\quad 1,\quad 1,\quad 1,\quad 1,\quad 1+RBx\Big),$$

while the associated earnings sequences in Conc-Disp are

$$\Big(1+B(1-x),\ 1+\tfrac{RBx}{8},\ 1+\tfrac{RBx}{8},\ 1+\tfrac{RBx}{8},\ 1+\tfrac{RBx}{8},\ 1+\tfrac{RBx}{8},\ 1+\tfrac{RBx}{8},\ 1+\tfrac{RBx}{8},\ 1+\tfrac{RBx}{8}\Big).$$

We denote by x the subject's choice variable: the share of the budget B that she "saves", that is, allocates to the later payoff, $x \in X$ with $X = \{0, {}^1/_{100}, {}^2/_{100}, \dots, 1\}$.

23. This is the modern way to control for transaction costs (Andreoni and Sprenger, 2012, p. 3339). In addition, each subject receives two individualized e-mail messages after the experiment that include a complete listing of all payments. Before making any decisions in the experiment, the written instructions inform subjects in detail about these two messages. Hence, subjects know in advance that they will be informed when exactly they have to inspect their bank statements to check that they have received the promised amounts.

	w weeks			w weeks					t
Conc-Conc	$1 + B(1-x)$	1	1	1	1	1	1	1	$1 + RBx$
Conc-Disp$_2$	$1 + B(1-x)$	1	1	1	1	1	1	$1 + RBx/2$	$1 + RBx/2$
Conc-Disp$_4$	$1 + B(1-x)$	1	1	1	1	$1 + RBx/4$	$1 + RBx/4$	$1 + RBx/4$	$1 + RBx/4$
Conc-Disp$_8$	$1 + B(1-x)$	$1 + RBx/8$	$1 + RBx/8$	$1 + RBx/8$	$1 + RBx/8$	$1 + RBx/8$	$1 + RBx/8$	$1 + RBx/8$	$1 + RBx/8$

Figure C.1
Budget Sets Conc-Conc and Conc-Disp

	w weeks			w weeks				7 months	t
Conc-Conc	1	1	1	1	1	1	1	$1 + B(1-x)$	$1 + RBx$
Disp$_2$-Conc	1	1	1	1	1	1	$1 + \frac{B(1-x)}{2}$	$1 + \frac{B(1-x)}{2}$	$1 + RBx$
Disp$_4$-Conc	1	1	1	1	$1 + \frac{B(1-x)}{4}$	$1 + \frac{B(1-x)}{4}$	$1 + \frac{B(1-x)}{4}$	$1 + \frac{B(1-x)}{4}$	$1 + RBx$
Disp$_8$-Conc	$1 + \frac{B(1-x)}{8}$	$1 + \frac{B(1-x)}{8}$	$1 + \frac{B(1-x)}{8}$	$1 + \frac{B(1-x)}{8}$	$1 + \frac{B(1-x)}{8}$	$1 + \frac{B(1-x)}{8}$	$1 + \frac{B(1-x)}{8}$	$1 + \frac{B(1-x)}{8}$	$1 + RBx$

Figure C.2
Budget Sets Conc-Conc and Disp-Conc

Notes: The figures depict all types of budget allocations that subjects face. They allocate a budget B between an earlier and a later payoff. In the upper part, the earlier payoff is concentrated on the first date, and the later payoff is either concentrated on the last date or dispersed over the n last dates. In the lower part, the later payoff is concentrated at the last date, and the earlier payoff is either concentrated at the second-to-last date or dispersed over n earlier dates. For the values of B, R, and w that we used, see Section C.1.3. The savings rate x is subjects' choice variable: they select one $x \in X = \{0, {}^{1}\!/_{100}, {}^{2}\!/_{100}, \ldots, 1\}$ in each trial.

Conc-Conc consists of two types of budget sets, linked to either Conc-Disp or Disp-Conc:

- In the type linked to Conc-Disp, subjects can shift money from the earliest to the last payment date.
- In the type linked to Disp-Conc, subjects allocate money between the second-to-last and the last payment date.

In every Conc-Conc decision, subjects receive B euros if they allocate their additional payment to the earlier date. If they allocate it to the later date, they receive RB euros, with $R := 1 + r > 1$. They can also choose convex combinations of payments by choosing $x \in X = \{0, {}^{1}/_{100}, {}^{2}/_{100}, \ldots, 1\}$, which gives rise to an earlier payment of $B(1-x)$ euros and a later payment of RBx euros. While each payment date is separated by w weeks for the first type, matching the Conc-Disp decisions, this is true only for the first 8 payment dates of the second type, matching the Disp-Conc decisions. Here, the distance between the second-to-last and last payment date is 7 months. We chose this large gap between $t = 8$ and $t = 9$ in order to minimize ceiling effects, that is, in order to avoid a situation in which subjects exclusively choose the largest, latest payment.

For the unbalanced budget sets, Conc-Disp and Disp-Conc, either the later or earlier payoff is dispersed over $n \in \{2, 4, 8\}$ payment dates:

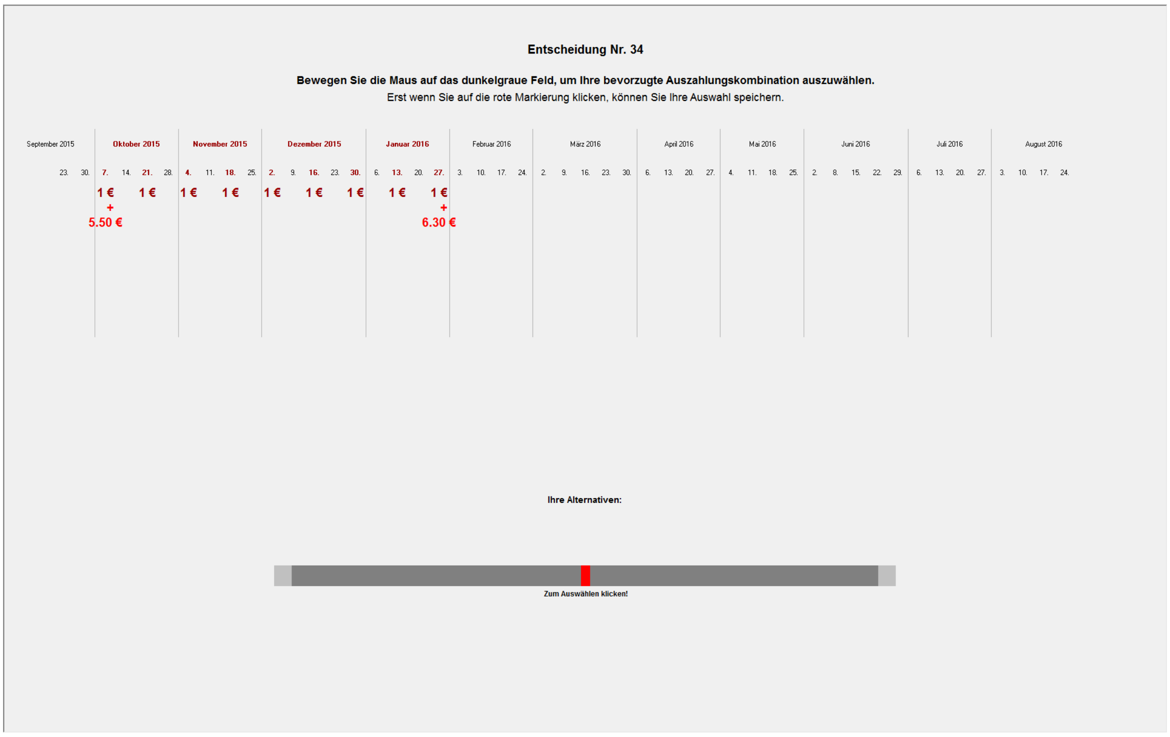
- In Conc-Disp, subjects allocate monetary amounts between the earliest payment date and the last n payment dates over which the later payoff is dispersed.
- In Disp-Conc, subjects allocate money between n earlier payments and a single later payment. That is, the amount of $B(1-x)$ euros paid at the second-to-last date in Conc-Conc is now dispersed over the second-to-last payment date and $n-1$ earlier dates in Disp-Conc.

In a first step, we are interested in the comparison of chosen allocations between Conc-Conc and Conc-Disp. This comparison tests whether subjects behave differently in the case that the benefits of choosing a high savings rate x are dispersed over multiple future dates (Conc-Disp) than when they are sconcentrated on a single future date (Conc-Conc). Concentration bias predicts that individuals underweight dispersed consequences relative to concentrated consequences. In Conc-Disp, the benefits of behaving *patiently* attract little attention, because they are dispersed in the form of comparatively small payments over several dates. By contrast, in Conc-Conc, the benefit of behaving *patiently* is concentrated in a single, comparatively large—that is, attention-grabbing—payment. Thus, individuals are predicted to pay *less* attention to the later payoff in Conc-Disp than in Conc-Conc, which promotes a *lower* savings rate in Conc-Disp than in Conc-Conc. Figure C.3 shows an exemplary decision screen with $B = €11$ and $r \approx 15\%$ for both Conc-Conc (upper panel) and Conc-Disp with $n = 8$ (lower panel). Through a slider, subjects choose their preferred $x \in X$.[24]

In a second step, we are also interested in the comparison of allocation decisions between Conc-Conc and Disp-Conc. Since the benefits of being *impatient*, that is, of choosing a small x, are dispersed in Disp-Conc, individuals tend to neglect them according to concentration bias. By contrast, the benefit of behaving *impatiently* is concentrated in a single—that is, attention-grabbing—payment in Conc-Conc. Therefore, concentration bias predicts that individuals pay *less* attention to the earlier payoff in Disp-Conc than in Conc-Conc, which promotes a *higher* savings rate in the former condition. Figure C.4 shows the decision screen of an exemplary decision with $B = €11$ and $r \approx 15\%$ for both Conc-Conc (upper panel) and Disp-Conc (lower panel).

C.1.2. Decision times, Cognitive Reflection Test, and mental-arithmetic task. Similarly to Table A.1 in Online Appendix A, we test for heterogeneity of the concentration-bias effect. To do so, we collect the time that subjects take to make their decision, apply the Cognitive Reflection Test (CRT; Frederick, 2005), and let subjects complete a math task in which they are asked to calculate as many sums as possible within five minutes. Each sum consists of 4 to 9 decimal numbers and is rewarded with €0.20 if correctly solved. If a subject does not enter the correct sum within three attempts, €0.05 are deducted from her earnings. To avoid negative earnings, subjects receive an initial endowment of €1.

24. The slider has no initial position—it appears only after subjects first position the mouse cursor over the slider bar. This is done to avoid default effects.



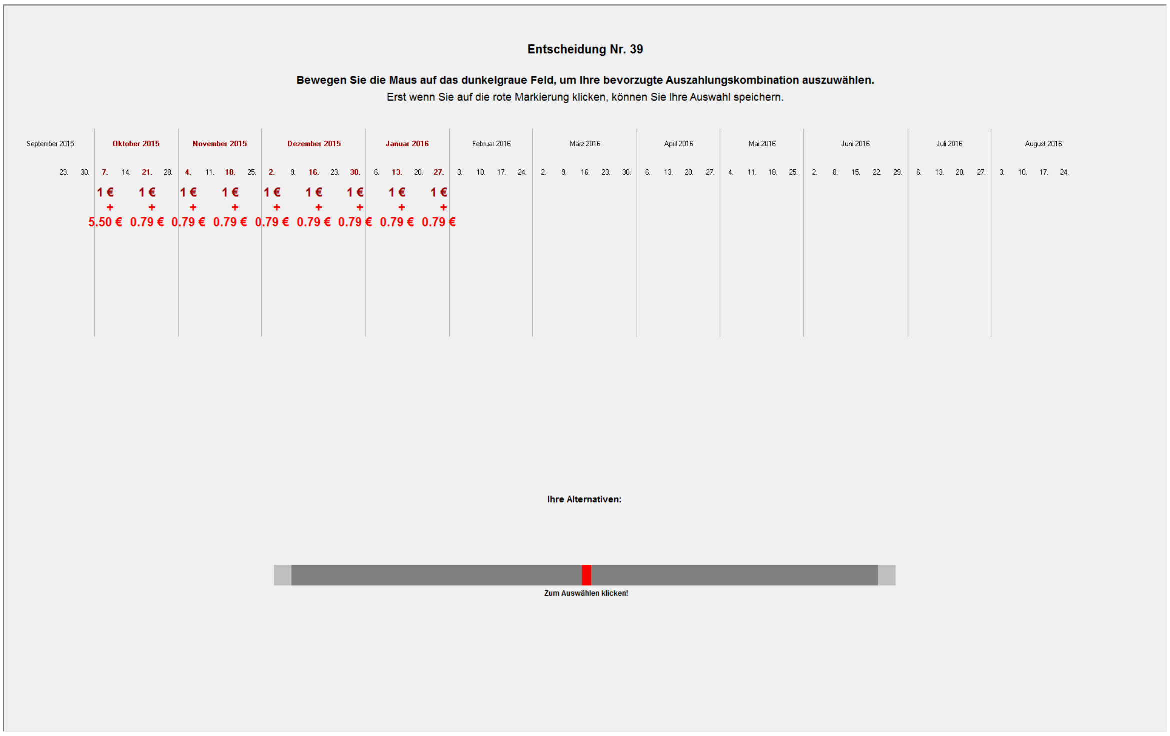
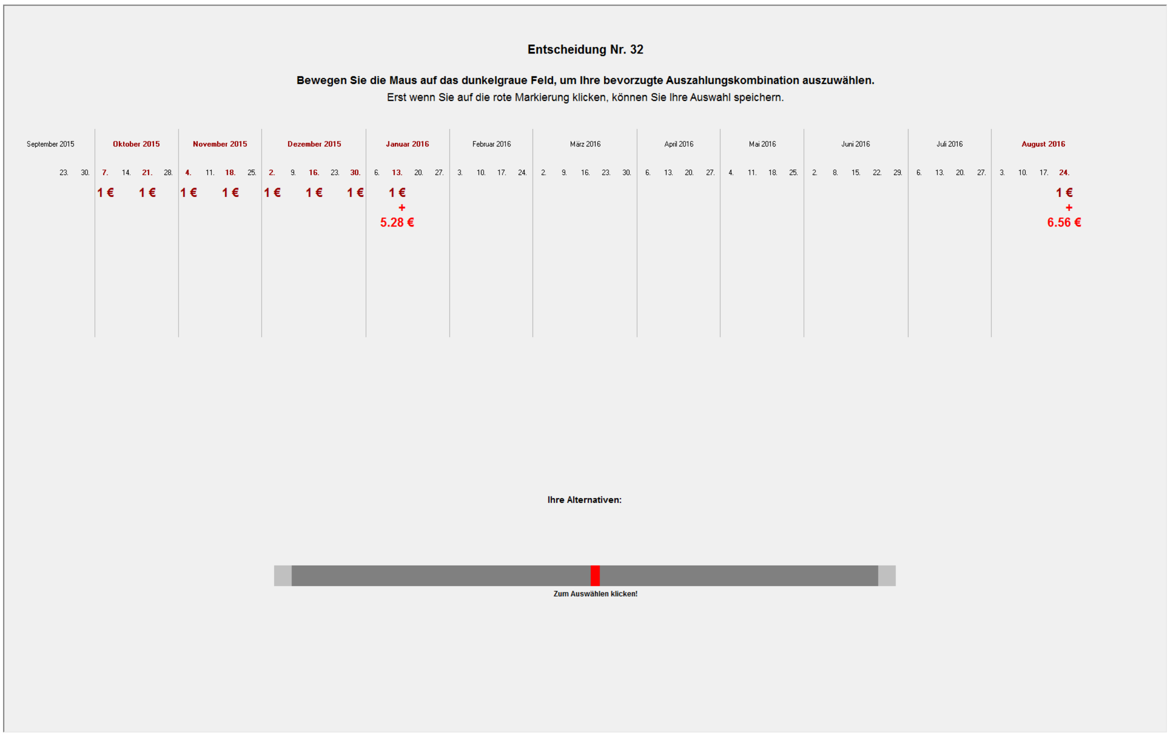


Figure C.3

Screenshots of a Conc-Conc decision (top) and of a Conc-Disp decision (bottom). Here, $x = {}^{50}/_{100}$ of the budget B is shifted to the later payoff. The later payoff is concentrated on the last date in Conc-Conc and dispersed over $n = 8$ dates in the depicted Conc-Disp trade-off.

C.1.3. Procedure. The experiment was conducted in two waves at the BonnEconLab.

In the first wave, each subject made 36 choices across different budget sets. One set of subjects ($N = 47$) faced 12 budget sets ($B = 8$, $r \in \{20\%, 50\%, 80\%\}$; $B = 11$, $r \in \{15\%, 36\%, 58\%\}$; $w \in \{3, 6\}$) of the type Conc-Disp for $n = 4$ or 8, and the corresponding Conc-Conc decisions. A second set of subjects ($N = 46$) faced the same parameters for Disp-Conc, again for $n = 4$ or 8, and the corresponding Conc-



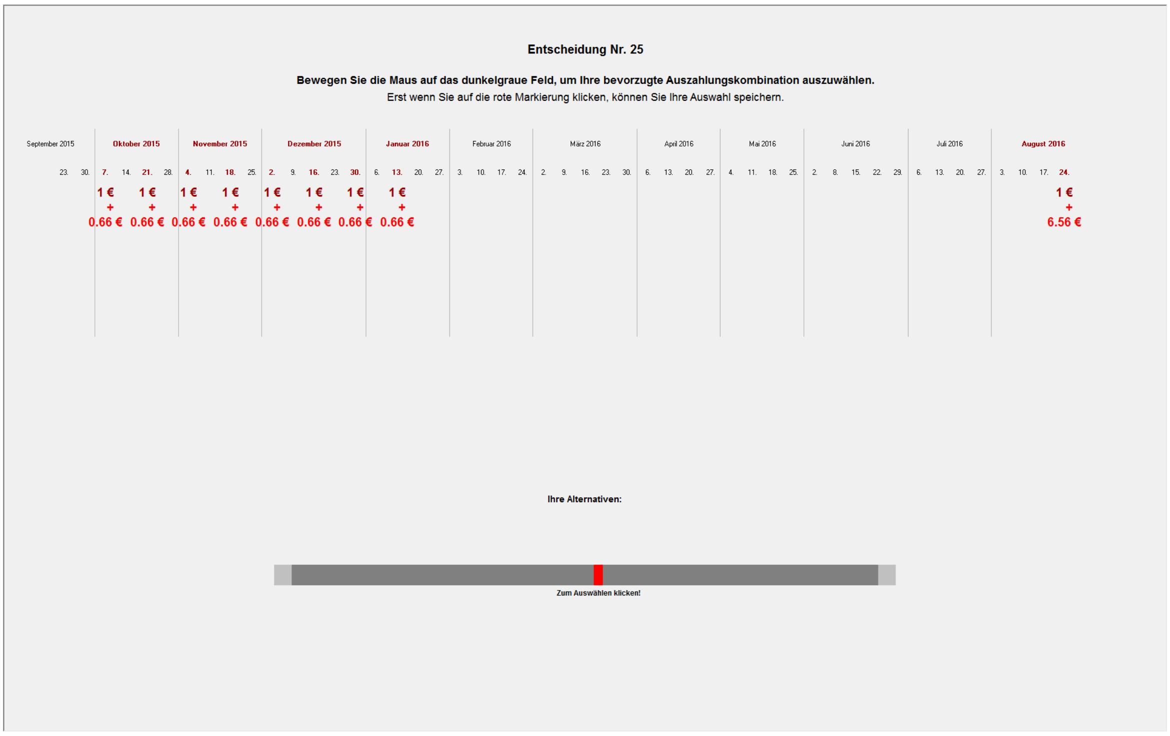


Figure C.4

Screenshots of a Conc-Conc decision (top) and of a Disp-Conc decision (bottom). Here, $x = {}^{48}/_{100}$ of the budget B is shifted from the earlier payoff. The earlier payoff is concentrated on the second-to-last date in Conc-Conc and dispersed over $n = 8$ dates in the depicted Disp-Conc trade-off.

Conc budget sets. That is, in the first wave, Conc-Disp and Disp-Conc decisions varied between subjects. (Note, however, that the relevant balanced-versus-unbalanced comparisons are all within-subject.)

In the second wave, we combined Conc-Disp and Disp-Conc decisions within-subject.[25] All sub-

25. In the first wave, participation in any of the two comparisons was randomized between-subjects. This is because we initially considered including an interest rate as high as $r = 80\%$ reasonable, given that in previous studies,

jects ($N = 92$) made 32 choices as follows: they were given four budget sets ($B = 11$; $r \in \{15\%, 58\%\}$; $w \in \{2, 3\}$) of each of the two Conc-Conc and the six associated Conc-Disp and Disp-Conc types ($n \in \{2, 4, 8\}$ for each).

The order in which the budget sets were presented was randomized per subject. All sessions took place on a Thursday or Friday. The time line on the screen always started on next week's Wednesday. The earliest bank transfer for any earnings sequence was on that Wednesday or on the Wednesday two or three weeks later. Thus, subjects' earnings sequences always started at least 5 or 6 days in the future.

Subjects were also asked to choose between additional earnings sequences presented in the form of choice lists (24 in the first, 28 in the second wave). These test for concentration bias in a slightly different manner. We do not analyse the choice lists in this paper but refer to Section 3.B of Schwerter (2016).

Each session of the experiment lasted 90 minutes. Subjects earned on average €21.61. We used z-Tree (Fischbacher, 2007) for conducting the experiment and hroot (Bock et al., 2014) for the invitations.

C.2 Predictions

In the following, we derive predictions for discounted utility as well as for the focusing model. We assume that individuals base their decisions on utility derived from receiving monetary payments c_t at various dates t. Additionally, we make the standard assumption that utility from money is increasing in its argument but not convex: $u'(c_t) \geq 0$ and $u''(c_t) \leq 0$. The assumption regarding the curvature of the utility function is in line with previous findings in the literature. Andreoni and Sprenger (2012) and Augenblick et al. (2015), for instance, estimate that utility in money is concave (albeit close to linear). Regarding the aggregate utility functions for discounted utility and for focus-weighted utility, please see Section 2.1.

C.2.1. Discounted utility. The decision maker chooses how much to allocate to the different periods by maximizing her utility over all possible earnings sequences available within a given budget set. We use the superscript $^{\text{DU}}$ to indicate decisions based on discounted utility.

Conc-Conc vs. Conc-Disp. In Conc-Conc, the decision maker decides how much to allocate to the different payment dates by choosing

$$x^{\text{DU}}_{\text{C-C}} \;:=\; \arg\max_{x\in X}\left\{D(1)u(1+B(1-x))+\sum_{t=2}^{8}D(t)u(1)\;+\;D(9)u(1+RBx)\right\}.$$

In Conc-Disp, the later payoff is dispersed over the last n payment dates. The decision maker therefore chooses

$$x^{\text{DU}}_{\text{C-D}} \;:=\; \arg\max_{x\in X}\left\{D(1)u(1+B(1-x))+\sum_{t=2}^{9-n}D(t)u(1)+\sum_{t=9-n+1}^{9}D(t)u(1+RBx/n)\right\}.$$

Since $D'(t) \leq 0$ and $u''(\cdot) \leq 0$—as well as $D(t) \geq 0$, $0 \leq x \leq 1$, $B \geq 0$, $R \geq 1$, and $u'(\cdot) > 0$—the following holds: The marginal negative consequences of being patient, that is, of increasing x, are the same across Conc-Conc and Conc-Disp,

$$D(1)u'(1+B(1-x))\times(-B),$$

while the marginal benefits of increasing x are weakly smaller in Conc-Conc than in Conc-Disp,

$$D(9)u'(1+RBx)\times RB \;\leq\; \sum_{t=9-n-1}^{9}D(t)u'(1+RBx/n)\times RB/n.$$

participants had exhibited extremely strong discounting. It turned out, however, that this led to ceiling effects. In response to this, we decided not to include such extreme trials in the second wave. Instead, we used the time freed up to let all subjects in the second wave participate in both comparisons. This is unproblematic because all balanced–unbalanced comparisons are within-subject comparisons in both waves. Moreover, with virtually the same number of subjects in the two comparisons during the first wave (47 and 46), calculating averages across both comparisons does not suffer from unequal group sizes. Please note that the findings regarding balanced–unbalanced differences that we present below are rather conservative due to the mentioned ceiling effects.

This effect is driven both by the (weak) concavity of the utility function u and the fact that in CONC-DISP, parts of the benefits occur earlier and are, thus, discounted less than in CONC-CONC. Therefore, the decision maker allocates at least as much money to later payment dates in CONC-DISP as in CONC-CONC.

We thus have the prediction

$$d^{\mathrm{DU}}_{\text{C-D}} \;:=\; x^{\mathrm{DU}}_{\text{C-C}} - x^{\mathrm{DU}}_{\text{C-D}} \;\leq\; 0. \tag{C.1}$$

Moreover, $d^{\mathrm{DU}}_{\text{C-D[8]}} \leq d^{\mathrm{DU}}_{\text{C-D[4]}} \leq d^{\mathrm{DU}}_{\text{C-D[2]}} \leq 0$. Here, d stands for "difference" and $[n]$ refers to the degree of dispersion.

CONC-CONC VS. DISP-CONC. In analogy to above, we denote the decision maker's optimal choices by $x^{\mathrm{DU}}_{\text{C-C}}$ and $x^{\mathrm{DU}}_{\text{D-C}}$, respectively. In DISP-CONC, the second-to-last payoff is dispersed over n earlier dates. Here, the marginal negative consequences of increasing x are greater in absolute terms in DISP-CONC than in CONC-CONC. This effect is, again, driven both by the (weak) concavity of the utility function u and the fact that in DISP-CONC, parts of the negative consequences occur earlier and are thus discounted less. This induces the decision maker to save at most as much in DISP-CONC as in CONC-CONC under discounted utility.

Discounted utility then predicts that

$$d^{\mathrm{DU}}_{\text{D-C}} \;:=\; x^{\mathrm{DU}}_{\text{D-C}} - x^{\mathrm{DU}}_{\text{C-C}} \;\leq\; 0. \tag{C.2}$$

Since discounting is the least for the greatest dispersion, we have $d^{\mathrm{DU}}_{\text{D[8]-C}} \leq d^{\mathrm{DU}}_{\text{D[4]-C}} \leq d^{\mathrm{DU}}_{\text{D[2]-C}} \leq 0$.

C.2.2. Concentration bias. As explained in Section 2.1, focusing extends the standard model of discounted utility by a weighting function g that can cause a disproportionate focus on a single period.

CONC-CONC VS. CONC-DISP. For CONC-CONC, date-1 utility ranges from $u_1(1)$ to $u_1(1+B)$, while date-9 utility ranges from $u_9(1)$ to $u_9(1+RB)$. For CONC-DISP, date-1 utility also ranges from $u_1(1)$ to $u_1(1+B)$. However, date-9 utility ranges only from $u_9(1)$ to $u_9(1+RB/n)$. Thus, date-9 utility receives a lower weight in CONC-DISP than it receives in CONC-CONC, $g_{9,\text{C-C}} > g_{9,\text{C-D}}$. In fact, the larger the degree of dispersion, the smaller is the difference $\max u_9 - \min u_9$, and thus the lower is the weight, that is, $g_{9,\text{C-D[2]}} > g_{9,\text{C-D[4]}} > g_{9,\text{C-D[8]}}$.

In exchange for this downweighting of u_9, the preceding weights $g_{9-n+1}, \ldots, g_8$ are larger in CONC-DISP than in CONC-CONC. This is because for the payment dates $t = 9-n+1, \ldots, 8$, the utility range, $\max u_t - \min u_t$, is $u(1) - u(1) = 0$ in CONC-CONC, while it is positive in CONC-DISP. If g is sufficiently steep, then the relatively large weight g_9 will cause the expression

$$\sum_{t=2}^{8} g_t\, u_t(1) + g_9\, u_9(1+RB) \qquad\qquad \text{in CONC-CONC}$$

to be greater than

$$\sum_{t=2}^{9-n} g_t\, u_t(1) + \sum_{t=9-n+1}^{9} g_t\, u_t(1+RB/n) \qquad \text{in CONC-DISP}.$$

This means that the benefits of being patient are underweighted in CONC-DISP relative to CONC-CONC. The prediction of the standard model—inequality (C.1)—is reversed: focused thinkers save more in CONC-CONC than in CONC-DISP.

Let the superscript $^{\mathrm{CB}}$ (for "concentration bias") indicate choices according to focus-weighted utility. With a sufficiently steep weighting function g,[26] we have

$$d^{\mathrm{CB}}_{\text{C-D}} \;:=\; x^{\mathrm{CB}}_{\text{C-C}} - x^{\mathrm{CB}}_{\text{C-D}} \;>\; 0$$

as well as $d^{\mathrm{CB}}_{\text{C-D[8]}} \geq d^{\mathrm{CB}}_{\text{C-D[4]}} \geq d^{\mathrm{CB}}_{\text{C-D[2]}} > 0$.

26. The weighting function has to be steep enough to offset any factors that favour the dispersed payoff, such as discounting and concavity of the per-period utility function.

Table C.1
Concentration bias in MONEY-MAIN

$\hat{d}$ in MONEY-MAIN	0.063***
	(0.011)
Observations	277
Subjects	185

Notes: This table presents an estimate of the difference in savings rates between balanced (CONC-CONC) and unbalanced (CONC-DISP and DISP-CONC) trade-offs. The standard error is in parentheses, clustered on the subject level. The number of observations does not equal twice the number of subjects because the subjects in the first wave participated only in one comparison, while the subjects in the second wave participated in both (see Section C.1.3). $^{*}\,p < 0.10$, $^{**}\,p < 0.05$, $^{***}\,p < 0.01$.

CONC-CONC VS. DISP-CONC. In DISP-CONC, the negative consequences of saving are dispersed over several payment dates, while they are concentrated on a single, thus attention-grabbing, payment date in CONC-CONC. Just as before, if the slope of g is sufficiently steep, then focus weighting reverses the prediction of discounted utility—inequality (C.2)—and posits that individuals save more in DISP-CONC than in CONC-CONC:

$$d^{\mathrm{CB}}_{\mathrm{D\text{-}C}} := x^{\mathrm{CB}}_{\mathrm{D\text{-}C}} - x^{\mathrm{CB}}_{\mathrm{C\text{-}C}} > 0,$$

and that $d^{\mathrm{CB}}_{\mathrm{D[8]\text{-}C}} \geq d^{\mathrm{CB}}_{\mathrm{D[4]\text{-}C}} \geq d^{\mathrm{CB}}_{\mathrm{D[2]\text{-}C}} > 0$.

C.2.3. Hypotheses. We hypothesize that concentration bias is sufficiently strong so that it induces individuals to save more in CONC-CONC than in CONC-DISP, $d^{\mathrm{CB}}_{\mathrm{C\text{-}D}} > 0$, as well as to save more in DISP-CONC than in CONC-CONC, $d^{\mathrm{CB}}_{\mathrm{D\text{-}C}} > 0$. Both effects combined give rise to the prediction of aggregate concentration bias: $d^{\mathrm{CB}} > 0$, with d^{CB} being the average of $d^{\mathrm{CB}}_{\mathrm{C\text{-}D}}$ and $d^{\mathrm{CB}}_{\mathrm{D\text{-}C}}$.

Hypothesis C.1. *Subjects allocate more money to payoffs that are concentrated on a single date than to equal-sized payoffs that are dispersed over multiple earlier dates, $d^{\mathrm{CB}} > 0$ (in contrast to standard discounting).*

Let d^{CB}_{n} capture the difference in savings, averaged over both comparisons, for the degree of dispersion n. With this, we can express our second prediction.

Hypothesis C.2. *The effect described in Hypothesis C.1 is the more pronounced, the more dispersed a payoff is, that is, $d^{\mathrm{CB}}_{8} > d^{\mathrm{CB}}_{4} > d^{\mathrm{CB}}_{2} > 0$.*

C.3 Results

The fact that subjects make several allocation decisions for CONC-CONC, CONC-DISP, and DISP-CONC budget sets allows us to calculate for each individual the average difference in the savings rates between the CONC-CONC and the associated CONC-DISP and DISP-CONC budget sets. Denote by $\hat{x}$ and $\hat{d}$ the empirical counterparts of the variables introduced in Section C.2, that is, of the savings rate x^{CB} and the between-condition difference d^{CB}. With this, we can test our hypotheses.

C.3.1. Test of Hypothesis C.1.

Result C.1. *On average, subjects allocate more money to payoffs that are concentrated rather than dispersed, that is, our measure of concentration bias, $\hat{d}$, is significantly larger than zero.*

Our first result supports Hypothesis C.1. Subjects allocate $\hat{d} = 6.3$ percentage points (p.p.) more money to payoffs that are concentrated rather than dispersed. This treatment effect is statistically significant, using a t-test, with standard errors corrected for potential clustering on the subject level (see Table C.1).This result provides evidence for concentration bias as predicted by Kőszegi and Szeidl (2013).

A closer look at the specific comparisons between CONC-CONC and CONC-DISP as well as CONC-CONC and DISP-CONC substantiates the result: Subjects allocate, on average, more money to later payment dates in CONC-CONC than in CONC-DISP, $\hat{d}_{\mathrm{C\text{-}D}} = 5.7$ p.p. (= 9.12%), and they allocate, on average, more money

Table C.2
Frequencies of the two measures of concentration bias, $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$, being positive, zero, or negative

	$\hat{d}_{\text{C-D}}$ (1)	$\hat{d}_{\text{D-C}}$ (2)
Difference is positive	63 (45%)	59 (43%)
Difference is zero	47 (34%)	51 (37%)
Difference is negative	29 (21%)	28 (20%)
N	139	138

Notes: This table presents frequencies of the difference in savings rates between Conc-Conc and Conc-Disp in column 1 and between Conc-Conc and Disp-Conc in column 2.

to later payment dates in Disp-Conc than in Conc-Conc, $\hat{d}_{\text{D-C}} = 6.8$ p.p. $(= 9.65\%)$.[27] Both $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$ are significantly greater than zero in a t-test (both $p < 0.001$). This demonstrates that concentration bias is driven by both present-biased and future-biased choices, consistent with the central assumption of the focusing model.

The results reported in Table C.2 provide additional support. Table C.2 shows the frequencies of individual values of $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$ being less than, greater than, or equal to zero. In both cases, the largest fraction of subjects has positive $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$ values, and there are more than twice as many subjects with positive than with negative $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$ values, respectively.

At the same time, there are sizeable shares of subjects whose $\hat{d}_{\text{C-D}}$ and/or $\hat{d}_{\text{D-C}}$ values are equal to zero. Let us investigate these subjects' behaviour in greater detail. In the case of $\hat{d}_{\text{C-D}}$, 4 out of 47 subjects choose $\hat{x}_{\text{C-C}} = 0$. Hence, there is no "room" for them to save even less in the Conc-Disp condition. However, for the remaining 43 subjects who choose a savings rate of $\hat{x}_{\text{C-C}} = 1$, there is "room" to save less in the unbalanced budget sets, that is, to choose $\hat{x}_{\text{C-D}} < \hat{x}_{\text{C-C}}$ in line with Hypothesis C.1—but they do not do so. Thus, for these 43 subjects, concentration bias may not matter.[28] Regarding the 51 subjects with $\hat{d}_{\text{D-C}} = 0$, it turns out that 45 subjects choose $\hat{x}_{\text{C-C}} = 1$. This means that they already save the entire budget in the Conc-Conc condition and their behaviour may be confined by a ceiling effect: our setting simply does not allow them to choose $\hat{x}_{\text{D-C}} > \hat{x}_{\text{C-C}}$, as concentration bias would predict. Thus, it might be that some of these 45 subjects would show an effect if they were given the "room" to do so.

C.3.2. Test of Hypothesis C.2.

Result C.2. *Our measure of concentration bias is the greater, the more dispersed payments in the Conc-Disp and Disp-Conc condition are, that is,* $\hat{d}_8 > \hat{d}_4 > \hat{d}_2 > 0$.

Our second result provides evidence in support of Hypothesis C.2: We find that subjects' average degree of concentration bias depends on the degree to which the dispersed payoff is spread over time. Our measure of concentration bias is $\hat{d}_8 = 8.10$ p.p. for 8 payment dates, $\hat{d}_4 = 6.56$ p.p. for 4 payment dates, and $\hat{d}_2 = 3.67$ p.p. for 2 payment dates. All three treatment effects are significantly greater than zero ($p < 0.001$ for $\hat{d}_8$ and $\hat{d}_4$; $p < 0.05$ for $\hat{d}_2$). Moreover, concentration bias in the case that payoffs are dispersed over 4 or 8 payment dates is significantly greater than when payoffs are dispersed over 2 payments dates. However, the difference between dispersion over 4 or 8 payment dates is not statistically significant: in an OLS regression, we find that concentration bias for 8 payment dates is significantly larger than for 2 payment dates ($p < 0.01$) but not significantly greater than for 4 payment dates ($p = 0.237$).

C.3.3. Heterogeneity. Table A.1 in Online Appendix A establishes a significant negative correlation of response time and the degree of concentration bias. In the money experiment, however, we do not find that the decision time correlates with concentration bias, see column 1 of Table C.3.

27. The savings rates in the conditions are $\hat{x}_{\text{C-C}} = 68.3\%$ and $\hat{x}_{\text{C-D}} = 62.5\%$, and $\hat{x}_{\text{D-C}} = 77.3\%$ and $\hat{x}_{\text{C-C}} = 70.5\%$.

28. It might also be the case that the interest rate is large enough to induce even a focused thinker to save all her budget. Moreover, since under discounted utility, subjects with a positive discount rate are better off in Conc-Disp trials, we cannot rule out that concentration bias exactly offsets the discounting-induced advantage of Conc-Disp for some subjects, moving them to $\hat{d}_{\text{C-D}} = 0$, rather than not affecting them at all.

Table C.3
Concentration bias in Money-Main and response time and cognitive skills

	(1)	(2)	(3)
$\hat{d}$ in Money-Main	0.063***	0.063***	0.063***
	(0.011)	(0.011)	(0.011)
Response Time	0.000		
	(0.009)		
CRT score		−0.016	
		(0.011)	
Math score			−0.020*
			(0.011)
Observations	277	277	277
Subjects	185	185	185
R^2	0.000	0.009	0.014

Notes: This table presents OLS regressions of the dependent variable, the difference between balanced and unbalanced trade-offs in Money-Main, on a measure on decision time (column 1), a standardised CRT score (column 2), and a standardised math score (column 3). Standard errors are in parentheses, clustered on the subject level. The number of observations does not equal twice the number of subjects, because the subjects in the first wave participated only in one comparison, while the subjects in the second wave participated in both (see Section C.1.3). $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

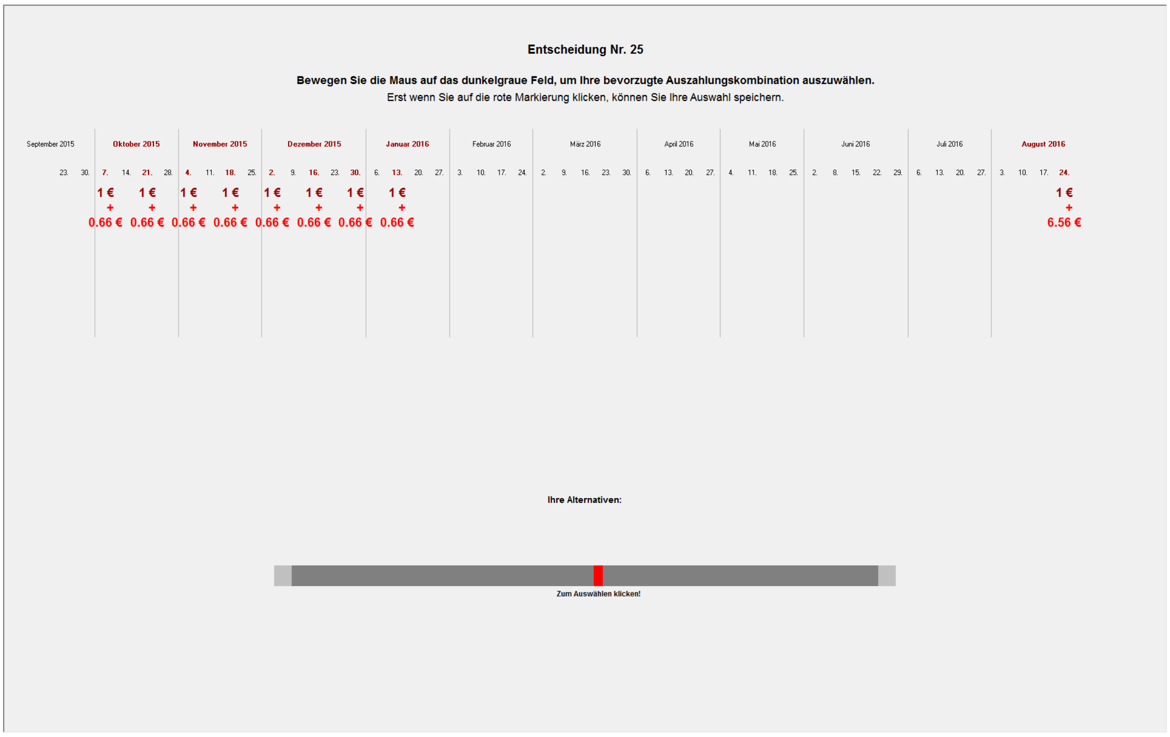
In addition, we test for potential influences of cognitive measures.As evident from Table C.3, we find that math ability as well as scoring high in the CRT negatively affect concentration bias. That is, we find a stronger concentration bias for individuals who are more impulsive or who do worse in the math task. However, the correlation with impulsivity is not significant, and the correlation with math ability is only weakly significant.

C.4 Mechanism

By analyzing subjects' choices in Money-Main, we provide evidence for concentration bias in intertemporal choice that is at odds with standard discounting, while it is predicted by the focusing model of Kőszegi and Szeidl (2013). As discussed in Section 3, it might be the case that focusing works through two different channels, concentration in time and framing of utility consequences. The intuition of the latter is that dispersed payments are cognitively more demanding to aggregate and therefore intangible and less accessible (Kahneman, 2003). Concentrated payments, by contrast, are easily palpable, and individuals can quickly process their underlying value. We aim to disentangle both channels of focusing via the condition Money-Mechanism that is similar to Mechanism-Treatment in the consumption experiment.

In the Money-Mechanism condition, all dispersed payoffs are "dispersed within a day" instead of being dispersed over different payment dates. More precisely, we combine the features of the unbalanced and balanced trade-offs: we make the "dispersed" payoffs equivalent to the concentrated ones, by scheduling all payments on the date of the concentrated payoff. In other words, the "dispersed within a day" payoffs are identical to the concentrated payoffs, except the difference in the display: subjects see 2, 4, or 8 relatively small monetary amounts that they need to add up in order to calculate the total earnings that they will receive at that date. Figure C.5 shows a screenshot of the graphical representation that is shown to subjects who participate in Money-Mechanism (lower panel) in comparison to the graphical representation used in Money-Main (upper panel).

Subjects in Money-Mechanism make the same number of allocation decisions as subjects in Money-Main. Each "dispersed within a day" payoff replaces the respective "dispersed over time" payoff from Money-Main. The concentrated payoffs remain exactly the same. Thus, we can calculate the same average difference of money allocated to concentrated payoffs between "balanced" and "unbalanced" budget sets, that is, $\hat{d}$, for subjects as before. While $\hat{d}$ measures concentration bias in Money-Main, it measures effects



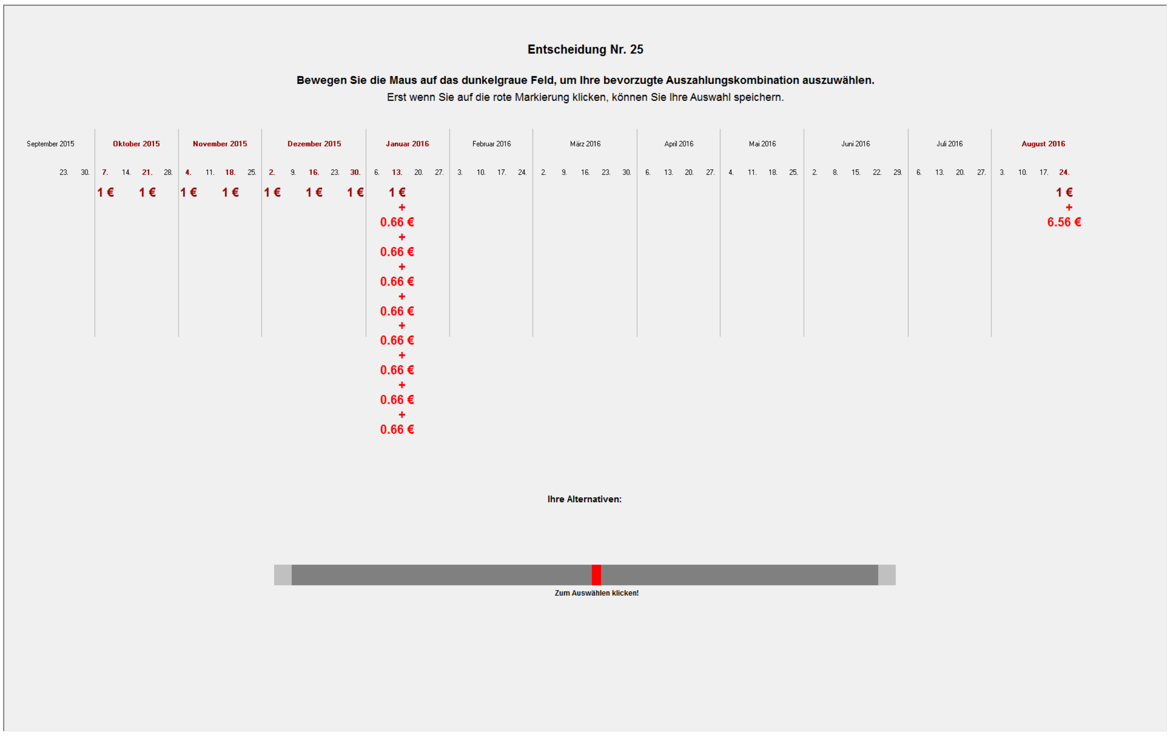


Figure C.5

Screenshots of a ("dispersed-over-time") Disp-Conc decision in Money-Main (top) and of the associated ("dispersed-within-a-day") decision in Money-Mechanism (bottom).

resulting from framing in Money-Mechanism.

If our empirical measure $\hat{d}$ is larger in Money-Main, this implies that the concentration bias observed in this condition cannot be fully explained by computational complexity and framing but that concentration in time matters as well.

As columns 1 and 2 of Table C.4 show, we find that merely presenting a payoff as the sum of multiple small payoffs, without any change in the timing of the payment, makes subjects choose this ("dispersed-

Table C.4
Difference-in-differences analysis of concentration bias, $\hat{d}$, in Money-Main (dispersed over time) vis-à-vis Money-Mechanism (dispersed within a day)

	(1)	(2)
$\hat{d}$ in Money-Mechanism	0.026***	0.025***
	(0.006)	(0.007)
Difference: Money-Main − Money-Mechanism	0.036***	0.038***
	(0.013)	(0.013)
Response Time		0.000
		(0.000)
CRT score		−0.007
		(0.007)
Math score		−0.011*
		0.006
Observations	562	562
Subjects	374	374
R^2	0.016	0.029

Notes: This table presents OLS regressions of the dependent variable, the difference between balanced and unbalanced trade-offs. Column 2 additionally controls for decision time and standardised CRT and math scores. Standard errors are in parentheses, clustered on the subject level. The number of observations does not equal twice the number of subjects, because the subjects in the first wave participated only in one comparison, while the subjects in the second wave participated in both (see Section C.1.3). $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

within-a-day") payoff less frequently than the associated concentrated payoff. On average, subjects allocate 2.6 p.p more of their budget to concentrated than to dispersed-within-a-day payoffs in Money-Mechanism. This effect is significantly different from zero ($p < 0.01$) and indicates that splitting up payoffs in itself has an effect on subjects' behaviour.

However, we also find that our measure of concentration bias is significantly greater ($p < 0.01$) in Money-Main: the effect is 3.6 p.p. larger than in Money-Mechanism, indicating that the effect in Money-Main is more than twice as strong as in Money-Mechanism. Note that this is the case even though discounting works against the effect in Money-Main more strongly than against the effect in Money-Mechanism: discounting makes the dispersed-over-time payoffs more attractive than the dispersed-within-a-day payoffs. We thus find evidence that concentration bias affects intertemporal choice through both framing and concentration in time. This corroborates the evidence that we present in Section 3 for the consumption experiment.

D Nonseparability and Calibration Exercise

D.1 Overview

If a decision maker expects her disutility from work to decrease sufficiently strongly in the number of completed real-effort tasks on previous workdays, she could, in principle, be more willing to work in an unbalanced trade-off than in balanced trade-offs. For instance, this could be the case if subjects anticipated large learning effects between workdays. While such an anticipation of learning effects (or other interpretations of why subjects might anticipate disutility from work may decrease in previously completed tasks) may in principle be possible, we consider it unlikely that subjects in our experiment think about the real-effort task that way:

First, the real-effort task was designed to be dull and repetitive, and not to provide an opportunity for learning beyond mere familiarity effects. Given that subjects experienced how dull and repetitive the task is and that little if any learning is possible before they made their decisions, we think that it is not plausible that subjects anticipated a violation of additive separability. (See Section 4.)

Second, we conduct a calibration exercise to check whether a violation of time separability could plausibly explain subjects' average choices in the balanced and unbalanced choices in the Main-Treatment condition. We find that a violation of time separability that is strong enough to explain our main evidence for concentration bias implies one of the following: it either predicts a pattern of indifference points in the balanced trade-offs that is at odds with subjects' actual behaviour; or it requires an absurd level of anti-discounting.

In the calibration exercise, we make structural assumptions (for details see Section D.2) in order to compute the parameter values that best fit subjects' average balanced and unbalanced choices in Main-Treatment. Since our calibration exercise is based on arguably reasonable and flexible functional forms, our findings provide an important plausibility check whether a violation of time separability could explain our evidence for concentration bias. In particular, we consider and compare two cases: In case 1, we assume time separability and utility weights according to the focusing model. In case 2, we rule out focusing but instead allow for a violation of time separability. In particular, we allow that a decision maker's disutility of completing additional tasks on workday t increases (e.g., due to anticipated fatigue effects) or decreases (e.g., due to anticipated learning effects) in the total number of tasks that she completed on all previous workdays. We allow the curvature of this increase or decrease in disutility to be concave or convex or to change (once) from one to the other. In both case 1 and 2, we assume that discounting is exponential, the disutility of effort could be convex, linear, or concave, and the utility of redeeming the restaurant voucher could be convex, linear, or concave.

In panel (i) and (ii) of Figure D.1, we show that we can fit subjects' choices from Main-Treatment quite well in both cases, although the fit is better in the case of focusing (i) than in the case of time *non*separability (ii). In both cases the balanced choices (lower blue line) and the unbalanced choices (upper bold blue line) can be predicted quite well (dashed red lines). As mentioned above, we find that the disutility of tasks must be decreasing strongly in the tasks completed on prior workdays (e.g., anticipated learning) if time *non*separability is supposed to explain our evidence for concentration bias. An important difference between focusing and time *non*separability is, however, that a good fit in the case of time *non*separability requires an absurdly high level of discounting, while focusing is compatible with a reasonable parameter constellation. In particular, the best fitting parameters for focusing include slight discounting (a between-workdays discount factor of around 0.998, which translates into a daily discount factor of 0.999), convex disutility of real effort, and concave utility of the voucher. Time *non*separability, by contrast, requires both convex effort disutility and convex voucher utility, and an absurdly high degree of anti-discounting (a between-workdays discount factor of around 1.48, which translates into a daily discount factor of 1.24).[29]

To illustrate why time *non*separability requires extremely high anti-discounting, we consider in pan-

29. Note that previous laboratory studies have estimated daily discount factors very close to unity (for instance, Augenblick and Rabin, 2019, estimate the daily discount factor to be between 1.003 and 1.005, while others find values smaller than one, e.g., Augenblick et al., 2015, find a weekly [daily] discount factor of 0.999 [0.9999]).

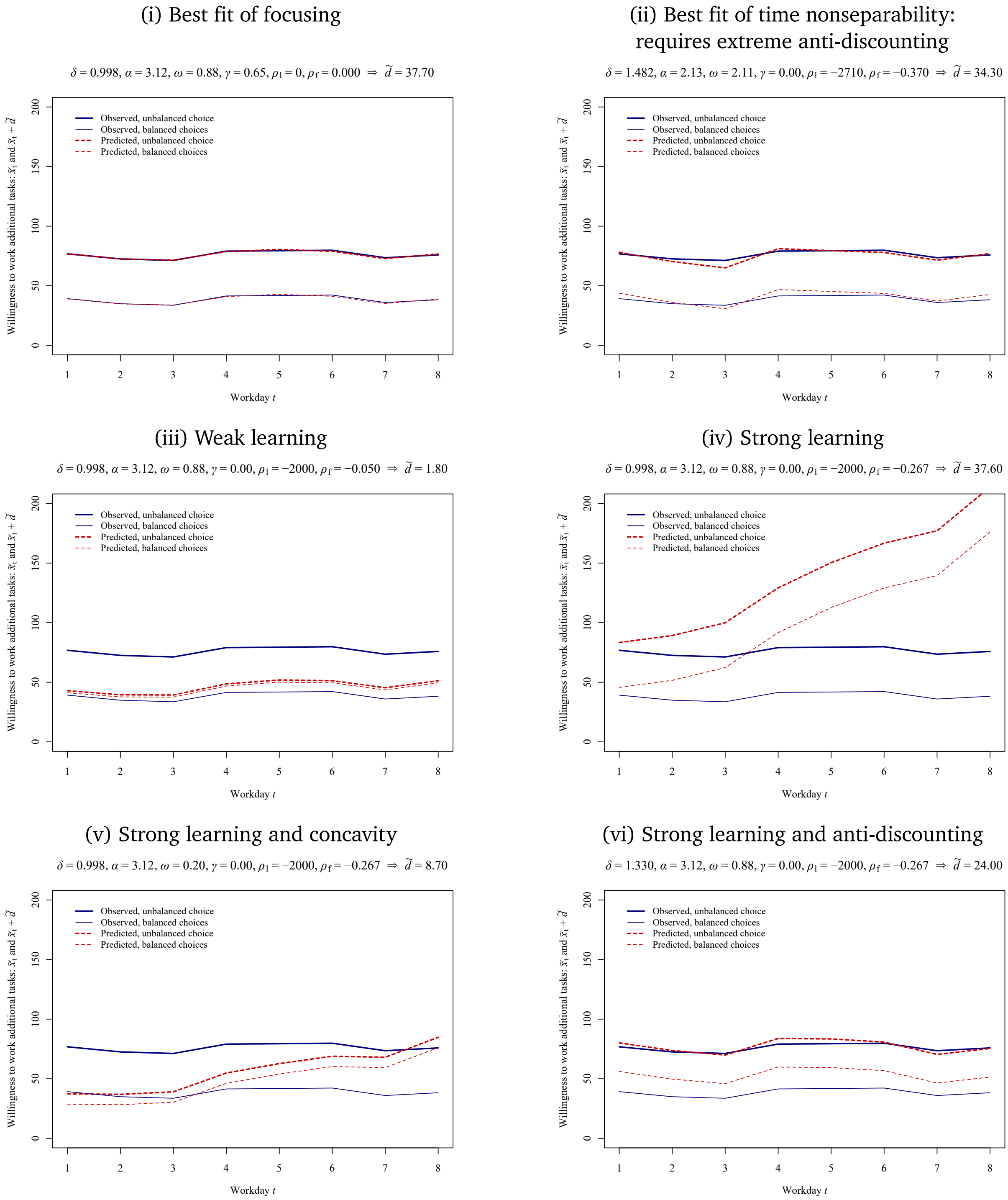


Figure D.1

Average observed and predicted choices in MAIN-TREATMENT according to our calibration exercise. In each panel, we state the parameters on which the prediction is based. δ denotes the between-workdays discount factor; α captures the curvature of the disutility to complete the real effort tasks ($\alpha > 1$: convex disutility, $\alpha < 1$: concave disutility); ω captures the curvature of the utility to redeem the restaurant voucher ($\omega > 1$: convex utility, $\omega < 1$: concave utility); ρ_f and ρ_l capture sign, strength, and curvature of time *non*separability ($\rho_f > 0$: fatigue, $\rho_f < 0$: learning); $\gamma > 0$ captures focusing.

els (iii), (iv), (v) and (vi) of Figure D.1 four counterfactual cases: We keep the fitted parameters from case (i) and remove focus weighting. Instead, we introduce anticipated learning. Panel (iii) demonstrates that weak learning cannot predict the observed difference in balanced and unbalanced choices. A high degree of anticipated learning allows predicting the observed difference between balanced and unbalanced choices. As panel (iv) shows, however, with such a high level of anticipated learning, subjects' willingness to complete tasks should increase substantially over time, since on later workdays subjects should expect to reap the benefits of learning. This prediction of anticipated learning is clearly at odds with subjects' observed choices. Panels (v) and (vi) of Figure D.1 show that this latter prediction of anticipated learning can be mitigated through substantial concavity in the voucher utility (v) or through absurdly strong anti-discounting (vi). Note that only in the latter case, anticipated learning continues to predict a large difference in balanced and unbalanced choices.

D.2 Technical details

In the following paragraphs, we provide the technical background for the results that we discussed in the previous section. Recall that focus-weighted utility is given by equation (2):

$$\tilde{U}(\boldsymbol{c}, C) \coloneqq \sum_{t=1}^{9} g_t(C) D(t) u(c_t). \tag{D.1}$$

For time nonsaparability, we assume history-dependent weights h_t instead of the focus weights g_t:

$$U^{\mathrm{e}}(\boldsymbol{c}) \coloneqq \sum_{t=1}^{9} h_t(\boldsymbol{c}_{t-1}) D(t) u(c_t). \tag{D.2}$$

While the focus weights depend on the choice set C, the weights h_t depend on the decision maker's previous workload, that is, on the vector $\boldsymbol{c}_{t-1} = (c_0, \ldots, c_{t-1})$. The weights h_t thereby capture potential effects of a decision maker's work experience on her subsequent willingness to work. The effects can take on the form of fatigue effects—the weights increase in the number of tasks completed previously—or of learning effects—the weights decline in the number of tasks completed previously. The latter is necessary to predict an *increased* willingness to work in the unbalanced choice in our experiment (i.e., $\tilde{d} > 0$), and the effect has to be sufficiently strong: the increase in disutility from working more in earlier periods must be outweighed by the reduction of disutility in later periods.

In our calibration exercise, we make assumptions regarding the functional forms of the components of (D.1) and (D.2). We choose the following specifications—which allow for a large degree of flexibility, as can be seen below:

- discount function: $D(t) = \delta^t$ $(\delta > 0)$;
- disutility of labour: $u(c_t) = -(-c_t)^{\alpha}$ $(c_t < 0, \alpha > 0)$;
- utility of the voucher: $u(c_t) = \phi\, c_t^{\omega}$ $(c_t > 0, \phi > 0, \omega > 0)$;
- focus weights: $g_t(C) = g[\Delta_t(C)] = [\Delta_t(C)]^{\gamma} = [\max_{\boldsymbol{c}\in C} D(t)u(c_t) - \min_{\boldsymbol{c}\in C} D(t)u(c_t)]^{\gamma}$ $(\gamma > 0)$;
- experience weights: $h_t(\boldsymbol{c}_{t-1}) = 1 \Bigg/ \left\{1 + \exp\left[-\frac{\rho_{\mathrm{f}}}{100}\left(\sum_{s=0}^{t-1} c_s - \bar{c} - \rho_{\mathrm{l}}\right)\right]\right\}$ for $t = 1, \ldots, 8$ $h_9 = 1$;
  $\bar{c} = 675.78$: average experience in unbalanced choices in MAIN-TREATMENT.

The quantities denoted in lowercase Greek letters are parameters to be estimated by fitting the predictions of the models to the observed choices. Two explanations may be in order:

- ϕ is a parameter necessary to weigh utility of the voucher and disutility of labour.
- We allow the experience weights to be as flexible as possible. That is, we allow them to increase or decrease over time; we also allow for concavity, convexity, and a reversal of the curvature. To permit this very flexibility, we pick the logistic function for modelling the experience weights. The logistic function has two parameters, which we denote by ρ_{l} and ρ_{f}: ρ_{l} is the location and ρ_{f} the slope parameter. ρ_{f} determines the direction of the experience effect: learning if $\rho_{\mathrm{f}} < 0$, time-separable utility if $\rho_{\mathrm{f}} = 0$, and fatigue if $\rho_{\mathrm{f}} > 0$. ρ_{l} and ρ_{f} jointly determine the strength of the effect, including the curvature, that is, an increasing or decreasing marginal effect or switch between the two.

We estimate the parameters by fitting the average willingness to work in the balanced and unbalanced choices of the MAIN-TREATMENT condition.[30] We collect the relevant parameters in $\boldsymbol{\theta}^{\mathrm{f}} := (\delta, \alpha, \omega, \phi, \gamma)$ for focus-weighted utility and in $\boldsymbol{\theta}^{\mathrm{e}} := (\delta, \alpha, \omega, \phi, \rho_{\mathrm{f}}, \rho_{\mathrm{l}})$ for experience-weighted utility.

The fitting criterion is the sum of squared differences between the observed choices and the choices predicted by the $\boldsymbol{\theta}$ under consideration. The summation is performed over both types of choices, balanced and unbalanced. Hence, the fitting is done in the choice domain, not in the utility domain. This procedure implies a two-stage numerical minimisation, because the choices leading to indifference cannot be calculated analytically: one cannot solve for $\tilde{x}_j$ and $\tilde{d}$ in order to express them as explicit functions of the preference parameters. On the first stage, the fitting procedure therefore numerically calculates, for each balanced choice $j = 1, \ldots, 8$, the number of additional tasks $\tilde{x}_j$ that would make the decision maker indifferent, for a given $\boldsymbol{\theta}$, between the baseline work plan, $\boldsymbol{c}^{\mathrm{b},j} = (-\boldsymbol{e}, v^j)$, and the overtime work plan, $\boldsymbol{c}^{\mathrm{o},j} = (-e_1, \ldots, -e_{j-1}, -e_j - \tilde{x}_j, -e_{j+1}, \ldots, -e_8, v^j + r^j)$. It simultaneously calculates, for the given $\boldsymbol{\theta}$, the $\tilde{d}$ (with a precision of 0.1) that would lead to indifference between $\boldsymbol{c}^{\mathrm{b}} = (-\boldsymbol{e}, v)$ and $\boldsymbol{c}^{\mathrm{o}} = (-\boldsymbol{e} - \tilde{\boldsymbol{x}} - \tilde{d} \cdot \mathbf{1}, v + R)$ in the unbalanced choice. On the second stage, the distance between the predicted and the observed choices is minimised numerically over all $\boldsymbol{\theta}$ under consideration.

The presence of the first stage does not permit the use of standard nonlinear regression techniques. Consequently, we conduct an iterated grid search, a standard method for optimization problems with complex objective functions (see Judd, 1998, p. 100). Computational feasibility restricts us to fitting the average choices instead of fitting individual choices.[31]

30. See equation (10): $\tilde{\boldsymbol{x}} = (39.2, 34.9, 33.6, 41.5, 41.8, 42.2, 35.9, 38.3)$ and equation (11): $\tilde{\boldsymbol{x}} + \tilde{d} \cdot \mathbf{1} = \tilde{\boldsymbol{x}} + 37.6 \cdot \mathbf{1} = (76.8, 72.5, 71.2, 79.1, 79.4, 79.8, 73.5, 75.9)$.

31. For the estimation, the utility contributions were normalised as follows: the number of tasks worked on day t was divided by the maximum average number of tasks worked on any workday, 183.15; the value of the voucher, v^j or $v^j + b^j$, respectively, was divided by the maximum voucher value, $v + R = 31.30$. This means that for linear preferences ($\alpha = \omega = 1$) and time-separable utility, the weighting parameter amounts to $\phi = 12.92$ for the balanced choices, because the average raise was $\overline{r}^j = 2.975$ euro and the average willingness to work was $\overline{\tilde{x}}_j = 38.425$ tasks, and $38.425/2.975 = 12.92$. The initial work experience is set to the number of practice trials that our participants completed in the lab, that is, $c_0 = 10$. The logit function giving rise to the h_t weights was normalised such that its maximum over the range of experiences in the balanced decisions of MAIN-TREATMENT, $[0, 1336]$, was always equal to 1. Crucially, these normalisations do not entail any loss of generality but merely help reduce the range of values that ϕ takes on, thereby facilitating the computationally demanding grid searches. Also importantly, the initial grids were identical for the parameters that the two models have in common, and the final grids were of identical precision for the shared parameters (0.001 for δ; 0.01 for α, ω, ϕ). This makes the goodness of the fits comparable across the two models.

E Instructions

E.1 Instructions for the consumption experiment

These are the instructions, translated from the German original, for the conditions Main-Treatment *and* Main-Control *of the consumption experiment* Main.

Screen 1—General information

Decisions and baseline compensation of all participants. Today's experiment consists of tasks that will be explained in further detail on the following pages. All participants of this study get a baseline compensation of €10.00.

Random selection of three participants. Among all participants, three will be selected randomly. For these persons, there will be up to 8 additional work dates online—and, in return, the opportunity to earn additional compensation. All participants who are not among the randomly chosen ones will not have to implement their decisions—and thus will not get invited to online sessions. The selection of these three participants is conducted at the end of this session. For the selection procedure, the cards with the cubicle numbers of all participants will be collected. Three cards will be drawn blindly and randomly. These draws determine the participants that have to implement their decisions.

Random selection of a decision that counts. For the three randomly selected participants, only one decision will be selected to be the decision that counts. The three randomly selected participants will have to implement this decision on all 8 future dates. For each person, exactly one decision will be chosen to be the decision that counts. All decisions have the same probability of being drawn. The decision that counts will be drawn randomly by the computer. In order to keep the instructions on the following pages as short as possible, they are written as if you were one of the chosen persons and as if the decision at hand were the one that counts.

Screen 2—Your Task: Translation of a sequence of numbers into a sequence of letters

Over the course of this study, you will have to complete various tasks, which are all of the same kind. In each task, you have to translate a sequence of 6 numbers into a sequence of letters. In order to do that, you will get an input box below the number sequence. Example:

[Example number sequence displayed—for instance, "18 19 7 14 2 2".]

You should translate this number sequence using an encryption key. Example:

[Example encryption key displayed—for instance,

1	*2*	*3*	*4*	*5*	*6*	*7*	*8*	*9*	*10*	*11*	*12*	*13*	*14*	*15*	*16*	*17*	*18*	*19*	*20*	*21*	*22*	*23*	*24*	*25*	*26*
Q	*V*	*E*	*Z*	*C*	*J*	*X*	*F*	*H*	*G*	*I*	*O*	*T*	*Y*	*M*	*W*	*R*	*L*	*D*	*K*	*S*	*P*	*A*	*N*	*B*	*U .]*

The encryption key will be presented in the form of a table. It assigns a specific letter to each number. Your task is to find the corresponding letter to every number and enter it in the input field below. Please do not use spaces in your input. The task is not case-sensitive. In the above example, the number sequence "18 19 7 14 2 2" is shown. The encryption key specified by the example table gives rise to the letter sequence "LDXYVV". It is this letter sequence that you need to enter in the input field. The number sequence as well as the encryption key change from task to task. As soon as you have entered the correct letter sequence, the input field turns green, and you can click "Continue" to show the next number sequence. You will now complete 10 of such tasks as an exercise.

Screen 3—Your current task

You have translated n number sequences so far. Thus, there are still $10 - n$ number sequences remaining. Please enter the corresponding letter from the code table for each number (without spaces).

Screen 4—End of the example tasks

You have completed the example tasks successfully.

Screen 5—Selection of 8 work dates

Tasks per work date. Today's study includes 8 future dates, all of which are weekdays. We call these 8 dates "work dates". From the dates displayed below, you can choose your own 8 work dates. On each work date, you will have to complete at least 100 tasks. You should take this into account when choosing your work dates. All tasks are coding tasks like the one you just practised.

Working online. The coding tasks will be solved online. That means that you can complete the tasks on your own computer. In the morning of the day on which you have to complete your tasks, you will get a pre-scheduled reminder email. For each date, you will have to complete the tasks between 6:00 and 24:00. Within this time span, you can freely choose when to complete the tasks. The tasks must be completed in one session. This means that you are not allowed to close your browser window while working on the tasks. You may, of course, take short breaks—but you must not restart your browser.

Choice of work dates. Please choose, out of the following 20 dates, 8 dates on which you would like to complete your tasks.

[list of possible dates]

Screen 6—Compensation

For the full completion of all tasks on your 8 work dates, you will receive a compensation that consists of two parts:

Part 1 of the compensation. Part 1 is a voucher for a restaurant visit to the "485°" pizza place in Cologne (Kyffhäuserstraße 44, close to the university, or Bonner Straße 34, Südstadt): On a day chosen by yourself, in the time from Mon., 19/08/2019, to Sun., 25/08/2019, you—and a companion—can visit "485°", and we will provide you with a voucher for a part of the costs of your restaurant visit. The value of the voucher will be determined during the course of today's experiment. Here is an overview of the costs of food and drinks at the pizza place:

Appetisers:	between € 3.50 and €14.50;
Pizzas (+ extra toppings):	between € 7.50 and €23.00;
Desserts:	between € 3.50 and € 4.80;
Soft drinks:	between € 2.90 and € 3.40;
Beers:	between € 3.20 and € 4.70;
Regular wines per glass:	between € 2.90 and € 8.00;
Regular wines per bottle:	between €18.00 and €28.00.

There will be no additional effort for you to use the voucher: We will communicate your name and the value of the voucher to "485°". You will get the voucher at the restaurant on the date that you chose for your restaurant visit. You will only have to pay a possible difference between the amount you consumed and the voucher. Here are two illustrating examples:

Example 1: The voucher has a value of €20.50. However, you do not spend the entire value of the voucher: You consume food and drinks for €19.30. Then the voucher covers the full amount of €19.30.

Example 2: The voucher has a value of €20.50. You and a companion consume food and drinks for €24.80. Then the voucher covers the amount of €20.50, and you have to pay the difference of €4.30.

Part 2 of the compensation. The second part is a transfer €100 to your bank account. The transfer to your bank account will be arranged one week after the date that you chose for your restaurant visit.

Choice of a date for your visit to the pizza place. Please choose a date on which you would like to visit the "485°" pizza place:

[list of possible dates]

Screen 7—Decisions [1st tab]

General Information *Work plans and voucher.* Today's experiment consists of different decisions. Each decision consists of a choice between Alternative A and Alternative B. Each alternative consists of a specific work plan and a voucher of a specific value. The work plan indicates how many tasks you have to complete on each of the 8 work days. The value of the voucher indicates the monetary amount that you will receive at most to cover the expenses of your visit to the "485°" pizza place on *[chosen date]*. In each decision, Alternative A yields a higher voucher value than Alternative B. In some decisions, the workload is higher according to Alternative A, and in some decisions, the workload is higher according to Alternative B.

Example 1.

[Example work plans displayed.]

Alternative A consists of a work plan according to which you have to complete 174 tasks on *[date DD.MM.YYYY]*; on all other dates you have to complete the same number of tasks as for Alternative B. Completing all tasks yields a voucher with a value of €11.50. In this example, Alternative B consists of a work plan according to which you have to complete 157 tasks on *[date DD.MM.YYYY]*; on all other dates you have to complete the same number of tasks as in Alternative A. Having completed all tasks, you will receive a voucher with a value of €7.50.

Randomly selected decision. At the end of the experiment, the computer randomly chooses one decision as the decision that counts. The alternative that you have chosen in this decision determines the value of the voucher as well as the number of tasks that you have to complete on the 8 work dates. If you complete the chosen work plan, you will get the voucher for the restaurant visit on the *[date chosen on Screen 6]*, and a week later €100 will be transferred to your bank account. On the next page, you will learn how to make the decisions. (Should you not be chosen at the end of the experiment, you will not have to work on any of the 8 dates, and there will be no additional compensation. In this case the experiment will be over for you today.)

Screen 8—Decisions [2nd tab]

Direct and indirect decisions. *Direct decisions.* You will make some of the decisions directly. In direct decisions, you will choose your preferred alternative of the displayed alternatives A and B via a mouse click. In these decisions, Alternative A is always displayed in green and Alternative B is always displayed in blue.

Indirect decisions. In order to save time, you do not have to make all decisions directly. Instead, you will make some decisions indirectly. All of your indirect decisions will entirely and exclusively be based on your own direct decisions.

You will determine your indirect decisions according to the following principle: If you have chosen an alternative in a direct decision, this alternative will also be chosen in an indirect decision if

- the chosen alternative itself has become better (rule 1) or
- the nonchosen alternative has become worse (rule 2).

In the following, this principle will be illustrated using examples for the two rules.

Example for rule 1.

Example decision 1 a: Direct decision

[Example work plans from example 1 and hypothetical decision in favour of Alternative A displayed.]

Example decision 2: Indirect decision resulting from decision 1 a

[Example work plans with Alternative A featuring a lower number of tasks on one day than in example 1 displayed; Alternative A preselected.]

Assume that you chose Alternative A in example 1. You no longer have to make example decision 2 as a direct decision, since Alternative B is unchanged and Alternative A has improved: on one date you need to work less. Thus, in example 2 you would choose Alternative A indirectly.

Example for rule 2.

Example decision 1 b: Direct decision

[Example work plans from example 1 and hypothetical decision in favour of Alternative B displayed.]

Example decision 3: Indirect decision resulting from decision 1 b

[Example work plans with Alternative A featuring a higher number of tasks on one day than in example 1 displayed; Alternative B preselected.]

Assume you have chosen Alternative B in example 1. You no longer have to make example decision 3 as a direct decision, since Alternative B is unchanged and Alternative A has become worse: on one date you have to work more. Thus, in example decision 3 you would choose Alternative B indirectly.

Information screen. At the end of the experiment, you will be shown all decisions that you made, both directly and indirectly.

Screen 9—Decisions [2rd tab]

Random selection of the decision that counts. *Identical probabilities.* Direct and indirect decisions are selected with equal probability to be the decision that counts.

- Since the computer may pick any direct decision to be the decision that counts, you should make every direct decision as if it were implemented.
- Since the computer may pick any direct decision to be the decision that counts, you should make every direct decision with great care. This is because, as explained on page 2, your indirect decisions will be determined entirely and exclusively by your direct decisions.

Decision Blocks. The decisions are divided into 9 blocks. For each decision block, the number and the content of its decisions are determined from its beginning. As mentioned previously, each decision of a decision block will be selected with the same probability by the computer to be the decision that counts. This holds for both the direct and the indirect decisions.

Thus, which alternatives you choose in the direct decisions of a decision block has no influence on the content and the number of the other decisions of the respective decision block. To repeat again: The number of and the content of the decisions in a block are fixed from the beginning of the block.

Which decisions of a block you will make directly and which you will make indirectly is, however, random to some degree:

- The first direct decision of each block will be randomly selected by the computer among all decisions of the respective block.
- The order of the following direct decisions will be determined by the computer such that you have to make as few direct decisions as possible—that is, to save as much time as possible.

Screen 10—Decisions [4th tab]

Practice decision block. On the following screen, you can practice the procedure by working through some hypothetical decision blocks. Following the hypothetical decision blocks, we will ask you some questions in order to test your understanding. After answering the questions correctly, you will make your decisions.

Screen 11—Practice decision block *i*

Please click "Continue" to proceed to a series of practice decisions. The decisions made are purely hypothetical. You will not have to implement any of them.

Screen 12—Practice decision screen

[No additional instructions.]

Screen 13—Control questions

Before making your decisions, please answer the following questions:

1. Which amount of money will you receive for completing your work schedule?

- ○ The more money, the more tasks you complete: €0.10 per task.
- ○ Independent of your decisions, you will receive €100.
- ○ The more money, the more tasks you complete: €0.25 per task.
- ○ Independent of your decisions, you will receive €150.

2. Which of the two alternatives gives you a higher-valued restaurant voucher in each decision?

- ○ Alternative A.
- ○ Alternative B.

3. All potential work schedules include at least 100 tasks that you have to complete at each work date. How many work dates exist?

- ○ 7 work dates.
- ○ 8 work dates.
- ○ 9 work dates.

4. Which of the following statements is correct?

- ○ At the beginning of a decision block, number and content of the decisions are fixed. The alternatives that you choose in the direct decisions of a decision block do not influence number and content of the choices of the respective choice block.
- ○ The alternatives that you choose in the direct decisions of a decision block influence number and content of the choices of the respective decision block.

If you still have any questions, please raise your hand now.
If there are no more questions, please click the button to start making your choices.

Screen 14—Decision block i

Now you can start with the decisions of the current decision block. Please click "Continue".

Screen 15—Decision screen

[Display of the decision screen.]

Screen 16—Please provide some additional information

[This screen was contingent on the subject's decisions: it appeared only if the subject had invariably chosen Alternative A in the respective decision block.]

You chose Alternative A in every single decision of this block. We would therefore like to ask you to provide the following additional information: Assume that Alternative A included even more tasks than it did in your most recent decision. Starting from which number of tasks on the highlighted date are you no longer willing to choose Alternative A but would prefer Alternative B?

Screen 17—Survey

The part of today's experiment in which you had to make decisions is over. In the following, there are four parts. In each part, we will ask you a question or present a task to you. Before showing you the overview of all the decisions you made, we would like to ask you the following questions.

- What is your gender?
- How old are you?
- What is your major?
- What was your final grade in a high school math class?
- How high is your disposable income each month (including financial support by your parents, BAFöG *[student financial aid]*), unemployment insurance payments; deducting housing, and health insurance expenditures)?

Screen 18—Overview of all decisions

Please click "Continue" to get to an overview of all decisions that you made.

The overview contains all decision blocks for which you made a decision. All your direct as well as your indirect decisions will be shown.

Screen 19—Overview of all decisions

[Display of the overview.]

Screen 20—Picture puzzles

A set of picture puzzles consisting of 10 separate pictures will follow.

In each picture, there will be an empty field. Your task is to fill in this empty field in a way such that a logical progression of symbols is established.

Please try to solve as many as possible of the 10 tasks.

A click on "Continue" will start the task. You have 5 minutes to complete all the picture puzzles.

Screen 21—Picture puzzle i of 10

[Display of the puzzle.]

Screen 22—Picture puzzle

You have correctly solved n out of 10 puzzles.

Screen 23—Addition of numbers

We would now ask you to add multiple numbers as often as possible. You have a total of 8 minutes to solve as many tasks as possible from a maximum of 20 tasks. For each 5 correctly solved tasks, you receive €1 as an additional payment. The numbers will be displayed at random either horizontally as integers or vertically as decimal numbers. A click on “Continue” starts the task.

Screen 24—Task number i

[Display of the task.]

Screen 25—Addition of numbers

You have correctly solved x tasks and will therefore receive an additional payment of €y.

Screen 26—Answering three questions

Finally, we would like to ask you to answer a question on each of the following three pages. In order to get to the first question click “Continue.”

Screen 27—Question number 1

A bat and a ball cost €1.10 in total. The bat costs €1.00 more than the ball. How much does the ball cost?

Screen 28—Question number 2

If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?

Screen 29—Question number 3

In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?

Screen 30—Randomly selected decision

You made decisions in 9 decision blocks. The computer randomly selected block number i as the block that is relevant for your compensation. From this decision block, decision number j was randomly selected as the decision that counts. This decision was a choice between the following two alternatives:

[Display of the alternatives available in the decision that counts.]

In this decision, you picked Alternative X over Alternative Y.

The work plan that is relevant for you and that you have to implement is:

[Display of the work plan resulting from the decision that counts.]

If you are randomly selected, among the participants of the current session, to be a person to work on all 8 work dates, the following will apply:

On each work date, you have to complete the number of tasks indicated. The completion of the work plan will be compensated as follows:

- by a voucher of €x value for a restaurant visit to the “485°” pizza place on *[chosen date]* as well as
- by a transfer of €100 to your bank account, carried out one week after your restaurant visit.

The persons that will have to work on the 8 work dates will be selected in a few minutes. To select them, we will collect your cubicle numbers and will then blindly—that is, randomly—draw three of them.

For today’s session, you will receive a cash payment of €10.00 (participation fee) + €y (addition task).

Please remain seated, remain quiet, and wait for further instructions.

E.2 Instructions for the money experiment

These are the instructions, translated from the German original, for the money experiment. In the money experiment, subjects made choices from convex time budgets. While the text of the instructions was identical for all conditions, the income sequences displayed on the respective screens differed between conditions.

Screen 1—Welcome

We would like to ask you to be quiet during the experiment and to use the computer only for tasks which are part of this experiment.

If you have any questions, please raise your hand. We will come to you for help.

Please put your cell phone into the bag at your place.

Screen 2—Information about the procedure

Part 1. In the first part of this experiment, you will unconditionally receive nine €1 payments, which will be transferred to your bank account at various dates in the future. Moreover, you will receive one additional payment or multiple additional payments for this first part. You can decide yourself when this/these additional payment/s will be transferred. For any decision you make the following will always hold:

If you choose a later payment, you will receive, in total, more money than when you choose an earlier payment.

Overall you will make 60 decisions about the timing and the amount of money of your additional payment(s). After you have made your decisions, one decision will be randomly picked by the computer and paid out for real. Since every decision is picked with the same probability, it is advisable for you to make every decision as if it were the payoff-relevant decision.

Your payment for Part 1 will be transferred to your bank account. All requests for transfers will be transmitted to the bank as future-dated transfers today. We will send you an e-mail with all the requests transmitted to the bank, so that you can check whether the instructions sent to the bank are correct!

After the last transfer we will send you another e-mail message which will remind you of all dates and amounts of the payments made to you.

If you have any question, please raise your hand. We will come to you for help.

Part 2. In Part 2 of the experiment, we will ask you to perform a different task. You will receive money for doing this task. We will provide you with information about the exact payment for this second part right before its beginning. Your payment for the second part is independent of the payment for the first part, and you will get paid in cash at the end of the experiment.

Screen 3—Bank details

[On this screen, subjects enter their bank details.]

Screen 4—Choice lists

Part 1a. In the first 24 decisions, you have to choose your most preferred option out of nine possible payment alternatives. In all of these decisions, you have the possibility to receive your whole payment earlier in time or, alternatively, a larger total amount later in time.

In the following, before the experiment starts, we will show you two exemplary payment alternatives such that you can familiarize yourself with the decision screens of this experiment.

Screen 5—Example 1

In this example, the first alternative has been selected. The slider is positioned such that payment alternative no. 1 is displayed. In this example, payment alternative no. 1 corresponds to a payment of €8 on the earliest possible date. Additionally, €1 is transferred to your bank account at nine different dates.

Screen 6—Example 2

In this example, the sixth alternative has been selected. The slider is positioned in a way such that payment alternative no. 6 is displayed. In this example, payment alternative no. 6 corresponds to multiple payments of €1.50 on the highlighted dates. Additionally, €1 is transferred to your bank account at nine different dates.

Screen 7—Example 3

You can choose your preferred option out of nine alternatives. All alternatives differ in the total amount of money and the points in time at which the associated transfers are made. The following always applies: If you choose a later payment, you will receive, in total, more money than when you choose an earlier payment.

On the next screen, all nine payment alternatives of this decision will be shown in an animation.

The transfer dates are highlighted in red.

After the animation you have the possibility to have another look at all payment alternatives, and you will be able to choose your most preferred alternative.

This hint will be shown for the first three decisions.

Screen 8—Budget sets

Part 1b. In part 1b you will now make the remaining 36 decisions.

In each decision you have the possibility to allocate a certain amount of money to earlier and later dates. The less money you allocate to earlier dates, the more money you receive later. This entails that the total amount is the larger, the more money you allocate to later dates.

You make the decisions by moving a pointer on a slider with your mouse.

You can practice the use of the slider here: *[Example slider shown.]*

You move a red marker by positioning your mouse over the dark-grey bar (do not click!). If you click the red marker, your choice is logged and can be saved afterwards. For this purpose, a red button “Record choice!” will appear. After clicking this button, your current choice is saved.

If you want to correct a logged choice, click the red marker again and subsequently move the mouse to your preferred position.

Screen 9—End of Part 1

This was the last decision of Part 1 of the experiment.

Before you learn which decision from the first part will be paid out for real, we would like to ask you to take part in the second part of the experiment.

Please click the “Continue” button.

Screen 10—Part 2

In this part we would like to ask you to add up a string of figures as often as you can manage. You have 5 minutes time for performing this task.

You receive a base payment of €1 for this part. The more numbers you succeed to sum up correctly, the more money you earn: You receive €0.20 for each correct summation.

You are given three attempts for each summation. If you are not able to calculate the sum correctly by the third attempt, you lose €0.05.

(Attention: You have to use a period [.] instead of a comma [,] when writing decimal numbers.)

Screen 11—Feedback

You have solved x tasks correctly, and you have y times entered a wrong solution in all three attempts.

You receive €z for this task. You will receive the payment in cash in a few minutes.

Screen 12—Survey

The experiment will be over soon. In closing, we would like to ask you to answer ten questions. After answering these ten questions, you will learn your payment/s for the first part and receive your remuneration for the second part.

[Display of socioeconomic questionnaire with 7 items.]

Screen 13—CRT 1

A bat and a ball cost €1.10 in total. The bat costs €1.00 more than the ball. How much does the ball cost?

Screen 14—CRT 2

If it takes 5 machines 5 minutes to make 5 widgets, how long would it take 100 machines to make 100 widgets?

Screen 15—CRT 3

In a lake, there is a patch of lily pads. Every day, the patch doubles in size. If it takes 48 days for the patch to cover the entire lake, how long would it take for the patch to cover half of the lake?

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