Concentration Bias in Intertemporal Choice*

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Abstract

We present novel results on individuals' intertemporal choices that cannot be explained by exponential and hyperbolic discounting, the standard approaches to intertemporal decision making in economics. In particular, we provide causal evidence from novel lab experiments that intertemporal choices are systematically affected by whether consequences of intertemporal choice are concentrated in single or dispersed over multiple periods: (i) Individuals are more impatient in the case that the costs of impatient behavior are dispersed over many future periods than when they are concentrated in a single future period. (ii) Individuals are more patient in the case that the costs of patient behavior are dispersed over multiple earlier periods than when they are concentrated in a single earlier period. Both findings demonstrate concentration bias in individuals' intertemporal choices. We contrast our findings to results from a control experiment to distinguish between theoretical explanations of concentration bias. Despite the prevalence of dispersed payoffs and costs in everyday life, no empirical study so far has investigated whether spreading payoffs over time causally impacts discounting. Our results suggest that previous studies may have neglected an important channel that influences intertemporal decisions.

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1 Introduction

Almost any decision that we make has intertemporal consequences—costs and benefits that occur at various points in time. These consequences often stretch over numerous days, months, or even years. The canonical model of intertemporal choice in economics—exponential discounting—posits that individuals evaluate each available option by aggregating all of its intertemporal consequences into a weighted sum and choose the option that yields the largest weighted sum (Samuelson, 1937). Individuals' weighting is typically assumed to reflect a constant discount rate—which implies time-consistent time preferences. Additionally, individuals are implicitly assumed not to suffer from cognitive limitations that inhibit their ability or their willingness to aggregate intertemporal consequences in accordance with their preferences.

Accumulating evidence documents that individuals' intertemporal behavior is often not consistent with predictions based on exponential discounting (Thaler, 1981; Frederick, Loewenstein, and O'Donoghue, 2002; Dohmen et al., 2012).[1] Aside from that development, economists have become increasingly convinced that cognitive limitations often shape individuals' behavior (Simon, 1955), as suggested by evidence from the realms of decision making under risk and uncertainty (Tversky and Kahneman, 1974) as well as individuals' purchasing decisions (Chetty, Looney, and Kroft, 2009; Lacetera, Pope, and Sydnor, 2012). Recent theories of stimulus-driven attention (Bordalo, Gennaioli, and Shleifer, 2012, 2013; Kőszegi and Szeidl, 2013; Bushong, Rabin, and Schwartzstein, 2016) as well as goal-driven attention (Sims, 2003; Gabaix, 2014) model the effects of cognitive limitations on individuals' behavior more generally.[2] Therefore, taking cognitive limitations into account constitutes a promising approach towards improving our understanding of decision making also in the area of intertemporal choice. However, empirical evidence regarding this matter is lacking.

In this paper, we address this gap in the literature and study individuals' aggregation of intertemporal consequences in light of potential limited-cognition effects. Our research question is whether individuals overweight intertemporal consequences that are concentrated in single periods relative to consequences that are dispersed over multiple periods. This research question builds on the aforementioned theories of limited attention—in particular, on Kőszegi and Szeidl (2013)—and is motivated by a pervasive asymmetry within intertemporal trade-offs: benefits are often concentrated in single, "attention-grabbing" periods, while associated costs are dispersed in "intangible" doses over numerous periods. For instance, avoiding the

[1] When proposing exponential discounting, Samuelson (1937, p. 159) already pointed out its "serious limitations" and its restricted descriptive validity (see p. 160).

[2] Models of goal-driven attention assume that attention is a scarce resource and that decision makers choose how to allocate attention to the different choice features. In contrast, models of stimulus-driven attention suppose attention is "automatically" attracted by—or driven towards—certain features which stick out in the choice context.

hassle of exercising in the gym today marginally deteriorates physical well-being each following day; or the prospect of receiving a large bonus payment at the end of the year may come at the cost of working half an hour overtime each day until then. While concentrated consequences—e.g., avoiding the hassle of exercising or receiving the bonus—may be attention-grabbing, consequences that are dispersed in small doses over time—e.g., deteriorated physical well-being or overtime work—may be less tangible. This potentially leads individuals to be biased towards concentrated benefits. Such concentration bias has two types of unique and testable implications for intertemporal choices: if benefits are concentrated in a period before its dispersed costs, concentration bias predicts choices biased towards the present; if benefits are concentrated in a period after its dispersed costs, concentration bias predicts "future-biased" choices.

We test these implications of concentration bias for intertemporal choices in a series of laboratory experiments. We thereby contribute to the literature in two important ways: First, we designed novel experiments in which we vary whether intertemporal consequences are concentrated or dispersed. This allows us to provide causal evidence on whether individuals systematically overweight concentrated intertemporal consequences relative to dispersed consequences. Second, our experiments were designed to yield tests which directly inform how potential concentration bias effects should be modeled. In particular, our design allows us to distinguish between explanations for potential concentration bias effects that build on stimulus-driven attention and on goal-driven attention—as is discussed in greater detail below. Moreover, we test the main assumption of the focusing model of Kőszegi and Szeidl (2013).

In our main experiment, subjects were endowed with multiple budget sets across the different trials (in a variation of the methodology developed in Andreoni and Sprenger, 2012). Each budget set consisted of multiple earnings sequences, and subjects had to pick exactly one earnings sequence per trial out of the offered set. Each earnings sequence specified a series of 9 money transfers to subjects' bank accounts at given dates in the future. In making their choices, subjects faced a trade-off between the earlier and later payoffs; that is, they had to accept lower earlier payoffs at the benefit of increasing later payoffs. The sum total was the greater, the more money subjects allocated to later payment dates. The larger the amount of money that subjects decided to receive at later payment dates—i.e., the more they were willing to wait in order to receive an overall larger sum of money—the more patient we consider them (see Andreoni and Sprenger, 2012).

The crucial innovation of our experimental design is that we extend the method of convex budget sets through the inclusion of both "concentrated" and "dispersed" payoffs. A concentrated payoff consists of a single payment on a particular date. In contrast, a dispersed payoff includes payments on 2, 4, or 8 distinct dates. We varied within-subject whether the intertemporal allocation was a balanced (BAL) trade-off between two payoffs that were each concentrated or whether it was an unbalanced

(UNBAL) trade-off between one payoff that was concentrated and another payoff that was dispersed.

For instance, in condition $\text{BAL}^{\text{I}}_{1:1}$, the sequence of 9 dated money transfers can be expressed as the vector

$$\mathbf{c}^{\text{BAL, I}}_{1:1} \;=\; [1 + B\,(1 - x), 1, 1, 1, 1, 1, 1, 1, 1 + R\,B\,x],$$

with the i^{th} entry specifying the euro amount of the i^{th} payment.[3] $R := 1 + r$ is an interest factor (with r being our experimental nominal interest rate), and B is the endowment that can be paid out on the first payment date. That is, R and B denote parameters of a given income sequence (e.g., $B = €11$, $r = 15\%$). The variable x is the subject's choice variable, $x \in \mathbf{X}$ with $\mathbf{X} = \{0, 1/100, 2/100, \ldots, 1\}$. Throughout the paper, we designate choice sets by uppercase **C**, and we use lowercase **c** to denote elements of **C**. Each x goes along with a particular element $\mathbf{c}^{\text{BAL, I}}_{1:1}$ from the budget set $\mathbf{C}^{\text{BAL, I}}_{1:1}$ so that $\mathbf{C}^{\text{BAL, I}}_{1:1} := \{\mathbf{c}^{\text{BAL, I}}_{1:1} \mid x \in \mathbf{X}\}$.

When choosing x for $\mathbf{C}^{\text{BAL, I}}_{1:1}$, subjects faced a balanced trade-off between two concentrated payoffs, one on the first payment date and the other on the last payment date. Subjects decided what fraction of their first payment they would forgo in exchange for receiving the remaining fraction plus interest at the last payment date. (For instance, with $B = €11$ and $r = 15\%$, subjects could receive up to $€1 + €12.60$ as the last payment.)

In condition $\text{UNBAL}^{\text{I}}_{1:n}$, subjects were endowed with budget sets $\mathbf{C}^{\text{UNBAL, I}}_{1:n}$ that included similar earnings sequences. However, they faced trade-offs which featured a concentrated early payoff and a later payoff that was dispersed over the last n payment dates: instead of receiving $R\,B\,x$ at the last payment date, $R\,B\,x$ was dispersed over the last n payment dates, so that subjects received $R\,B\,x\,/\,n$ per date, with $n \in \{2, 4, 8\}$. For instance, elements of the budget set given to subjects in $\text{UNBAL}^{\text{I}}_{1:8}$ can be represented as

$$\begin{aligned}\mathbf{c}^{\text{UNBAL, I}}_{1:8} \;=\; \Big[&1 + B\,(1 - x), 1 + \frac{R\,B\,x}{8}, 1 + \frac{R\,B\,x}{8}, 1 + \frac{R\,B\,x}{8}, 1 + \frac{R\,B\,x}{8},\\ &1 + \frac{R\,B\,x}{8}, 1 + \frac{R\,B\,x}{8}, 1 + \frac{R\,B\,x}{8}, 1 + \frac{R\,B\,x}{8}\Big].\end{aligned}$$

When comparing allocation decisions between $\text{BAL}^{\text{I}}_{1:1}$ and $\text{UNBAL}^{\text{I}}_{1:8}$, concentration bias predicts that individuals allocate more money to the concentrated early payoff in UNBAL than in BAL, because the costs of allocating money to this concentrated payoff are dispersed over the last 8 payment dates—and therefore less tangible—rather than concentrated—and therefore attention-grabbing—on the last

[3] The superscript I denotes one out of two cases of BAL trade-offs in our experiment, which will be explained further below.

payment date.[4] Thus, concentration bias predicts that in light of dispersed future costs, individuals behave *less patiently* than in light of concentrated future costs.

We also test for concentration bias in the opposite direction. That is, we investigate whether individuals behave more patiently when facing dispersed early costs in combination with a concentrated later benefit. We call the respective condition $\text{UNBAL}^{\text{II}}_{n:1}$; again, $n \in \{2, 4, 8\}$ denotes the number of payment dates over which the payoff is dispersed. In $\text{UNBAL}^{\text{II}}_{n:1}$, the dispersed payoff is the earlier payoff, while the later payoff is concentrated. In other words, $\text{UNBAL}^{\text{II}}_{n:1}$ simply reverses the temporal structure of $\text{UNBAL}^{\text{I}}_{1:n}$. Comparing allocation decisions between $\text{UNBAL}^{\text{II}}_{n:1}$ and the associated balanced condition $\text{BAL}^{\text{II}}_{1:1}$ allows us to answer the question whether concentration bias can also increase individuals' patience, relative to standard discounted utility.

The results of our main experiment support both predictions of concentration bias. Subjects allocated significantly more money to concentrated payoffs than to the associated dispersed payoffs. Moreover, the effect is the stronger, the larger the number of payment dates over which the dispersed payoffs are spread.

These results directly speak to the assumptions made by the aforementioned theories of economic choice based on stimulus-driven attention (Bordalo, Gennaioli, and Shleifer, 2012, 2013; Kőszegi and Szeidl, 2013; Bushong, Rabin, and Schwartzstein, 2016). Kőszegi and Szeidl (2013) build their model on the "focusing assumption" that a utility difference in a particular attribute attracts the more attention the larger the utility range spanned by the available options at that particular payment date is. Based on the focusing assumption, the model predicts concentration bias in intertemporal choice when each payment date of a budget set is taken to be a separate attribute. Our findings, therefore, are consistent with the main assumption of Kőszegi and Szeidl (2013). In their relative-thinking model, Bushong, Rabin, and Schwartzstein (2016) make the opposite assumption of Kőszegi and Szeidl (2013)—that is, individuals pay the less attention to deviations in a attribute, the larger the overall utility range spanned by the available options along that attribute is. Salience theory (Bordalo, Gennaioli, and Shleifer, 2012, 2013) is built on an assumption that is equivalent to the focusing assumption as well as the additional assumption of diminishing sensitivity, according to which the difference in an attribute attracts the less attention, the larger the average absolute value of that attribute is. Since our findings are consistent with the focusing assumption, our results are also predicted by versions of the salience model for which diminishing sensitivity plays a minor role. We provide a detailed discussion of the implications of our results

[4] Note that according to exponential discounting (with a positive discount rate and non-convex utility), the dispersed later payoff in $\text{UNBAL}^{\text{I}}_{1:n}$ is more attractive than the concentrated later payoff in $\text{BAL}^{\text{I}}_{1:1}$, because its present value is higher. Therefore, exponential discounting predicts—in contrast to concentration bias—that subjects allocate at most as much money to the first payment date in $\text{UNBAL}^{\text{I}}_{1:8}$ as in $\text{BAL}^{\text{I}}_{1:1}$, because the dispersed payoff in $\text{UNBAL}^{\text{I}}_{1:8}$ has a higher present value than the concentrated late payoff in $\text{BAL}^{\text{I}}_{1:1}$ as long as subjects discount future payments.

for Bordalo, Gennaioli, and Shleifer (2012, 2013), Kőszegi and Szeidl (2013), and Bushong, Rabin, and Schwartzstein (2016) in Section 2.2 and in Appendix A.

Our findings on concentration bias could also be explained by left-digit bias (Lacetera, Pope, and Sydnor, 2012) and goal-driven attention models: Individuals may mis-aggregate a dispersed payoff, because they pay relatively too much attention to the left-most digit of the individual payments and, therefore, arrive at smaller estimates of the sum of a dispersed payoff. Alternatively, individuals do not engage in the cognitively costly tasks of carefully calculating the sum of a dispersed payoff because of "rational" contemplations. Rather, they discount an estimate of the sum because of risk aversion. In order to differentiate between stimulus-driven attention models and these alternative explanations, we designed and conducted a control experiment. In a difference-in-differences analysis between our main and the control experiment, we find that subjects exhibit concentration bias beyond what can be explained by left-digit bias and goal-driven attention. This provides further evidence for the main assumption of Kőszegi and Szeidl (2013). However, we also find evidence that goal-driven attention and left-digit bias significantly affect intertemporal choice. Thus, our documented concentration bias is driven by both forces: stimulus-driven attention as well as left-digit bias and goal-driven attention.

Our results make two contributions to the understanding of intertemporal choice. First, previous research has primarily focused on modifying the exponential discounting by considering (quasi-)hyperbolic discounting (D. Laibson, 1997; O'Donoghue and Rabin, 1999) to study present-biased (and time-inconsistent) behavior. Concentration bias is complementary to hyperbolic discounting. While hyperbolic discounting predicts present bias even in the case that individuals face balanced trade-offs—for instance, when individuals trade off concentrated consequences—concentration bias in isolation predicts present-bias only in unbalanced trade-offs. In combination with hyperbolic discounting, concentration bias predicts present-biased behavior to be amplified when future consequences are dispersed rather than concentrated. This helps to explain a discrepancy between recent experimental findings and results from the analysis of field data: according to recent studies (Andreoni and Sprenger, 2012; Augenblick, Niederle, and Sprenger, 2015), the observed degrees of present bias are smaller in experimental settings than in many field contexts (e.g., low gym attendance, DellaVigna and Malmendier, 2006; resistance to the annuitization of pension plans, Warner and Pleeter, 2001; Davidoff, Brown, and Diamond, 2005). In experimental setups, individuals so far faced almost exclusively balanced trade-offs, while trade-offs are typically unbalanced in most field contexts. Second, concentration bias leads to more patient behavior when future consequences are concentrated. This insight may be relevant for policy makers who are interested in improving patience of individuals. For instance, withdrawals from 401(k) plans may loose their attractiveness to individuals when they are payed to individuals in a dispersed rather than concentrated manner.

Our evidence for concentration bias also explains the absence of evidence for the hedonic editing hypothesis (Thaler and Johnson, 1990). While diminishing sensitivity implies that individuals have a preference to integrate gains or separate losses, empirical tests find little to no support for this evidence (Thaler and Johnson, 1990; Lehenkari, 2009). According to concentration bias, individuals may prefer to integrate gains (thereby making them concentrated)—such that they are "noticeable"—and segregate (or disperse) losses—such that they are less noticeable.

Section 2 provides evidence for concentration bias in intertemporal choice. We analyze the control experiment in Section 3. In Section 4, we discuss the plausibility of potential alternative explanations. Section 5 concludes.

2 Evidence for Concentration Bias

This section provides evidence that concentration bias affects intertemporal decision making. In the following we present the design of the main experiment and derive behavioral predictions based on the focusing model of Kőszegi and Szeidl (2013). We then report and discuss the findings of the experiment.

2.1 Design

Our main experiment is designed to allow for a precise measurement of intertemporal decision making when decision makers face consequences over multiple periods. In particular, each participant makes intertemporal decisions of different types, i.e., with consequences that are either concentrated on a single payment date each (BAL) or one of the consequences is dispersed over multiple payment dates (UNBAL). Comparing how patiently individuals behave between those two types of decisions identifies concentration bias. This allows us to test the main predictions of Kőszegi and Szeidl (2013).

2.1.1 Intertemporal Choices. In our experiment, subjects make choices from multiple budget sets of which only one is randomly chosen to be payoff-relevant at the end of the experiment (random incentive mechanism). Each earnings sequence included in the different budget sets specifies a series of 9 money transfers to subjects' bank accounts at given dates in the future. (De facto, the earliest payment date was 5–7 days in the future.) We describe the precise structure of the budget sets and the included earnings sequences in the following paragraphs. Figures 1 and 2 illustrate the budget sets.

Subjects decide for each budget set whether to decrease earlier payments at the benefit of increasing later payments. The sum total is the greatest, the more money subjects allocate to later payment dates. Put differently, we implement an intertemporal budget constraint with a positive nominal interest rate, r. The more money subjects allocate to later payment dates, the more patient we consider them. In doing so, we extend the "convex budget set" approach to intertemporal decision mak-

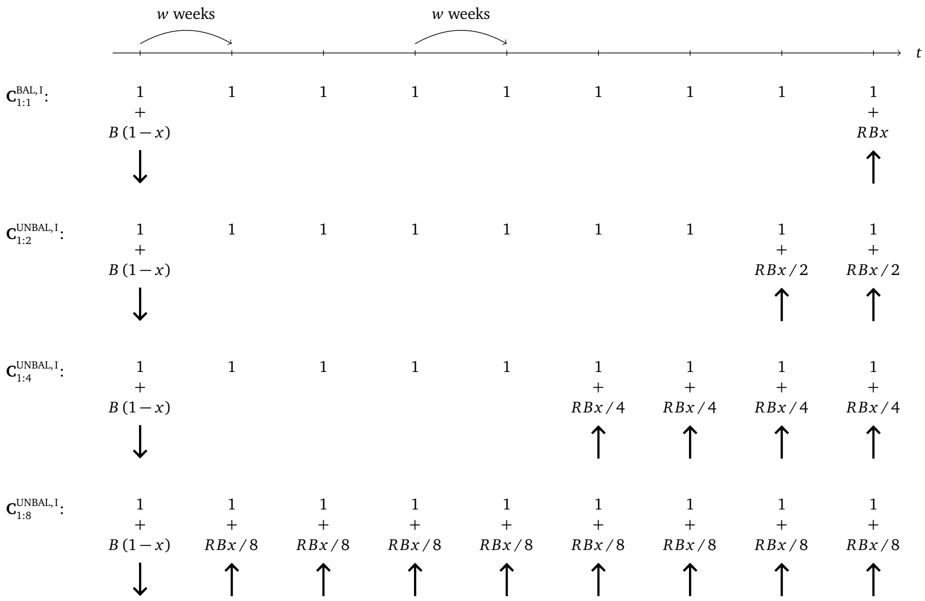


Figure 1. Budget Sets $\mathbf{C}^{\mathrm{BAL,\,I}}_{1:1}$ and $\mathbf{C}^{\mathrm{UNBAL,\,I}}_{1:n}$

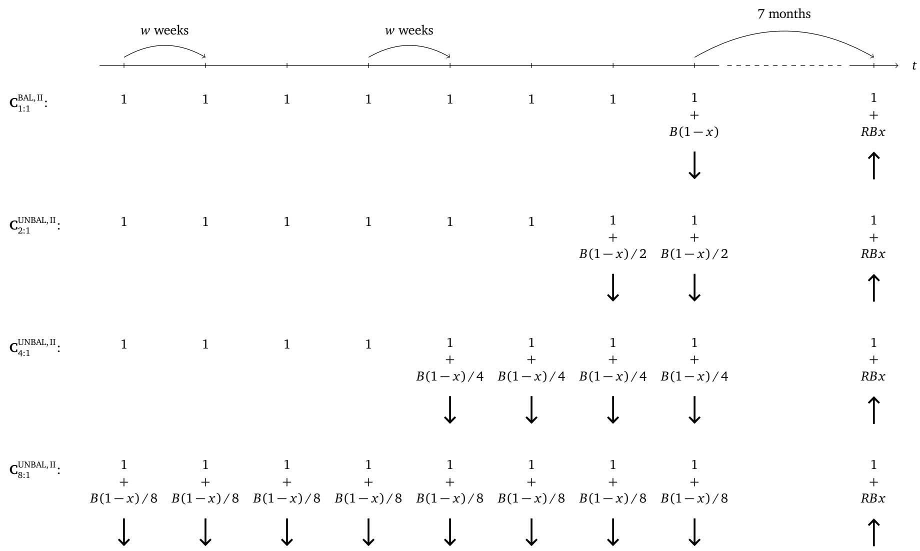


Figure 2. Budget Sets $\mathbf{C}^{\mathrm{BAL,\,II}}_{1:1}$ and $\mathbf{C}^{\mathrm{UNBAL,\,II}}_{n:1}$

ing introduced by Andreoni and Sprenger (2012) to settings in which individuals face more than two payment dates.

We vary within-subject the characteristics of the intertemporal budget constraint between two conditions, BAL and UNBAL. In both conditions, subjects receive a fixed amount of €1 at each of the 9 payment dates to hold the number of transfers constant across conditions. Subjects allocate an additional amount of money between payment dates. In BAL, the allocation is between exactly two payment dates; the intertemporal allocation thus involved consequences that are concentrated on single payment dates each. Decreasing (increasing) a payment increases (decreased) a payment on exactly *one* other date. By contrast, in UNBAL, subjects allocate money between multiple payment dates. More precisely, there is one consequence that is concentrated on a single date, while the other consequence is dispersed over multiple dates. Decreasing (increasing) the concentrated consequence increases (decreased) the payments on *several* (2, 4, or 8) other dates.

BAL consists of two types of budget sets, $\mathbf{C}_{1:1}^{\mathrm{BAL,I}}$ and $\mathbf{C}_{1:1}^{\mathrm{BAL,II}}$. In $\mathrm{BAL}_{1:1}^{\mathrm{I}}$, subjects can shift money from the earliest to the last payment date at the benefit of receiving interest. In $\mathrm{BAL}_{1:1}^{\mathrm{II}}$, subjects allocate money between the second-to-last and the last payment date. In both $\mathrm{BAL}_{1:1}^{\mathrm{I}}$ and $\mathrm{BAL}_{1:1}^{\mathrm{II}}$, 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 $x \in \mathbf{X} = \{0, {}^1\!/_{100}, {}^2\!/_{100}, \ldots, 1\}$, which determines an earlier payment of $B\,(1-x)$ euros and a later payment of $RB\,x$ euros. While each payment date is separated by w weeks in $\mathrm{BAL}_{1:1}^{\mathrm{I}}$, this is true only for the first 8 payment dates in $\mathrm{BAL}_{1:1}^{\mathrm{II}}$. The distance between the second-to-last and last payment date is 7 months in $\mathrm{BAL}_{1:1}^{\mathrm{II}}$. We chose this large gap between $t = 8$ and $t = 9$ in order to minimize ceiling effects, i.e., in order to avoid a situation in which subjects exclusively choose the largest, latest payment.

UNBAL consists of two types of budget sets, $\mathbf{C}_{1:n}^{\mathrm{UNBAL,I}}$ and $\mathbf{C}_{n:1}^{\mathrm{UNBAL,II}}$, that are related to $\mathbf{C}_{1:1}^{\mathrm{BAL,I}}$ and $\mathbf{C}_{1:1}^{\mathrm{BAL,II}}$, respectively. In $\mathrm{UNBAL}_{1:n}^{\mathrm{I}}$, subjects allocate monetary amounts between the earliest payment date and multiple later payments, where $n \in \{2, 4, 8\}$ denotes the number of payment dates over which the later payoff is dispersed. Thus, instead of receiving $RB\,x$ euros at the last payment date, like in $\mathrm{BAL}_{1:1}^{\mathrm{I}}$, the amount of $RB\,x$ euros is dispersed over the final and the previous $n-1$ payment dates in $\mathrm{UNBAL}_{1:n}^{\mathrm{I}}$. By contrast, in $\mathrm{UNBAL}_{n:1}^{\mathrm{II}}$, subjects allocate money between multiple earlier payments—the number of payment dates is again $n \in \{2, 4, 8\}$—and a single later payment. Instead of receiving $B\,(1-x)$ euros at the second-to-last date, like in $\mathrm{BAL}_{1:1}^{\mathrm{II}}$, the amount of $B\,(1-x)$ euros is dispersed over the second-to-last payment date and multiple $(n-1)$ earlier dates, so that $B\,(1-x)\,/\,n$ euros are paid per date. The interval between payment dates follows the respective BAL counterpart for each UNBAL condition.

In a first step, we are interested in the comparison of chosen allocations between $\mathrm{BAL}_{1:1}^{\mathrm{I}}$ and $\mathrm{UNBAL}_{1:n}^{\mathrm{I}}$. This comparison tests whether subjects behave differently in the case that the negative consequences of behaving impatiently, i.e., of choosing a smaller x, are dispersed over multiple future dates ($\mathrm{UNBAL}_{1:n}^{\mathrm{I}}$) rather than concentrated on a single future date ($\mathrm{BAL}_{1:1}^{\mathrm{I}}$). Concentration bias predicts that individuals underweight dispersed consequences relative to concentrated consequences.[5] In $\mathrm{UNBAL}_{1:n}^{\mathrm{I}}$, the negative consequences of behaving impatiently are less attention-grabbing, as they are dispersed in the form of small payments over several

[5] This will be discussed in greater detail below.

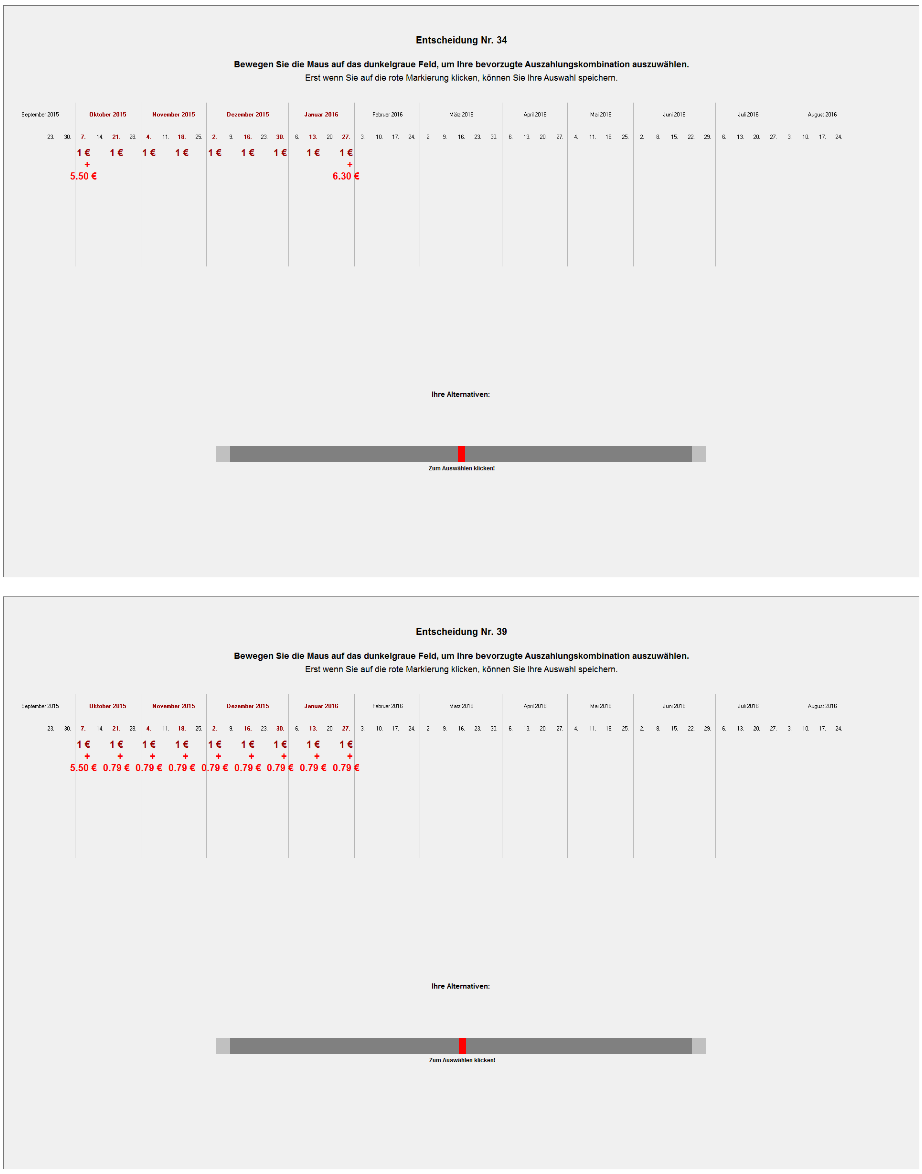


Figure 3. Screenshots of a $\text{BAL}^{\text{I}}_{1:1}$ Decision (Top) and an $\text{UNBAL}^{\text{I}}_{1:8}$ Decision (Bottom)

Note: For the values of B, R, and w that we used, see Section 2.1.3. The arrows indicate whether and in which direction payments at the respective payment dates change if the savings rate x is increased.

dates. By contrast, in $\text{BAL}^{\text{I}}_{1:1}$, the negative consequences of behaving impatiently are concentrated in a single—i.e., attention-grabbing—payment. Thus, individuals are predicted to pay more attention to the negative consequences in $\text{BAL}^{\text{I}}_{1:1}$ than in $\text{UNBAL}^{\text{I}}_{1:n}$, which promotes impatient behavior in the latter condition.

Figure 3 shows the decision screen of an exemplary decision with $B = €11$ and $r \approx 15\%$ for both $\text{BAL}^{\text{I}}_{1:1}$ (upper panel) and $\text{UNBAL}^{\text{I}}_{1:8}$ (lower panel). Through a slider, subjects chose their preferred $x \in \mathbf{X}$. The slider position in Figure 3 indicates $x = 0.5$, i.e., the earliest payment is reduced by €5.50. Since $r \approx 15\%$, this amounts to €6.30 that are paid at later payment dates. While these €6.30 are paid in a single sum on the latest payment date in $\text{BAL}^{\text{I}}_{1:1}$, they are dispersed into equal parts over the last 8 payment dates—i.e., 8 consecutive payments of €0.79—in $\text{UNBAL}^{\text{I}}_{1:8}$.[6] Concentration bias predicts that the dispersed payoff of €6.30 will be underweighted relative to the concentrated payoff of €6.30.

In the second step, we are also interested in the comparison of allocation decisions between $\text{BAL}^{\text{II}}_{1:1}$ and $\text{UNBAL}^{\text{II}}_{n:1}$. To reiterate, concentration bias predicts that individuals underweight dispersed consequences relative to concentrated consequences. Since the negative consequences of behaving patiently, i.e., of choosing a large x, are dispersed in $\text{UNBAL}^{\text{II}}_{n:1}$, individuals tend to neglect them according to concentration bias. By contrast, in $\text{BAL}^{\text{II}}_{1:1}$, the negative consequences of behaving patiently are concentrated in a single—i.e., attention-grabbing—payment. Therefore, concentration bias predicts that individuals pay more attention to the negative consequences in $\text{BAL}^{\text{II}}_{1:1}$ than in $\text{UNBAL}^{\text{II}}_{n:1}$, which promotes patient behavior in the latter condition.

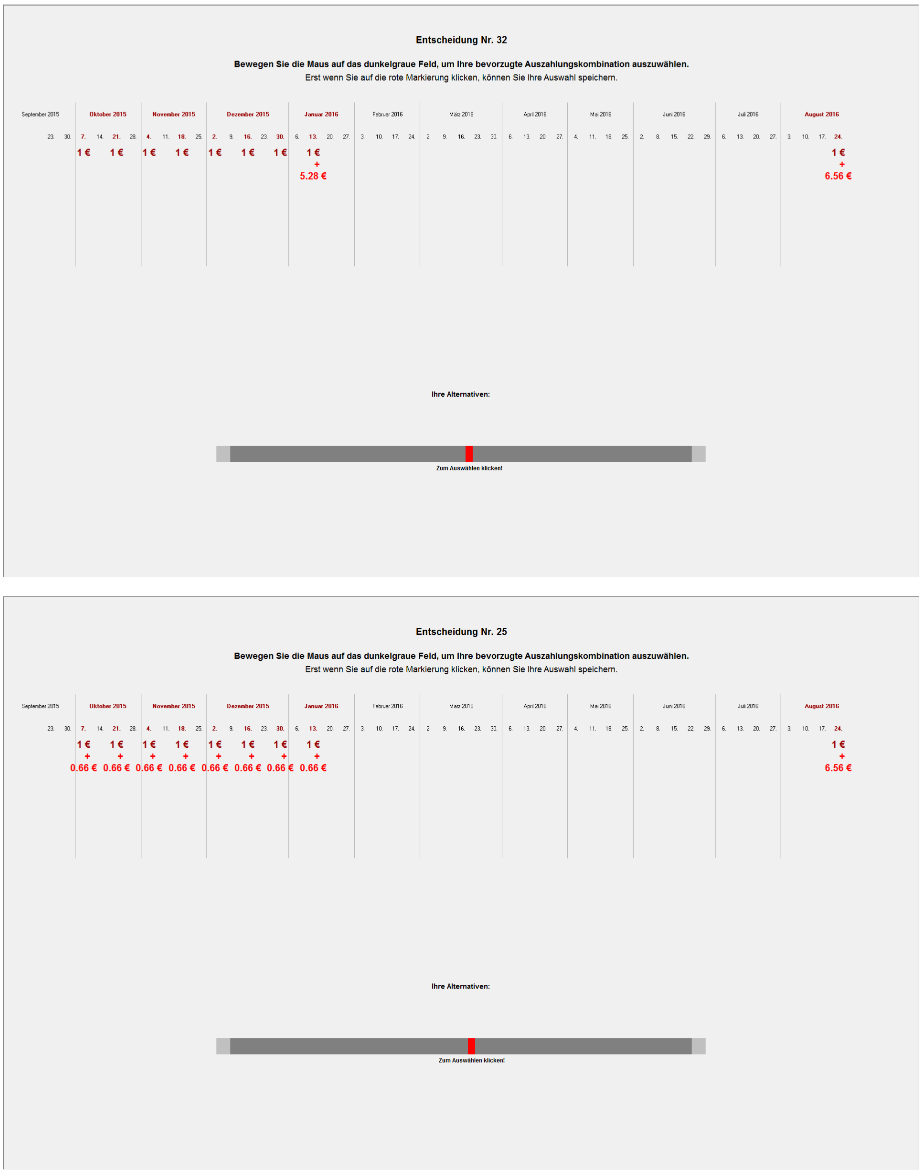
Figure 4 shows the decision screen of an exemplary decision with $B = €11$ and $r \approx 15\%$ for both $\text{BAL}^{\text{II}}_{1:1}$ (upper panel) and $\text{UNBAL}^{\text{II}}_{8:1}$ (lower panel). The slider position in Figure 4 indicates $x = 0.48$, which implies that €6.56 are paid at the latest payment date. While the remaining $B\,(1-x) = €5.28$ are paid as a single sum on the second-to-last payment date in $\text{BAL}^{\text{II}}_{1:1}$, they are dispersed into equal parts over the first 8 payment dates—i.e., 8 consecutive payments of €0.66—in $\text{UNBAL}^{\text{II}}_{8:1}$.

2.1.2 Decision Time, Cognitive Reflection Test, and Calculation Task. Concentration bias could be understood as a heuristic-like decision-making process that differs from deliberate contemplation over the advantages and disadvantages—or benefits and costs—of an action. This suggests the potential for heterogeneity in the degree to which individuals are affected by concentration bias. First, an indicator for use of heuristics would be if individuals spend less time deciding on trade-offs in the UNBAL trials than the BAL trials—despite the fact that UNBAL trials are cognitively more demanding. Second, individuals who are less able to control their impulses might be more prone to concentration bias. Third, individuals that are less capable of calculating sums of payoffs might exhibit more pronounced concentration bias. We test for these three sources of potential heterogeneity by measuring decision time, letting subjects complete the Cognitive Reflection Test (CRT; Frederick, 2005) and a calculation (mental-arithmetic) task at the end of the experiment.

We measure the overall seconds individuals take to decide for each budget set.

The CRT measures the degree to which individuals are prone to let their decision making be governed by their impulses rather than deliberate contemplation (see

[6] We always rounded the second decimal place up so that the sum of the payments included in a dispersed payoff was always at least as great as the respective concentrated payoff.



(Bottom)

Figure 4. Budget Sets: Screenshots of a $\text{BAL}^{\text{II}}_{1:1}$ Decision (Top) and an $\text{UNBAL}^{\text{II}}_{8:1}$ Decision

Frederick, 2005; Oechssler, Roider, and Schmitz, 2009). We did not incentivize the CRT.

We use the calculation task to proxy subjects' capability to aggregate consequences. Since the consequences in this experiment were sums of monetary payments, we asked subjects to calculate sums of strings of small and repetitive decimal numbers. Subjects were asked to calculate as many sums as they could in five minutes. The strings were between four and nine numbers long; for instance, subjects were asked to calculate "$1.35 + 1.35 + 1.35 + 1.35 + 1.35 + 1.35 + 1.35$" or "$1.71 + 1.71 + 1.71 + 1.71 + 1.71 + 1.71 + 1.71 + 1.71 + 1.71$." We use precisely this calculation task, because it closely mirrors the type of aggregation that is required for the intertemporal decisions that individuals face in the experiment. For solving a string correctly, subjects received €0.20. If they did not solve a string correctly within three attempts, €0.05 were deducted from their earnings. To avoid negative earnings, subjects received an initial endowment of €1. Earnings for the calculation task were paid in cash at the end of the respective session.

2.1.3 Procedure. The experiment was conducted in two waves at the BonnEconLab in spring and summer 2015.

In the first wave, each subject made 36 choices across different budget sets. One set of subjects ($N = 47$) faced 12 budget sets ($w \in \{3, 6\}$; $B = 8$, $r \in \{20\%, 50\%, 80\%\}$; and $B = 11$, $r \in \{15\%, 36\%, 58\%\}$) of each of the types $\text{BAL}^{\text{I}}_{1:1}$, $\text{UNBAL}^{\text{I}}_{1:4}$, and $\text{UNBAL}^{\text{I}}_{1:8}$ $[2 \times 2 \times 3 \times 3 = 36]$. A second set of subjects ($N = 46$) faced the same parameters for $\text{BAL}^{\text{II}}_{1:1}$, $\text{UNBAL}^{\text{II}}_{4:1}$, and $\text{UNBAL}^{\text{II}}_{8:1}$ budget sets. In the second wave, each subject made 32 choices across different budget sets: all subjects ($N = 92$) received four budget sets ($w \in \{2, 3\}$, $B = 11$, $r \in \{15\%, 58\%\}$) of each of the two BAL ($\text{BAL}^{\text{I}}_{1:1}$, $\text{BAL}^{\text{II}}_{1:1}$) and the three respective UNBAL types ($\text{UNBAL}^{\text{I}}_{1:n}$, $\text{UNBAL}^{\text{II}}_{n:1}$, $n \in \{2, 4, 8\}$) $[2 \times 2 \times (2 + 3 + 3) = 32]$.[7]

Experimental sessions took place on Thursday or Friday. The earliest bank transfer for any earnings sequence was always next week's Wednesday. Thus, subjects' earnings sequences always started 5 or 6 days in the future. Recall that we are interested in the within-subject difference of intertemporal choices between BAL and UNBAL budget sets. Since BAL and UNBAL earnings sequences always start at the same point in time per subject, the temporal distance between the experiment and the first payment date is irrelevant.

Overall, subjects in both waves were also asked to choose from additional earnings sequences presented in the form of 24 (first wave) and 28 (second wave) choice lists. In this paper, we do not analyze these choice lists. The choice lists also test for concentration bias but in a different manner. While the choice lists yield equally supportive evidence for concentration bias, they do not allow for the comparison with the control experiment that we report in Section 3. We therefore include a more detailed description and our analysis of behavior for the choice lists in Section B.1.

[7] In the first wave, participation in Condition I or II was randomized between-subjects. This was because we initially considered including an interest rate as high as $r = 80\%$ reasonable, given that in previous studies, participants had exhibited extremely strong discounting (see, e.g., Figure 2 from Dohmen et al., 2010). It turned out, however, that this led to ceiling effects. In response to this, we decided against such extreme trials for the second wave. Instead, we used the time freed up by the omission of trials with such an extreme interest rate to let all subjects in the second wave participate in both Condition I and Condition II. This is unproblematic because all BAL–UNBAL comparisons are nevertheless within-subject comparisons. Moreover, with virtually the same number of subjects in Condition I and in Condition II during the first wave (47 vs. 46), calculating averages across both conditions does not suffer from unequal group sizes. Please note that the findings regarding BAL–UNBAL differences that we present below are rather conservative due to the ceiling effects.

Each session of the experiment lasted 90 minutes. Subjects earned on average €21.61. They were not allowed to use any auxiliary electronic devices during the experiment. We used the software z-Tree (Fischbacher, 2007) for conducting the experiment and hroot (Bock, Baetge, and Nicklisch, 2014) for inviting subjects from the BonnEconLab's subject pool and recording their participation. Prior to their participation, subjects gave informed consent and agreed to providing us with their bank details (this prerequisite had already been mentioned in the invitation messages sent out to subjects via hroot).

2.2 Predictions

In this section, we examine predictions regarding individuals' behavior in the main experiment. In particular, we consider two distinct cases: individuals base their allocation decisions solely on standard time preferences (discounted utility); or individuals are "focused thinkers," that is, they are affected by concentration bias in the way specified by Kőszegi and Szeidl (2013). We also discuss the conditions under which predictions for two alternative stimulus-driven attention approaches, salience theory (Bordalo, Gennaioli, and Shleifer, 2013) and relative thinking (Bushong, Rabin, and Schwartzstein, 2016), can be derived for our setup.

By discounted utility, we refer to any intertemporal utility function that is time-separable and that values a payment further in the future at most as much as an equal-sized payment closer in the future.[8] Importantly, the predictions derived below hold for all three frequently used types of discounting—exponential, hyperbolic, and quasi-hyperbolic—and not only exponential discounting.

Subjects were endowed with multiple budget sets. Each earnings sequence in budget set $\mathbf{C}$ comprised 9 payments. For each budget set, subjects chose a share x of the early payment(s) to allocate to later payment dates. Between the BAL and UNBAL conditions, we varied the type of the intertemporal budget constraint (one could also call this the "allocation technology"). Table 1 lists $\mathbf{C}$ for the different types of intertemporal budget constraints that we implemented. Prior to subjects' allocation decision $x \in \mathbf{X}$ with $\mathbf{X} = \{0, 1/100, 2/100, \dots, 1\}$, each budget set $\mathbf{C}$ is a set of 101 earnings sequences. Out of this set, an allocation decision x determines a unique instance $\mathbf{c}$ that specifies payments c_t for the payment dates $t = 1, \dots, 9$. For example, in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$, a choice of $x = 0.5$ implemented the earnings sequence $\mathbf{c} = [c_1, \dots, c_9] = [1 + B/2, 1, 1, 1, 1, 1, 1, 1, 1 + R(B/2)]$.

In the following, we assume that subjects base their decisions on utility derived from receiving the monetary payments c_t at date t. This is an assumption that is frequently made in experiments on intertemporal decision making. One way to justify this assumption is that individuals anticipate to consume the payments they receive

[8] In many articles, discounted utility and exponential discounting are treated as synonymous. In contrast to this, other authors use discounted utility as the generic concept and regard particular types of discounting, such as exponential, hyperbolic, or quasi-hyperbolic, as instances of discounted utility. We use the latter terminology.

Table 1. Budget Sets Offered to Subjects across Trials

$\mathbf{C}_{1:1}^{\mathrm{BAL,I}}$	$= \left[1+B(1-x), \quad 1,1,1,1,1,1,1,1+RBx\right]$
$\mathbf{C}_{1:2}^{\mathrm{UNBAL,I}}$	$= \left[1+B(1-x), \quad 1,1,1,1,1,1,1+\frac{RBx}{2},1+\frac{RBx}{2}\right]$
$\mathbf{C}_{1:4}^{\mathrm{UNBAL,I}}$	$= \left[1+B(1-x), \quad 1,1,1,1,1+\frac{RBx}{4},1+\frac{RBx}{4},1+\frac{RBx}{4},1+\frac{RBx}{4}\right]$
$\mathbf{C}_{1:8}^{\mathrm{UNBAL,I}}$	$= \left[1+B(1-x), \quad 1+\frac{RBx}{8},1+\frac{RBx}{8},1+\frac{RBx}{8},1+\frac{RBx}{8},1+\frac{RBx}{8},1+\frac{RBx}{8},1+\frac{RBx}{8},1+\frac{RBx}{8}\right]$
$\mathbf{C}_{1:1}^{\mathrm{BAL,II}}$	$= \left[1,1,1,1,1,1,1, \quad 1+B(1-x), \quad 1+RBx\right]$
$\mathbf{C}_{2:1}^{\mathrm{UNBAL,II}}$	$= \left[1,1,1,1,1,1, \quad 1+\frac{B(1-x)}{2},1+\frac{B(1-x)}{2}, \quad 1+RBx\right]$
$\mathbf{C}_{4:1}^{\mathrm{UNBAL,II}}$	$= \left[1,1,1,1, \quad 1+\frac{B(1-x)}{4},1+\frac{B(1-x)}{4},1+\frac{B(1-x)}{4},1+\frac{B(1-x)}{4}, \quad 1+RBx\right]$
$\mathbf{C}_{8:1}^{\mathrm{UNBAL,II}}$	$= \left[1+\frac{B(1-x)}{8},1+\frac{B(1-x)}{8},1+\frac{B(1-x)}{8},1+\frac{B(1-x)}{8}, \quad 1+\frac{B(1-x)}{8},1+\frac{B(1-x)}{8},1+\frac{B(1-x)}{8},1+\frac{B(1-x)}{8}, \quad 1+RBx\right]$

Note: x is the choice variable. Subjects chose some $x \in \mathbf{X}$ in each trial.

within a short period around date t. Given that the maximum payment was below €20 and that any two payment dates were separated by at least two weeks, this assumption seems reasonable (see the arguments in favor of this view in Halevy, 2014). Kőszegi and Szeidl (2013) themselves make the same assumption of "money in the utility function": "in some applications we also assume that monetary transactions induce direct utility consequences, so that for instance an agent making a payment experiences an immediate utility loss. The idea that people experience monetary transactions as immediate utility is both intuitively compelling and supported in the literature: … some evidence on individuals' attitudes toward money, such as narrow bracketing (…) and laboratory evidence on hyperbolic discounting (…), is difficult to explain without it." Last but not least, the papers by McClure, D. I. Laibson, et al. (2004) and McClure, Ericson, et al. (2007) demonstrate that brain activation, as measured by functional magnetic resonance imaging, is similar for primary and monetary rewards.

Additionally, we make the standard assumption that utility from money is increasing in its argument but not convex, i.e., $u'(c_t) \geq 0$ and $u''(c_t) \leq 0$.[9]

2.2.1 Discounted Utility. Individuals make their allocation decisions by comparing the aggregated consumption utility of each earnings sequence $\mathbf{c} \in \mathbf{C}$. Discounted

[9] We discuss the plausibility of this assumption in detail in Section 4.2.

utility assumes that the utility of each period enters overall utility additively. That is, utility derived from the payment to be received at future date t can be expressed as $u_t(c_t) \coloneqq D(t)\,u(c_t)$. Here, $D(t)$ denotes the individual's discount function for conversion of future utility into present utility. The discount function satisfies $0 \leq D(t)$ and $D'(t) \leq 0$, such that a payment further in the future is valued at most as much as an equal-sized payment closer in the future.[10]

The utility of earnings sequence $\mathbf{c}$ with payments c_t in periods $t = 1, \dots, T$ is then given by

$$U(\mathbf{c}) \;=\; \sum_{t=1}^{T} u_t(c_t) \;=\; \sum_{t=1}^{T} D(t)\,u(c_t). \tag{1}$$

Individuals choose how much to allocate to the different periods by maximizing their utility over all possible earnings sequences available within a given budget set $\mathbf{C}$.

$\mathbf{BAL}^{\mathrm{I}}_{1:1}$ vs. $\mathbf{UNBAL}^{\mathrm{I}}_{1:n}$. We consider $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ and $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$ first. In $\mathrm{BAL}^{\mathrm{I}}_{1:1}$, individuals decide how much to allocate to the different payment dates by choosing

$$\begin{aligned} x^\star(\mathbf{C}^{\mathrm{BAL,I}}_{1:1}; B, R) \;&\coloneqq \\ &\arg\max_{x \in \mathbf{X}} D(1)\,u(1 + B\,(1 - x)) + \sum_{t=2}^{8} D(t)\,u(1) + D(9)\,u(1 + RBx), \end{aligned}$$

and in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$ by choosing

$$\begin{aligned} x^\star(\mathbf{C}^{\mathrm{UNBAL,I}}_{1:n}; B, R) \coloneqq \\ &\arg\max_{x \in \mathbf{X}} D(1)\,u(1 + B\,(1 - x)) + \sum_{t=2}^{9-n} D(t)\,u(1) \\ &\qquad\qquad + \sum_{t=9-(n-1)}^{9} D(t)\,u(1 + RBx/n). \end{aligned}$$

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. While the marginal negative consequences of being patient, i.e., of increasing x, are the same across $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ and $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$,

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

the marginal positive consequences are weakly smaller in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ than in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$,

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

[10] Normalization such that $D(t) \leq 1$ is not necessary in our case. Provided that t is a metric time measure where $t = 0$ stands for the present, examples are $D(t) \coloneqq \delta^t$ with some $\delta > 0$ for exponential discounting and $D(t) \coloneqq (1 + \alpha\,t)^{-\gamma/\alpha}$ with some $\alpha, \gamma > 0$ for generalized hyperbolic discounting.

This effect is driven both by the (weak) concavity of the utility function u and the fact that in $\text{UNBAL}^{\text{I}}_{1:n}$, parts of the positive consequences occur earlier and are, thus, discounted less. Therefore, individuals allocate to later payment dates at least as much in $\text{UNBAL}^{\text{I}}_{1:n}$ as in $\text{BAL}^{\text{I}}_{1:1}$. Hence, collectively, we have

$$d^{\star}_{\text{I},n}(B,R) \;:=\; x^{\star}(\mathbf{C}^{\text{BAL, I}}_{1:1};B,R) - x^{\star}(\mathbf{C}^{\text{UNBAL, I}}_{1:n};B,R) \;\leq\; 0,$$

with $d^{\star}_{\text{I},8}(B,R) \leq d^{\star}_{\text{I},4}(B,R) \leq d^{\star}_{\text{I},2}(B,R)$.

In the following, let $d^{\star}_{\text{I}}$ denote the mean of all $d^{\star}_{\text{I},n}(B,R)$ and let $x^{\star}(\mathbf{C}^{\text{UNBAL, I}}_{1:\bullet})$ be the mean of all $x^{\star}(\mathbf{C}^{\text{UNBAL, I}}_{1:n};B,R)$ for $n \in \{2,4,8\}$ and for all B and R. That is, when referring to means over all n, B, and R, we replace the n by $\bullet$ in the subscript. Thus, discounted utility predicts that on average (across parameters B and R as well as across $\text{UNBAL}^{\text{I}}_{1:2}$, $\text{UNBAL}^{\text{I}}_{1:4}$, and $\text{UNBAL}^{\text{I}}_{1:8}$), subjects are at least as patient in $\text{UNBAL}^{\text{I}}_{1:\bullet}$ as in $\text{BAL}^{\text{I}}_{1:1}$, i.e.,

$$d^{\star}_{\text{I}} \;:=\; x^{\star}(\mathbf{C}^{\text{BAL, I}}_{1:1}) - x^{\star}(\mathbf{C}^{\text{UNBAL, I}}_{1:\bullet}) \;\leq\; 0. \tag{2}$$

The intuition behind this prediction is as follows: The latest payoff, which is concentrated in $\text{BAL}^{\text{I}}_{1:1}$, is dispersed over n payment dates in $\text{UNBAL}^{\text{I}}_{1:n}$. Importantly, the latest payment of the dispersed payoff is transferred at the same date as the latest payment in $\text{BAL}^{\text{I}}_{1:1}$. All other payments in the dispersed payoff are transferred earlier, as illustrated in Figure 1. Thus, a large part of the later payoff is discounted to a lesser degree in $\text{UNBAL}^{\text{I}}_{1:n}$ than in $\text{BAL}^{\text{I}}_{1:1}$. Therefore, subjects are weakly better off in $\text{UNBAL}^{\text{I}}_{1:n}$ than in $\text{BAL}^{\text{I}}_{1:1}$ if the same amount of money is allocated to later dates. Consequently, according to discounted utility, subjects allocate at least as much to later payment dates in $\text{UNBAL}^{\text{I}}_{1:n}$ as in $\text{BAL}^{\text{I}}_{1:1}$.

$\text{BAL}^{\text{II}}_{1:1}$ vs. $\text{UNBAL}^{\text{II}}_{n:1}$. We consider $\text{BAL}^{\text{II}}_{1:1}$ and $\text{UNBAL}^{\text{II}}_{n:1}$ next. In $\text{BAL}^{\text{II}}_{1:1}$, individuals decide how much to save by choosing

$$x^{\star}(\mathbf{C}^{\text{BAL, II}}_{1:1};B,R) \;:=\; \arg\max_{x\in\mathbf{X}} \sum_{t=1}^{7} D(t)\,u(1) + D(8)\,u(1+B\,(1-x)) + D(9)\,u(1+RBx),$$

and in $\text{UNBAL}^{\text{II}}_{n:1}$, by choosing

$$x^{\star}(\mathbf{C}^{\text{UNBAL, II}}_{n:1};B,R) \;:=\; \arg\max_{x\in\mathbf{X}} \sum_{t=1}^{8-n} D(t)\,u(1) + \sum_{t=8-(n-1)}^{8} D(t)\,u(1+(B\,(1-x))\,/\,n) + D(9)\,u(1+RBx).$$

Here, the following holds. The marginal positive consequences of postponing, i.e., of increasing x, are identical across $\text{BAL}^{\text{II}}_{1:1}$ and $\text{UNBAL}^{\text{II}}_{n:1}$, $D(9)\,u'(1+RBx)\times b$. At the same time, the marginal negative consequences are greater in absolute terms in $\text{UNBAL}^{\text{II}}_{n:1}$ than in $\text{BAL}^{\text{II}}_{1:1}$,

$$\sum_{t=8-(n-1)}^{8} D(t)\, u'(1 + (B\,(1-x))\,/\,n) \times (-B\,/\,n) \\ \leq\; D(8)\, u'(1 + B\,(1-x)) \times (-B).$$

This effect is, again, driven both by the (weak) concavity of the utility function u and the fact that in $\text{UNBAL}^{\text{II}}_{n:1}$, parts of the negative consequences occur earlier and are, thus, discounted less. Therefore, individuals save at most as much in $\text{UNBAL}^{\text{II}}_{n:1}$ as in $\text{BAL}^{\text{II}}_{1:1}$.

The second-to-last payoff of $\text{BAL}^{\text{II}}_{1:1}$ is dispersed over n earlier dates in $\text{UNBAL}^{\text{II}}_{n:1}$, as is illustrated in Figure 2. Thus, a larger share of the total amount stemming from the earlier payments is discounted over a shorter time span in $\text{UNBAL}^{\text{II}}_{n:1}$ than in $\text{BAL}^{\text{II}}_{1:1}$. This induces subjects to save at most as much in $\text{UNBAL}^{\text{II}}_{n:1}$ as in $\text{BAL}^{\text{II}}_{1:1}$ if they behave in line with discounted utility.

Define $d^{\star}_{\text{II},n}(B,R)$ in analogue to $d^{\star}_{\text{I},n}(B,R)$ above. Let $d^{\star}_{\text{II}}$ denote the mean of all $d^{\star}_{\text{II},n}(B,R)$, and let $x^{\star}(\mathbf{C}^{\text{UNBAL, II}}_{\bullet:1})$ be the mean of all $x^{\star}(\mathbf{C}^{\text{UNBAL, II}}_{n:1}; B,R)$ for $n \in \{2,4,8\}$ and for all B and R used in our study. Discounted utility then predicts for the average over all n, B, and R that

$$d^{\star}_{\text{II}} \;:=\; x^{\star}(\mathbf{C}^{\text{UNBAL, II}}_{\bullet:1}) - x^{\star}(\mathbf{C}^{\text{BAL, II}}_{1:1}) \;\leq\; 0. \tag{3}$$

Since dispersion is greatest in $\text{UNBAL}^{\text{II}}_{8:1}$ and least pronounced in $\text{UNBAL}^{\text{II}}_{2:1}$, we have $d^{\star}_{\text{II},8} \leq d^{\star}_{\text{II},4} \leq d^{\star}_{\text{II},2} \leq 0$.

2.2.2 Concentration Bias. In this section, we extend the model of standard discounted utility with the weighting function g as proposed by Kőszegi and Szeidl (2013). Period-t weights g_t scale period-t consumption utility u_t. Individuals are assumed to maximize focus-weighted utility, which is defined as follows:

$$\tilde{U}(\mathbf{c},\mathbf{C}) \;:=\; \sum_{t=1}^{T} g_t(\mathbf{C})\, u_t(c_t). \tag{4}$$

$\tilde{U}$ has two arguments, the earnings sequence $\mathbf{c}$ and the choice set (budget set) $\mathbf{C}$, because the weights g_t are given by a strictly increasing weighting function g which, in turn, takes as its argument the difference between the maximum and minimum possible utility for period t over all possible earnings sequences in set $\mathbf{C}$:

$$g_t(\mathbf{C}) \;:=\; g[\Delta_t(\mathbf{C})] \quad \text{with} \quad \Delta_t(\mathbf{C}) \;:=\; \max_{\mathbf{c}'\in\mathbf{C}} u_t(c'_t) - \min_{\mathbf{c}'\in\mathbf{C}} u_t(c'_t). \tag{5}$$

If the underlying consumption utility function is characterized by discounted utility, as above, then $u_t(c_t) := D(t)\, u(c_t)$. That is, focused thinkers put more weight on period t than on period t' if the discounted-utility-distance between the best and worst alternative is larger for period t than for period t'.

$\mathrm{BAL}^{\mathrm{I}}_{1:1}$ vs. $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$. We consider the implications of focus weighting on savings decisions in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ and $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$ first. We will see that the following intuition is captured by including g in the aggregation of consequences: In $\mathrm{BAL}^{\mathrm{I}}_{1:1}$, the positive consequences of being patient are concentrated on the last payment date and are, therefore, attention-grabbing. By contrast, in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$, the positive consequences of saving are less noticeable, as they are dispersed over several payment dates.

For $\mathrm{BAL}^{\mathrm{I}}_{1:1}$, date-1 utility ranges from $u_1(1)$ to $u_1(1+B)$ ($x=1$ or $x=0$, respectively), while date-9 utility ranges from $u_9(1)$ to $u_9(1+RB)$. For $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$, date-1 utility also ranges from $u_1(1)$ to $u_1(1+B)$. However, date-9 utility now ranges only from $u_9(1)$ to $u_9(1+RB/n)$ in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$. Thus, date-9 utility receives a lower weight in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$ than it receives in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$, $g_9(\mathbf{C}^{\mathrm{BAL,I}}_{1:1}) > g_9(\mathbf{C}^{\mathrm{UNBAL,I}}_{1:n})$. 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, i.e., $g_9(\mathbf{C}^{\mathrm{UNBAL,I}}_{1:2}) > g_9(\mathbf{C}^{\mathrm{UNBAL,I}}_{1:4}) > g_9(\mathbf{C}^{\mathrm{UNBAL,I}}_{1:8})$. In exchange for this downweighting of u_9, the preceding dates $t' = 8-(n-2),\dots,8$ receive a larger weight $g_{t'}$ in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$ than in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$. This is because for those payment dates, $\max u_{t'} - \min u_{t'} = 0$ in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$, while it is positive in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$. Importantly, g is strictly increasing. If g is sufficiently steep, then the relatively large weight g_9 multiplied by u_9 plus the sum over $g_{t'}u_1$ in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ is greater than the sum of the multiple smaller weights $g_{t'}$ multiplied by the per-period utility $u_{t'}$, including $g_9 u_9$, in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$.

Expressed verbally, the positive consequences of being patient are underweighted in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$ relative to $\mathrm{BAL}^{\mathrm{I}}_{1:1}$. If this relative underweighting of dispersed payoffs in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$ is sufficiently strong, focus-weighted utility predicts larger marginal utility from being patient in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ than in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$. In that case, the prediction of the standard model—as specified in equation (2)—is reversed: focused thinkers may want to save more in $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ than in $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$. Let ** indicate optimal choices according to discounted utility in combination with focusing (in analogue to * indicating optimal choices under discounted utility without focus weighting). Then we have, with a sufficiently steep weighting function g,[11]

$$d^{\star\star}_{\mathrm{I}} \;:=\; x^{\star\star}(\mathbf{C}^{\mathrm{BAL,I}}_{1:1}) - x^{\star\star}(\mathbf{C}^{\mathrm{UNBAL,I}}_{1:\bullet}) \;>\; 0 \tag{6}$$

as well as $d^{\star\star}_{\mathrm{I},8} \geq d^{\star\star}_{\mathrm{I},4} \geq d^{\star\star}_{\mathrm{I},2} > 0$. As in Section 2.2.1, these variables without any arguments denote averages over all n, B, and R.

$\mathrm{BAL}^{\mathrm{II}}_{1:1}$ vs. $\mathrm{UNBAL}^{\mathrm{II}}_{n:1}$. We now turn to the implications of focus-weighted utility on savings decisions in $\mathrm{BAL}^{\mathrm{II}}_{1:1}$ and $\mathrm{UNBAL}^{\mathrm{II}}_{n:1}$. Recall that in $\mathrm{UNBAL}^{\mathrm{II}}_{n:1}$, the negative consequences of saving are dispersed over several payment dates, while they are concentrated at a single, thus attention-grabbing, payment date ($t=8$) in $\mathrm{BAL}^{\mathrm{II}}_{1:1}$. The strictly increasing weighting function g captures this potential neglect of the

[11] The weighting function has to be steep enough to offset any potential effects that favor the dispersed payoff, including discounting, concavity of the per-period utility function, and the interest rate R.

dispersed payoffs in $\text{UNBAL}^{\text{II}}_{n:1}$. Date-8 utility ranges from $u_8(1)$ to $u_8(1+B)$ (for $x=1$ and $x=0$, respectively) in $\text{BAL}^{\text{II}}_{1:1}$. By contrast, it ranges only from $u_8(1)$ to $u_8(1+B/n)$ in $\text{UNBAL}^{\text{II}}_{n:1}$. Hence, focus-weighted utility assigns a lower weight to date-8 utility in $\text{UNBAL}^{\text{II}}_{n:1}$ than in $\text{BAL}^{\text{II}}_{1:1}$. In exchange for this downweighting of u_8, the preceding payment dates $t' = 7-(n-2), \ldots, 7$ receive a larger weight $g_{t'}$ in $\text{UNBAL}^{\text{II}}_{n:1}$ than in $\text{BAL}^{\text{II}}_{1:1}$. This is because for those dates, $\max u_{t'} - \min u_{t'} = 0$ in $\text{BAL}^{\text{II}}_{1:1}$, while it is positive in $\text{UNBAL}^{\text{II}}_{n:1}$. Just as before, if the slope of g is sufficiently steep, then the relatively large weight g_8, multiplied by u_8, plus the sum over the previous $g_{t'}\,u(1)$, in $\text{BAL}^{\text{II}}_{1:1}$ is greater than the sum of the multiple smaller weights $g_{t'}$, multiplied by $u_{t'}$, including $g_8\,u_8$, in $\text{UNBAL}^{\text{II}}_{n:1}$.

If such underweighting of the utility generated by early payments (up to payment date no. 8) is sufficiently strong in $\text{UNBAL}^{\text{II}}_{n:1}$, then focus-weighting reverses the prediction of the standard model—as stated in equation (3)—by predicting that individuals save more in $\text{UNBAL}^{\text{II}}_{n:1}$ than in $\text{BAL}^{\text{II}}_{1:1}$ (again, averages over all n, B, and R used in our experiment):

$$d^{\star\star}_{\text{II}} \;:=\; x^{\star\star}(\mathbf{C}^{\text{UNBAL, II}}_{\bullet:1}) - x^{\star\star}(\mathbf{C}^{\text{BAL, II}}_{1:1}) \;>\; 0, \tag{7}$$

and $d^{\star\star}_{\text{II},8} \geq d^{\star\star}_{\text{II},4} \geq d^{\star\star}_{\text{II},2} > 0$.

In the following, we compare savings decisions between $\text{BAL}^{\text{I}}_{1:1}$ and $\text{UNBAL}^{\text{I}}_{1:n}$ as well as between $\text{BAL}^{\text{II}}_{1:1}$ and $\text{UNBAL}^{\text{II}}_{n:1}$. We hypothesize that concentration bias is sufficiently strong and induces individuals to save more in $\text{BAL}^{\text{I}}_{1:1}$ than in $\text{UNBAL}^{\text{I}}_{1:\bullet}$, $d^{\star\star}_{\text{I}} > 0$, as well as more in $\text{UNBAL}^{\text{II}}_{\bullet:1}$ than in $\text{BAL}^{\text{II}}_{1:1}$, $d^{\star\star}_{\text{II}} > 0$. Both effects taken together yield the prediction regarding the aggregated concentration bias of $d^{\star\star} > 0$, with $d^{\star\star}$ being the average of $d^{\star\star}_{\text{I}}$ and $d^{\star\star}_{\text{II}}$.

Hypothesis 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, $d^{\star\star} > 0$ (in contrast to standard discounting).*

Define variables $d^{\star\star}_n$ to capture the differences in savings, averaged over the conditions I and II, for the different degrees of dispersion n: $d^{\star\star}_n := (d^{\star\star}_{\text{I},n} + d^{\star\star}_{\text{II},n})/2$ for $n \in \{2,4,8\}$ (as before, averaged over all B and R).

Hypothesis 2. *The effect described in Hypothesis 1 is the more pronounced, the more dispersed a payoff is, i.e., $d^{\star\star}_8 > d^{\star\star}_4 > d^{\star\star}_2 > 0$.*

2.2.3 Alternative Attention-Based Models. Closely related to Kőszegi and Szeidl's (2013) model of focusing, also Bushong, Rabin, and Schwartzstein (2016) and Bordalo, Gennaioli, and Shleifer (2013) assume that the decision maker attaches disproportionate attention to certain choice features. Both theories extend discounted utility, similar to the focusing model, with some weighting function g which determines the weights assigned to the different choice attributes.

Bushong, Rabin, and Schwartzstein's (2016) model of *relative thinking* is built on the same formal setup as the focusing model—see equations (4) and (5)—but the *reversed* central assumption on the slope of the focusing function g: Bushong, Rabin, and Schwartzstein (2016) assume that g is strictly monotonically decreasing.

In either approach, the range of a dimension is defined as the difference between the maximum and the minimum accessible utility and determines the weight the decision maker attaches to that dimension. But magnitudes of utility ranges have opposite effects for relative and for local thinkers. If a choice dimension is expanded in the utility range it offers, a focused thinker attaches more weight to a fixed outcome in that dimension, while a relative thinker attaches less weight to it. Accordingly, a relative thinker is predicted to go for options with dispersed advantages as these yield relatively good outcomes in many attributes. Focusing, however, induces choices of options which perform best in absolute terms in (possibly fewer) attributes. As a consequence, relative thinking predicts the opposite of a *concentration bias*, that is, a *dispersion bias* with respect to our experimental setup. We provide a detailed discussion of relative thinking in Appendix A.

Similar to the model on focusing by Kőszegi and Szeidl (2013), salience theory (Bordalo, Gennaioli, and Shleifer, 2013) states that individuals overemphasize attributes which stand out, and underrate less prominent, but possibly important aspects. Formally, salience theory is built on two main assumptions: ordering and diminishing sensitivity.

Ordering states that an attribute is the more salient the more it differs from the attribute's average level among all options in a given choice context. Accordingly, an individual focuses on the attributes where the alternatives are most different, neglecting the others. This can yield similar predictions as the focusing model and it might in particular imply a *bias toward concentration*.

On the other hand, diminishing sensitivity states that by uniformly increasing the value of an attribute for all goods, the salience of this attribute is reduced. Thereby, diminishing sensitivity mitigates ordering and can even result in relative thinking (Bordalo, Gennaioli, and Shleifer, 2013; Bushong, Rabin, and Schwartzstein, 2016). The relative strength of these two properties determines the predictions by salience theory for our experimental setup. Thus, Hypotheses 1 and 2 are consistent with those versions of the salience model where ordering is strong relative to diminishing sensitivity.

We provide a detailed discussion of the implications of our results for Bordalo, Gennaioli, and Shleifer (2013) in Appendix A. In particular, we show under which conditions salience theory predicts a concentration bias.

2.3 Results

Subjects made multiple allocation decisions in our experiment. In particular, subjects made several allocation decisions for BAL and UNBAL budget sets. This al-

Table 2. Testing Concentration Bias, $\hat{d}$, against Zero

Dependent variable	$\hat{d}$
Estimate	0.063***
	(0.011)
Observations	277
Subjects	185

Notes: Standard errors 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 in either Condition I or II, while the subjects in the second wave participated in both Condition I and Condition II (see Section 2.1.3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

lows us to calculate for each individual the average difference of money allocated to later payment dates between BAL and UNBAL budget sets. Denote by $\hat{x}$, $\hat{d}$, etc. the empirical counterparts of the variables introduced in Section 2.2, i.e., of $x^{\star/\star\star}$, $d^{\star/\star\star}$, etc. That is, $\hat{d}$ is the individual average of $\hat{d}_{\mathrm{I}} := \hat{x}(\mathbf{C}_{1:1}^{\mathrm{BAL,I}}) - \hat{x}(\mathbf{C}_{1:\bullet}^{\mathrm{UNBAL,I}})$ and $\hat{d}_{\mathrm{II}} := \hat{x}(\mathbf{C}_{\bullet:1}^{\mathrm{UNBAL,II}}) - \hat{x}(\mathbf{C}_{1:1}^{\mathrm{BAL,II}})$ across all decisions, i.e., across all n, B, and R.

2.3.1 Test of Hypothesis 1.

With this, we can report our first result.

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

Our first result supports Hypothesis 1. Subjects allocated $\hat{d} = 6.3$ percentage points (p.p.) more money to payoffs that were 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 2).[12] This result provides evidence for concentration bias as predicted by Kőszegi and Szeidl (2013).

A closer look at the specific comparisons between $\mathrm{BAL}_{1:1}^{\mathrm{I}}$ and $\mathrm{UNBAL}_{1:\bullet}^{\mathrm{I}}$ as well as $\mathrm{BAL}_{1:1}^{\mathrm{II}}$ and $\mathrm{UNBAL}_{\bullet:1}^{\mathrm{II}}$ substantiates our first finding. Subjects allocated on average more money to later payment dates in $\mathrm{BAL}_{1:1}^{\mathrm{I}}$ than in $\mathrm{UNBAL}_{1:\bullet}^{\mathrm{I}}$, $\hat{d}_{\mathrm{I}} = 5.7$ p.p. (= 9.12%). They also did so in $\mathrm{UNBAL}_{\bullet:1}^{\mathrm{II}}$ than in $\mathrm{BAL}_{1:1}^{\mathrm{II}}$, $\hat{d}_{\mathrm{II}} = 6.8$ p.p. (= 9.65%).[13] Both $\hat{d}_{\mathrm{I}}$ and $\hat{d}_{\mathrm{II}}$ are significantly larger than zero in a t-test (both $p < 0.01$). This demonstrates that concentration bias is driven by both present-biased as well as future-biased choices, consistent with the central assumption of the focusing model.

The results reported in Table 3 provide further support. Table 3 shows the frequencies of individual values of $\hat{d}_{\mathrm{I}}$ and $\hat{d}_{\mathrm{II}}$ being smaller, larger, or equal to zero. A sign-rank test shows that the values of both $\hat{d}_{\mathrm{I}}$ and $\hat{d}_{\mathrm{II}}$ are not distributed symmetrically around zero. In both cases, the largest fraction of subjects has positive $\hat{d}_{\mathrm{I}}$ and $\hat{d}_{\mathrm{II}}$ values, and there are more than twice as many subjects with positive than with negative $\hat{d}_{\mathrm{I}}$ and $\hat{d}_{\mathrm{II}}$ values, respectively.

[12] This finding is substantiated with a sign-rank test ($p < 0.001$).

[13] $\hat{x}(\mathbf{C}_{1:1}^{\mathrm{BAL,I}}) = 68.3\%$, $\hat{x}(\mathbf{C}_{1:\bullet}^{\mathrm{UNBAL,I}}) = 62.5\%$, $\hat{x}(\mathbf{C}_{\bullet:1}^{\mathrm{UNBAL,II}}) = 77.3\%$, and $\hat{x}(\mathbf{C}_{1:1}^{\mathrm{BAL,II}}) = 70.5\%$.

Table 3. Frequencies of the Two Measures of Concentration Bias, $\hat{d}_{\mathrm{I}}$ and $\hat{d}_{\mathrm{II}}$, Being Positive, Zero, or Negative

Difference	(1) $\hat{d}_{\mathrm{I}}$	(2) $\hat{d}_{\mathrm{II}}$
Positive	63 (45%)	59 (43%)
Zero	47 (34%)	51 (37%)
Negative	29 (21%)	28 (20%)
N	139	138

At the same time, there are seizable fractions of subjects whose $\hat{d}_{\mathrm{I}}$ and/or $\hat{d}_{\mathrm{II}}$ values are equal to zero. Let us investigate these subjects' behavior in greater detail. Out of 47 subjects with $\hat{d}_{\mathrm{I}} = 0$, four subjects chose $\hat{x}(\mathbf{C}_{1:1}^{\mathrm{BAL,I}}) = 0$ so that there was no "room" for them to save even less in the UNBAL condition, as our Hypothesis 1 predicts. However, for the remaining 43 subjects, there was "room" to save less in the unbalanced budget sets, i.e., to choose $\hat{x}(\mathbf{C}_{1:\bullet}^{\mathrm{UNBAL,I}}) < \hat{x}(\mathbf{C}_{1:1}^{\mathrm{BAL,I}})$ in line with Hypothesis 1—but they did not do so. Thus, for these 43 subjects, concentration bias may not have mattered. [14]

Regarding the second group, the 51 subjects with $\hat{d}_{\mathrm{II}} = 0$, it turns out that 45 subjects chose $\hat{x}(\mathbf{C}_{1:1}^{\mathrm{BAL,II}}) = 1$. This means that they were already so patient in the $\mathrm{BAL}_{1:1}^{\mathrm{II}}$ condition that their behavior may be confined by a ceiling effect: our task simply did not allow them to choose $\hat{x}(\mathbf{C}_{\bullet:1}^{\mathrm{UNBAL,II}}) > \hat{x}(\mathbf{C}_{1:1}^{\mathrm{BAL,II}})$, as concentration bias would have predicted. Thus, it might be that some of these 45 subjects would have shown an effect if they had been given "room" to do so.

2.3.2 Test of Hypothesis 2. Let us now turn to the question whether the degree of dispersion influences subjects' choices, i.e., to testing Hypothesis 2.

Result 2. *Our measure of concentration bias is the greater, the more dispersed payments in the UNBAL condition are, i.e.,* $\hat{d}_8 > \hat{d}_4 > \hat{d}_2 > 0$.

Our second result provides evidence in support of Hypothesis 2. We find that the degree of concentration bias that subjects exhibit 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; it is $\hat{d}_4 = 6.56$ p.p. for 4 payment dates and $\hat{d}_2 = 3.67$ p.p. for 2 payment dates. All three treatment effects are significantly larger than zero according to both t-tests and sign-rank tests ($p < 0.001$ for $\hat{d}_8$ and $\hat{d}_4$; $p < 0.05$ for $\hat{d}_2$ in both tests). Moreover, concentration bias in the case that payoffs were dispersed over 4 or 8 payment dates is significantly greater than when payoffs were 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

[14] However, since under exponential discounting subjects with a positive discount rate were better off in UNBAL trials, we cannot rule out that concentration bias moved them to $\hat{d}_{\mathrm{I}} = 0$ rather than concentration bias had no affect on them.

Table 4. Testing Difference in Decision Time, $\widehat{\text{dtime}}$, against Zero

Dependent variable	$\widehat{\text{dtime}}$
Estimate	-1.34^{***}
	(0.37)
Observations	277
Subjects	185

Notes: Standard errors 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 in either Condition I or II, while the subjects in the second wave participated in both Condition I and Condition II (see Section 2.1.3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

concentration bias for 8 payment dates is significantly larger than for 2 payment dates ($p < 0.01$) but not significantly larger than for 4 payment dates ($p = 0.237$).

2.3.3 Heterogeneity. The focusing model can be considered as a formalization of a rule of thumb which people use. The use of a dispersion-averse heuristic would lead to an additional hypothesis regarding the decision time of subjects. In BAL trials, subjects carefully check whether they think it is worth allocating more money to later payment dates, since the gains from being patient are directly assessable. In UNBAL, on the other hand, they may employ a quick (and frugal) heuristic that avoids dispersed payoffs. According to this rationale, we would expect that individuals take more time for BAL than UNBAL trials. Table 4 shows that this is the case. Subjects take on average significantly more time to decide in BAL than in UNBAL trials.

However, we find no evidence that this difference in decision time explains our findings on concentration bias in intertemporal choice. Column (1) of Table 5 shows that the individual difference in decision times between BAL and UNBAL trials is uncorrelated with the individual difference in allocation decisions between BAL and UNBAL trials. This finding suggests that concentration bias in intertemporal choice is not a consequence of spending less time in UNBAL trials than in BAL trials.

Table 3 shows that there is substantial heterogeneity in the degree of concentration bias between subjects. It is conceivable that this heterogeneity is related to heterogeneity in cognitive abilities and/or impulsivity. We therefore try to measure subjects' abilities that might be related to such effects by assessing math skills via an incentivized mental-arithmetic task. In this task, subjects were given five minutes time to calculate as many sums as they could of decimal numbers. These sums were of a similar kind as the monetary payments presented to subjects in the main experiment. The median subject calculated six sums correctly. Moreover, individuals who are less able to control their impulses might be more prone to concentration bias. We use the CRT (Frederick, 2005) as a measure of impulsiveness.

To test whether math ability and impulsivity affect concentration bias, we regress our measure of concentration bias, $\hat{d}$, on standardized measures of subjects' math ability and their CRT score. As evident from Table 5, we find that both these measures negatively affect concentration bias. That means, 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. Overall, we take this as suggestive evidence that cognitive ability plays a moderating role for concentration bias.

3 Control Experiment

The previous section has provided evidence for concentration bias in intertemporal choice that is at odds with exponential and hyperbolic discounting, while being predicted by the focusing theory of Kőszegi and Szeidl (2013) as well as variants of salience theory (Bordalo, Gennaioli, and Shleifer, 2012, 2013). The findings of the main experiment could also be explained by two different accounts of cognitive limitations: left-digit bias and goal-driven attention.

According to left-digit bias (Lacetera, Pope, and Sydnor, 2012), individuals pay relatively too much attention to the left-most digit of strings of numbers—in their study, prices for used cars. Applied to our setting, left-digit predicts that subjects arrive at systematically downward-biased estimates of the sum of a dispersed payoff. Instead of calculating the sum of, for instance, $1.58 + 1.58 + 1.58 + 1.58 + 1.58 + 1.58 + 1.58 + 1.58$ (or 8×1.58), individuals may use a cognitive shortcut that induces individuals to focus on the left digit of the individual terms of the

Table 5. Regression of the Measure of Concentration Bias, $\hat{d}$, on Decision Time, a Measure of Mathematical Ability, and CRT Scores

	(1)	(2)	(3)
Dependent variable	$\hat{d}$	$\hat{d}$	$\hat{d}$
$\widehat{\text{dtime}}$	0.000		
	(0.001)		
Standardized CRT score		−0.016	
		(0.011)	
Standardized Math score			−0.021*
			(0.011)
Constant	0.062***	0.064***	0.063***
	(0.011)	(0.011)	(0.011)
Observations	277	277	277
Subjects	185	185	185
R^2	0.000	0.009	0.013

Notes: Standard errors 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 in either Condition I or II, while the subjects in the second wave participated in both Condition I and Condition II (see Section 2.1.3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

sum.Individuals' rough estimate of the sum is then closer to 8—that is, 8×1 (the left digit)—than it should be. This example generalizes to all dispersed payoffs: left-digit bias leads individuals to underestimate dispersed payoffs vis-à-vis concentrated payoffs.

According to notions of goal-driven attention, individuals do not engage in carefully calculating the sum of a dispersed payoff because of "rational" contemplations. Once they are confronted with a dispersed payoff, for instance, $1.58 + 1.58 + 1.58 + 1.58 + 1.58 + 1.58 + 1.58 + 1.58$, they are confronted with a cognitively costly task. Individuals may believe that these costs may exceed the benefits from precisely knowing the sum of a dispersed payoff. In the case of risk aversion, goal-driven attentive individuals undervalue dispersed payoffs vis-à-vis concentrated payoffs.

To investigate the potential effects of left-digit bias and goal-driven attention, we made use of a control experiment in which all dispersed payoffs were "dispersed within a day" instead of being dispersed over different payment dates. Recall that the last bank transfer of a dispersed payoff in $\text{UNBAL}^{\text{I}}_{1:n}$ and $\text{UNBAL}^{\text{II}}_{n:1}$ is always made at the same date as the respective concentrated payoff in $\text{BAL}^{\text{I}}_{1:1}$ and $\text{BAL}^{\text{II}}_{1:1}$. In our "dispersed within a day" control experiment, we therefore mirrored the combined features of the UNBAL and BAL conditions: we made the dispersed payoffs de facto identical to the concentrated ones, by scheduling all "dispersed" payoffs on the date of the concentrated payoff. In other words, the "dispersed within a day" payoffs are completely equivalent to the concentrated payoffs except the difference in the display: subjects saw 2, 4, or 8 relatively small monetary amounts that they would have to sum up to calculate the total earnings that they would receive at that date. Figure 5 displays a screenshot of the graphical representation that was shown to subjects who participated in this control experiment (lower panel) in relation to the graphical representation used in the main experiment (upper panel).

Subjects in the control experiment made the same amount of allocation decisions as subjects in the main experiment, with each "dispersed within a day" decision being analogous to one "dispersed over time" decision in the main experiment. Thus, we can calculated the same average difference of money allocated to "concentrated"—or not dispersed—payoffs, i.e., $\hat{d}$, for subjects in the control experiment as we did for subjects of the main experiment. While $\hat{d}$ measures concentration bias in the main experiment, it measures effects resulting from left-digit bias as well as goal-driven attention in the control experiment.

In case our estimated measure of $\hat{d}$ is statistically larger in the main than in the control experiment, this would imply that the evidence for concentration bias in the main experiment cannot be explained by computational complexity. Recall that according to discounted utility, the dispersed-over-time payoffs are than the concentrated/dispersed-within-a-day payoffs. This means that a combination of mathematical error and discounting would imply that the effect is particularly strong for the dispersed-within-a-day payoffs—stronger than for the dispersed-over-time payoffs.

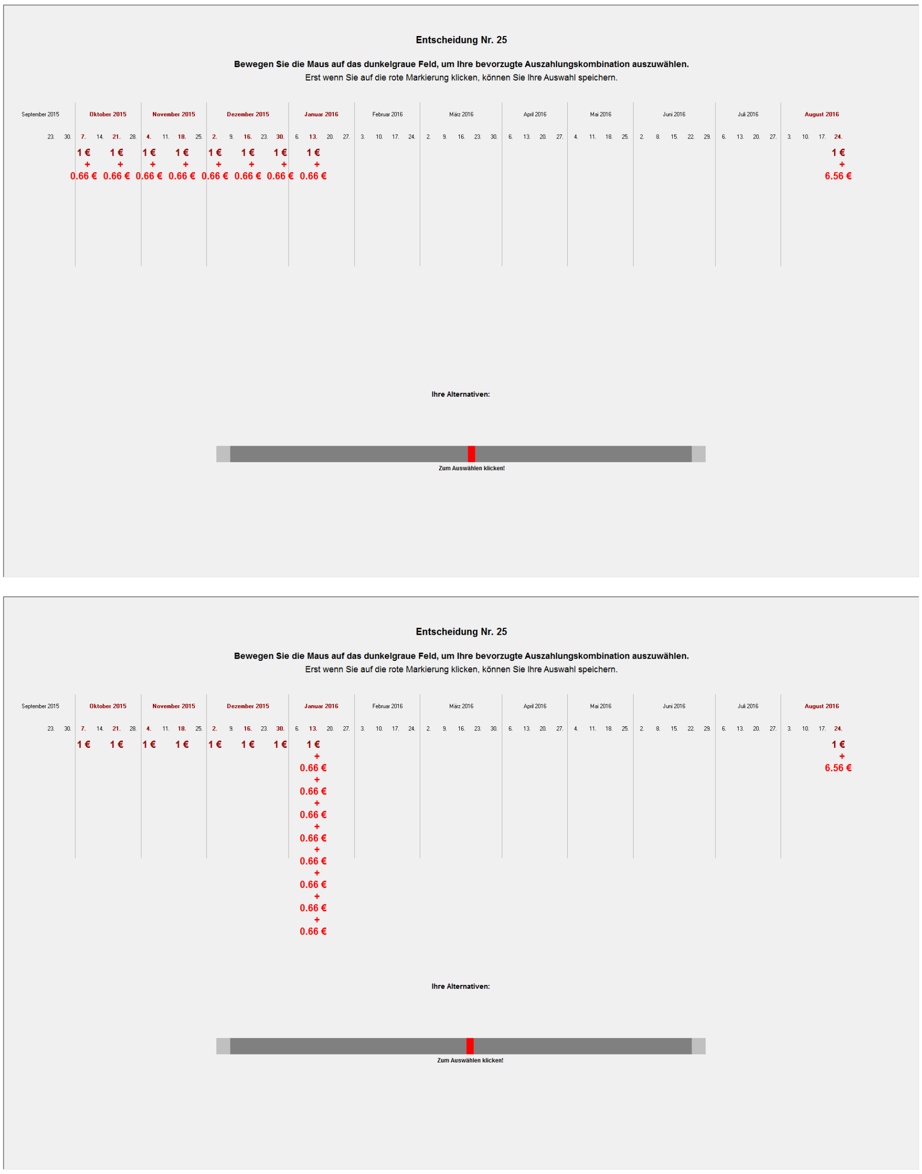


Figure 5. Budget Sets: Screenshots of an $\text{UNBAL}^{\text{II}}_{8:1}$ Condition in the Main (Top) and in the Respective Condition in the Control (Bottom) Experiment

We compare $\hat{d}$ between our main and control experiments in an OLS regression. This comparison is between subjects and involves 374 subjects; of these, 185 participated in the main experiment and 189 participated in the control experiment.[15] To compare the main experiment with the control experiment, we regress $\hat{d}$ on

[15] Except for the first five sessions, the main and control experiments were conducted during the same sessions, and subjects were assigned to the main or control experiment randomly within-session. During the first two sessions, only the main experiment was run; this was followed by three sessions in which only the control experiment was run.

Table 6. Difference-in-Difference Analysis of Concentration Bias, $\hat{d}$, in the Main Experiment (Dispersed over Time) vis-à-vis the Control Experiment (Dispersed within a Day)

Dependent variable	(1) $\hat{d}$	(2) $\hat{d}$
Main Experiment Dummy	0.036***	0.038***
	(0.013)	(0.013)
Decision Time		0.000
		(0.000)
Standardized CRT score		−0.007
		(0.007)
Standardized Math score		−0.011*
		0.006
Constant (= Effect in Control Experiment)	0.026***	0.025***
	(0.006)	(0.007)
Observations	562	562
Subjects	374	374
R^2	0.016	0.029

Notes: Standard errors 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 in either Condition I or II, while the subjects in the second wave participated in both Condition I and Condition II (see Section 2.1.3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

a dummy variable that takes on the value 1 for all subjects who participated in the ("dispersed over time") main experiment instead of the main experiment.[16] The coefficient on the constant measures the behavioral effect of splitting up a payoff into the sum of multiple small amounts in the control experiment, that is, the payoff is dispersed "within a day." The coefficient on the main-experiment dummy measures how much larger (or smaller) the effect of splitting up the payoff is in the main experiment, that is, the payoff is dispersed over time.

As Columns (1) and (2) of Table 6 show, we find that merely presenting a larger payoff as the sum of multiple small payoffs, without any change in the timing of the payoffs, makes subjects choose the concentrated more frequently: the coefficient on the constant in the control experiment is positive and significantly greater than zero ($p < 0.01$). On average, subjects allocate 2.6 p.p. more of their budget to concentrated than to dispersed payoffs in the control experiment. This indicates that splitting up payoffs in itself has an effect on subjects' behavior. We find that this effect is in the direction predicted by left-digit bias as well as goal-driven attention.

However, we also find that our measure of concentration bias is greater in the main experiment: the coefficient on the main-experiment dummy (0.036) is significantly greater than zero ($p < 0.01$). It is larger than the coefficient on the constant,

[16] We have up to two values for the dependent variable per subject, depending on whether a subject participated in both the $\text{BAL}^{\text{I}}_{1:1}$ and $\text{UNBAL}^{\text{I}}_{1:n}$ as well as the $\text{UNBAL}^{\text{II}}_{n:1}$ and $\text{BAL}^{\text{II}}_{1:1}$ conditions or only one of the two. Consequently, we cluster standard errors on the subject level.

suggesting that the effect in the main experiment is at least twice as strong as in the control experiment. On average, subjects allocated 6.3 p.p. more of their budget to concentrated payoffs than to dispersed payoffs in the main experiment (see Table 2). Importantly, this is the case even though discounting works against the effect in the main experiment: discounting makes the dispersed-over-time payoffs more attractive than the dispersed-within-a-day payoffs—which are consequentially identical to the concentrated payoffs—in the control experiment. This provides evidence that concentration bias affects intertemporal choice beyond what could be explained by left-digit bias and goal-driven attention.

Merely displaying a larger payoff as the sum of multiple smaller amounts thus cannot explain the concentration bias that we observed in our main experiment. This suggests that payments at different points in time indeed reflect different choice attributes, while amounts displayed separately but received on the same day do not. While Kőszegi and Szeidl (2013) treat the attribute representation of a set of options as exogenous (see p. 69 of their paper), our study helps to clarify which features of an option can be assumed to represent different attributes and which cannot.

4 Discussion

We have above presented evidence for a bias toward concentrated payoffs in intertemporal choice that is at odds with the aggregation of intertemporal consequences as it is modeled by standard discounted utility, i.e., by exponential and hyperbolic discounting. We have argued and presented evidence that, instead, behavior is the consequence of cognitive processes akin to the “focus weighting” proposed by Kőszegi and Szeidl (2013).

In this section, we discuss potential alternative explanations of the observed behavior and their plausibility. The first potential alternative explanation involves uncertainty about receiving the promised payments and associated time costs for inspecting that the promised bank transfers have actually been made. The second potential alternative would be that utility from money is convex.

4.1 Uncertainty and Transaction Costs

Any alternative explanation based on uncertainty and transactions costs would have to predict that individuals devalue dispersed payoffs relative to concentrated payoffs. Our experimental design allows us to make sure that such an asymmetric exposition of the different payoff types to uncertainty and transactions costs is implausible for our experimental design: we kept the number of payment dates constant between all intertemporal choices subjects were engaged in; We kept constant that all payments dates took place at least several days after subjects made the intertemporal choices; We kept the type of payment—i.e., bank transfers—constant between all payment dates. Neither dispersed nor concentrated payoffs where more or less convenient due to differences in the number of payments dates, differences in taking place in the presence or the future, nor differences in the type of payment.

Moreover, we minimized uncertainty associated with all payments. To this end, each subject received two individualized e-mail messages after the experiment—one a few days after participation and the second after the last money transfer had been made. Before making any decisions in the experiment, the written instructions informed subjects in detail about these two messages.

Both e-mail messages included a complete listing of all payments that subjects were to receive from the experiment. Hence, subjects knew in advance when exactly they would have to inspect their bank statements to check that they had received the promised amount. As already mentioned, the first of these two e-mail messages was sent out a few days after subjects' participation in the respective session. This was done so that we could ask subjects to check that the bank account number (IBAN) used for the bank transfers was correct. We also told them to contact us immediately in case anything was incorrect.

The second message was sent out shortly after the last bank transfer to the respective subject had been completed. This second message again stated all payment dates and asked subjects to contact us immediately in case they were missing any of the payments. Not a single subject replied to the first or second message. Only at irregular intervals throughout the 49-week-long payment phase did we receive a few messages from a handful of subjects who had changed their bank accounts during the payment phase. For those who contacted us, we immediately adjusted the predated bank transfers. Some subjects who changed their bank account failed to contact us. In this case, we contacted them. In the end, all subjects responded to our inquiries so that *all* promised payments could be made.

Since subjects were informed in the instructions that they would be sent these two e-mail messages, they knew that checking for having received the promised payments would be as simple as possible. Most importantly, however, inspection for the UNBAL conditions was just as easy as for the BAL conditions, because the number of payment dates was identical across all trials in all conditions: one would always have to search for payments at exactly nine different dates. Therefore, uncertainty about receiving and transaction costs regarding future payments do not affect dispersed and concentrated payoffs asymmetrically in our main experiment. Hence, it cannot explain the concentration bias that we observe.

4.2 Curvature of Utility from Money

Recall that our measure of concentration bias is based on the comparison of subjects' choices from balanced budget sets (that involve only concentrated payoffs) with their choices from unbalanced budget sets (that involve dispersed payoffs, which sum up to the same amount as but are paid earlier than the respective payoffs in the balanced budget sets). We find that subjects allocate more money to the concentrated payoffs in the unbalanced than in the balanced budget sets—which we call concentration bias. One might argue that this relative preference for concentrated payoffs could be explained by the per-period utility function over money being convex. This is because the concentrated payoffs involve a large payment in comparison with the payments that the dispersed payoffs consist of.

Obtaining evidence for the shape of utility over money is nontrivial because it requires that at least two monetary amounts be compared with each other without the one clearly dominating the other. Thus, estimates of the curvature of the utility function over money can be obtained in two ways—both entailing particular theoretical assumptions: the monetary amounts must be paid in different states of the world, i.e., comprise a lottery, or they have to be paid at different points in time.[17]

Andersen et al. (2008) advocate the former approach and argue that when estimating time preference parameters, one should control for the curvature of the utility function through a measure of the curvature that is based on observed choices under risk. Their study and numerous other studies on risk attitudes consistently reveal that the vast majority of subjects is risk-averse even over small stakes. Hence, for the vast majority of subjects, utility over money is concave according to this methodology (at least in the absence of probability weighting). However, others, most notably Andreoni and Sprenger (2012), have argued that the degree of curvature measured via risky choices probably overstates the degree of curvature effective in intertemporal choices (which could be due to probability weighting also contributing to risk aversion). Nevertheless, also Andreoni and Sprenger (2012) find that utility is concave, albeit close to linear.

Given this unambiguous evidence from previous studies, it is implausible that our subjects exhibit convex utility over money.

5 Conclusion

This paper is the first to provide causal evidence for concentration bias in intertemporal choice. Building on the "convex budget sets" method of Andreoni and Sprenger (2012), we designed a novel choice task that implements different types of intertemporal budget constraints. More specifically, both earlier and later benefits in this task take on the form of either concentrated (one-time) payoffs or payoffs that are dispersed over several payment dates. We used this choice task in a laboratory experiment to test how spreading payoffs over time influences individuals' intertemporal decisions. We find that the payoffs' degree of dispersion influences choices in a way that is incompatible with discounted utility but in line with concentration bias, as the "focusing model" by Kőszegi and Szeidl (2013) predicts. Our findings are rele-

[17] As a matter of fact, the latter was the motivation behind Samuelson (1937): "Under the following four assumptions, it is believed possible to arrive theoretically at a precise measure of the marginal utility of *money income* …" (p. 155; emphasis in the original).

vant not only for positive economics, i.e., for understanding and forecasting people's behavior, but also for normative economics, as we argue below.

The model most widely used for analyzing intertemporal decisions is discounted utility in combination with exponential discounting. However, people's decisions often seem to contradict exponential discounting. One example is low gym attendance (DellaVigna and Malmendier, 2006). A different example—with huge monetary stakes—is the "annuity puzzle" (see, e.g., Yaari, 1965; Warner and Pleeter, 2001; Davidoff, Brown, and Diamond, 2005; Benartzi, Previtero, and Thaler, 2011). It describes the phenomenon that many people choose an earlier lump-sum payment over a future rent that is paid periodically (the annuity) even when the rent has a substantially higher expected present value. In fact, many other decisions from everyday life are similar in that some of the available options are characterized by payoffs or costs that are dispersed over time—such as the benefits of not smoking or the costs of purchasing a new smartphone when choosing a payment plan.

Unfortunately, based on empirical data from the field, it is hard or even impossible to discriminate between competing explanations of observed behavior. More concretely, the "annuity puzzle" (Benartzi, Previtero, and Thaler, 2011) could be the product of Kőszegi–Szeidl-type focusing. However, other factors, such as uninsured medical expenses, bequest motives, and adverse selection, may also explain the surprisingly low observed degree of annuitization (see the discussions in Modigliani, 1986; Davidoff, Brown, and Diamond, 2005; Benartzi, Previtero, and Thaler, 2011).

By providing a controlled environment in which particular motives are ruled out or held constant, our lab experiment allows for establishing that the dispersion of consequences indeed causally affects discounting. We find that subjects exhibit concentration bias. Thus, they violate predictions of discounted-utility models, while their decisions are compatible with the focusing model by Kőszegi and Szeidl (2013). Via a control treatment, we show that merely displaying a larger payoff as the sum of multiple smaller amounts cannot explain the concentration bias that we observed in our main experiment. This suggests that payments at different points in time indeed reflect different choice attributes, while amounts displayed separately but received on the same day do not. Thus, our study helps to clarify which features of an option can be assumed to represent different attributes in the sense of Kőszegi and Szeidl and which cannot.

Our study contributes to the literature on intertemporal choice in two important ways: First, our series of experiments was designed to permit tests which directly inform how concentration bias effects should be modeled. In particular, the difference-in-difference analysis of the main and the control experiment allows us to distinguish between explanations for concentration bias that build on stimulus-driven attention and on goal-driven attention. Our results favor the stimulus-driven approach, in particular the model by Kőszegi and Szeidl (2013). Second, our experiment helps explain why in recent experiments, the observed degree of present bias and the incidence of time-inconsistent behavior are fairly low (Andreoni and Sprenger, 2012;

Augenblick, Niederle, and Sprenger, 2015), while they are found to be much more severe in the analysis of field data (e.g., Warner and Pleeter, 2001; Davidoff, Brown, and Diamond, 2005; DellaVigna and Malmendier, 2006). Our study does so by identifying the lack of dispersed payoffs in previous lab experiments as a plausible source of this discrepancy.

We are aware of one previous study, by Attema et al. (2016), that used dispersed payoffs in the form of multiple bank transfers to remunerate subjects. While Attema et al. propose a highly elegant method of "measuring discounting without measuring utility," their design was not intended to and is not capable of testing for concentration bias. Crucially, their method relies on measuring indifference between payment streams that consist of dispersed payoffs.[18] However, our evidence implies that the discount rate elicited by Attema et al.'s method is in fact a quantity jointly determined by the "genuine" discount rate and individuals' degree of concentration bias. As a consequence, as soon as an individual's discount rate does not equal zero, it is likely that the estimates obtained by Attema et al.'s method will be sensitive to the exact payment streams employed, i.e., to the size of the payoffs, their degree of dispersion, and their exact timing.

Outside the laboratory, concentration bias is essential information, for instance, regarding the assessment of the effectiveness of policy measures. It is conceivable that taxes on annuities are perceived as less severe than taxes on lump-sum payments or on current income. This illustrates that understanding how concentration bias affects people's intertemporal choices, in and outside the lab, is just as important for normative economics as it is for positive economics.

[18] The basic idea of their method is intriguingly simple: If an individual is indifferent between, say, \$10 today, and \$10 in one year plus an additional \$10 in two years, then we can measure discounting without having to take the utility function into account. Under exponential discounting with an annual discount factor δ, this indifference translates to $u(\$10) = \delta\, u(\$10) + \delta^2\, u(\$10)$, so that $u(\$10)$ cancels out and δ can be readily calculated as the solution to $1 = \delta + \delta^2$.

Appendix A Models of Attention-Based Decision Making

In this section, we apply the attention-based models by Bordalo, Gennaioli, and Shleifer (2013) and Bushong, Rabin, and Schwartzstein (2016) to an intertemporal setup and discuss their predictions with respect to our experimental setup in detail.

Relative thinking as proposed by Bushong, Rabin, and Schwartzstein (2016) shares much of its formalism with the focusing model; however, as the authors put it, "it sits in an interesting and uncomfortable relationship to their model: we say the range in a dimension has the exact opposite effect as it does in their model" (Bushong, Rabin, and Schwartzstein, 2016, p. 6). That is, while a greater range in a particular attribute means that a greater weight is assigned to that attribute according to the focusing model, it receives a lower weight according to relative thinking.

Salience theory (Bordalo, Gennaioli, and Shleifer, 2013), in contrast, enriches the focusing model by the additional assumption of diminishing sensitivity according to which the difference in attributes attracts the less attention the larger the absolute values of the attributes are. Via the so-called "ordering" property, a larger range can increase the weight that a particular attribute receives. Simultaneously, their model features the so-called "diminishing sensitivity" property which resembles relative thinking in that it works in the opposite direction. In salience theory, it is the relative strength of the two properties that determines the theoretical predictions for our experimental setup.

A.1 Relative Thinking

As already stated above, in general, Bushong, Rabin, and Schwartzstein (2016) predict effects in the opposite direction of the focusing model, regarding choices between goods. However, in their example of intertemporal choice, Bushong, Rabin, and Schwartzstein introduce a second way in which their model differs from the focusing model, which is how a decision maker handles future time periods.

In their dynamic decision problem, the agent decides at time t about consumption at point t and how much to save for future consumption. Importantly, in Bushong, Rabin, and Schwartzstein's view, a decision maker does not regard each future period as an independent dimension but "integrates" or "combines" all future periods into one dimension (p. 29). Hence, the decision problem is reduced to two dimensions. The range of the future dimension is equal to the difference in cumulative consumption utility under optimal spending (i.e., consumption smoothing, given the assumption of concave per-period utility) if everything is saved and if nothing is saved. This has the consequence that the range is large relative to the present dimension. Given that a larger range is associated with a lower weight, present bias is predicted.

The question that arises is what "integration" assumption is reasonable for our experimental setup. Several ways how such integration in conjunction with relative thinking could apply to our experimental setup seem reasonable to us.

First, one could argue that no integration occurs at all. In Bushong, Rabin, and Schwartzstein's (2016) model, the decision maker can revise her plans in every period. Both this malleability of the future and its inherent uncertainty may make present and future qualitatively different such that people might indeed sometimes combine all future periods into one all-encompassing dimension. While we find it plausible that in many situations—in particular, for choices over the far, highly uncertain future—decision makers lump a large number of periods into one general dimension, we do not believe that this is a plausible assumption for our experiment. In our experiment, there is only one choice which determines all payments for all nine dates which are all in the foreseeable future. Our display of the options on the screen, the fact that we included a calendar/time line, and the fact that subjects saw various options over different conditions, makes it reasonable that they perceived each payment as a separate attribute, just like Kőszegi and Szeidl (2013) assume. In that case, the only difference between the two models is the opposite range effect and, therefore, a *dispersion bias* is predicted by "relative thinking."

Second, if we take Bushong, Rabin, and Schwartzstein's (2016) assumption that *all future* periods are combined into one dimension literally, the model yields no difference to standard discounted utility: Since *all* payments lie in the future and no payment is received immediately, all periods are bundled into one dimension and are equally weighted, so that there is no weighting beyond standard discounting.

Third, softening the notion of how far the presence extends such that it includes the first period where a payment is received, $t = 1$, relative thinking could explain the result of "present bias" as revealed by the comparison between $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ and $\mathrm{UNBAL}^{\mathrm{I}}_{1:n}$. However, it could not explain the result of "future-biased behavior" as evident from the second comparison between $\mathrm{BAL}^{\mathrm{II}}_{1:1}$ and $\mathrm{UNBAL}^{\mathrm{II}}_{n:1}$. This is because in $\mathrm{UNBAL}^{\mathrm{II}}_{n:1}$, the relative thinker would bundle all payment dates of the dispersed payoff, except the first one, with the concentrated payoff on the final date. This would imply an equal weighting of all dates, except the first date. This first date would be, relatively, over-weighted and, if anything, a *dispersion bias* would be predicted by relative thinking.

A.2 Salience

In this section we introduce the salience model by Bordalo, Gennaioli, and Shleifer (2013) and apply it to our intertemporal setup. In Section A.2.3, we assume a particular class of salience functions and outline under which conditions salience theory predicts a concentration bias.

A.2.1 Basics of Salience Theory. Salience theory (Bordalo, Gennaioli, and Shleifer, 2012, 2013) represents an alternative behavioral model according to which the most distinctive features of the available alternatives receive a particularly large share of attention and are therefore over-weighted. More precisely, a particular attribute out of all attributes of an alternative becomes the more salient, the more it differs from that attribute's average level over all available alternatives.

Formally, alternatives are assumed to be uniquely characterized by the values they take in $T \geq 1$ attributes (or, "dimensions"). Utility is assumed to be additively separable in attributes, and salience attaches a decision weight to each attribute of each good which indicates how salient the respective attribute is for that good. Suppose an agent chooses one alternative from some finite choice set $\mathbf{C}$. Let t index the T different attributes, and let k index the K available alternatives. Let $u_t(\cdot)$ denote the function which assigns utility to values in dimension t. Denote by a_t^k the level of attribute t of good k and define $u_t^k := u_t(a_t^k)$ as the utility that dimension t of good k yields. Let $\overline{u}_t$ be the average utility level, across all K goods, of dimension t. The salience of each dimension of good k is determined by a symmetric and continuous salience function $\sigma(\cdot,\cdot)$ that satisfies the following two properties:

1. *Ordering.* Let $\mu := \text{sgn}(u_t^k - \overline{u}_t)$. Then for any $\epsilon, \epsilon' \geq 0$ with $\epsilon + \epsilon' > 0$, it holds that

$$\sigma(u_t^k + \mu\,\epsilon, \overline{u}_t - \mu\,\epsilon') \;>\; \sigma(u_t^k, \overline{u}_t). \tag{A.1}$$

2. *Diminishing sensitivity.* For any $u_t^k, \overline{u}_t \geq 0$ and all $\epsilon > 0$, it holds that

$$\sigma(u_t^k + \epsilon, \overline{u}_t + \epsilon) \;<\; \sigma(u_t^k, \overline{u}_t). \tag{A.2}$$

Following the smooth salience characterization proposed in Bordalo, Gennaioli, and Shleifer (2012, p. 1255), each dimension t of good k receives weight $\Delta^{-\sigma(u_t^k,\overline{u}_t)}$, where $\Delta \in (0,1]$ is a constant that captures an agent's susceptibility to salience. $\Delta = 1$ gives rise to a rational decision maker, and the smaller Δ, the stronger is the salience bias. We call an agent with $\Delta < 1$ a salient thinker.

A.2.2 Applying Salience Theory to Intertemporal Choice. In order to apply salience theory to intertemporal decisions, we assume that (i) each period at which a payment is made represents a separate choice dimension and (ii) agents maximize lifetime utility as it is given by the salience-weighted sum of all periods' instantaneous utilities.

Formally, let $t = 1, \ldots, T$ index periods in which payments occur. Denote by $\mathbf{C}$ the set of the available alternatives at time 0, i.e., when the decision maker makes her choice. As before, we index the K elements of $\mathbf{C}$ by k. We can summarize alternative k as the T-dimensional payment vector $\mathbf{a}^k$, such that an attribute a_t^k is the payment that option k offers in period t.

To keep the notation consistent with that used in the main text for discounted utility and for the focusing model, define $x := (k-1)/100$, i.e., $x \in \mathbf{X}$ with $\mathbf{X} = \{0, 1/100, 2/100, \ldots, 1\}$; we can then use $\mathbf{c}(x)$ instead of $\mathbf{a}^k$. The salience weight attached to the utility obtained from the payment in period t that results from choosing x is then given by $\Delta^{-\sigma(u_t(c_t(x)),\overline{u}_t)}$. Here, $u_t(c_t(x))$ is the instantaneous utility derived receiving payment $c_t(x)$ in period t, evaluated from the perspective of—i.e., discounted to—period 0.[19] The value $\overline{u}_t(\mathbf{C}) := \frac{1}{101}\sum_{x\in\mathbf{X}} u_t(c_t(x))$ is the associated average across all alternatives included in the choice set $\mathbf{C}$. A salience thinker's choice-relevant intertemporal utility of alternative x when facing budget set $\mathbf{C}$ is then given by

$$U^S(\mathbf{c}(x), \mathbf{C}) \;:=\; \sum_{t=1}^{T} \Delta^{-\sigma(u_t(c_t(x)),\overline{u}_t(\mathbf{C}))} u_t(c_t(x)). \tag{A.3}$$

These two assumptions are similar to those made by Kőszegi and Szeidl (2013), where also each period is considered as a separate attribute, and the objective function is given by the—focus-weighted—sum of all per-period utilities.[20]

We apply salience theory to our experimental setup as follows. For expositional simplicity, we assume in the remainder of this section that utility in money is linear and that the decision maker discounts future payoffs via $D(t) = 1$ for all t. That is, we assume that $u_t(c_t(x)) = c_t(x)$. Our following arguments hinge on neither of these assumptions.

Recall that in our experiment, each stream of payoffs (earnings sequence) is uniquely determined by the choice of x, where x is the fraction of the endowment B to be saved for the later payment dates. Option x yields payments $c_t(x)$ for $1 \leq t \leq 9$. The average payment in period t equals

$$\overline{c}_t(\mathbf{C}) \;=\; \frac{1}{101}\sum_{x\in\mathbf{X}} c_t(x). \tag{A.4}$$

Thus, in our experiment, a salient thinker chooses x in order to maximize

$$U^S(\mathbf{c}(x), \mathbf{C}) \;:=\; \sum_{t=1}^{9} \Delta^{-\sigma(c_t(x),\overline{c}_t(\mathbf{C}))} c_t(x). \tag{A.5}$$

A.2.3 Salience Theory and Concentration Bias. Salience theory's first core assumption, ordering, mirrors the basic intuition of the focusing model—according to which larger differences in payoffs attract more attention. As a consequence, in the absence of diminishing sensitivity, that is, if $\sigma(x+\epsilon, y+\epsilon) = \sigma(x, y)$ for all $\epsilon > 0$, salience could also account for the bias toward concentration.

De facto, however, salience is determined by the interplay between ordering and diminishing sensitivity. Suppose that in some period t the largest payoff

[19] If we assume discounted utility, the instantaneous utility function is identical across periods but augmented by the discount function, $u_t(c_t(x)) := D(t)\,u(c_t(x))$.

[20] One important difference between the salience and the focusing model is that under focusing, each period gets a fixed weight which is the same for all available options. By contrast, under salience, each option induces an individual ranking of the attributes, such that the weights for the different periods can differ between the available options.

$\max_{x\in\mathbf{X}} c_t(x)$ increases. Then, the focusing weight attached to payoffs obtained in that period increases. At the same time, however, also the average payoff $\bar{c}_t$ in that period increases (but to a lesser degree). By ordering, $\max_{x\in\mathbf{X}} c_t(x)$ becomes more salient. By diminishing sensitivity, however, the upward shift of $\max_{x\in\mathbf{X}} c_t(x)$ and the simultaneous increase of the average payoff $\bar{c}_t$ reduces the salience of $\max_{x\in\mathbf{X}} c_t(x)$. When, as in this example, ordering and diminishing sensitivity point in opposite directions, the "trade-off between them is pinned down by the specific salience function adopted" (Bordalo, Gennaioli, and Shleifer, 2013, p. 808).

Intuitively, the stronger the ordering property is, relative to diminishing sensitivity, the more likely salience theory is to predict concentration bias. If a salience function is homogeneous, the—otherwise unspecified—trade-off between ordering and diminishing sensitivity is pinned down by its degree of homogeneity. A salience function is called homogeneous (of degree i) if for all $\phi > 0$ there is some $i \in \mathbb{N}_0$ such that $\sigma(\phi\, x, \phi\, y) = \phi^i\, \sigma(x, y)$ for all x, y. In particular, a salience function is homogeneous of degree zero if $\sigma(\phi\, x, \phi\, y) = \sigma(x, y)$ and homogeneous of degree one if $\sigma(\phi\, x, \phi\, y) = \phi\, \sigma(x, y)$. Bordalo, Gennaioli, and Shleifer (2013) use $\sigma_0(x, y) := |x - y|/(|x| + |y|)$ as the standard salience function throughout their analysis. It is homogeneous of degree zero and gives diminishing sensitivity a rather strong role as, for instance, a payoff of \$2 compared to an average of \$1 is as salient as a payoff of \$20 compared to an average of \$10. In contrast, the salience function $\sigma_1(x, y) := (x - y)^2/(|x| + |y|)$ is homogeneous of degree one and gives diminishing sensitivity a much smaller role. A payoff of \$20 compared to an average of \$10 is ten times as salient as a payoff of \$2 compared to an average of \$1. As Leland and Schneider (2016) put it, homogeneity of degree one induces *increasing proportional sensitivity* as $\sigma_1(\phi\, x, \phi\, y) > \sigma_1(x, y)$ for $\phi > 1$. They have proposed a variant of Bordalo, Gennaioli, and Shleifer's (2013) salience model which satisfies increasing proportional sensitivity. In the following, we will use the notion of homogeneity in order to capture the relative importance of diminishing sensitivity.

Given that it is not tractable to solve the maximization problem that a salient thinker faces in our experiment for general salience functions σ and general parameters B and R, we pick up our leading examples, that is, $B = 11$ and $R = 1.15$ and treatments $\mathrm{BAL}^{\mathrm{I}}_{1:1}$ and $\mathrm{UNBAL}^{\mathrm{I}}_{1:8}$ to show how the relative importance of diminishing sensitivity pins down whether salience theory predicts a concentration bias or not.[21]

First, consider treatment $\mathrm{BAL}^{\mathrm{I}}_{1:1}$. Denote by $U^S(\mathbf{c}(x), \mathbf{C}^{\mathrm{BAL,I}}_{1:1})$ the utility of choosing to save fraction x of the endowment for later payment dates when the budget set is $\mathbf{C}^{\mathrm{BAL,I}}_{1:1}$, as individuals face it in condition $\mathrm{BAL}^{\mathrm{I}}_{1:1}$. A salient thinker compares

[21] If diminishing sensitivity was strong enough, salience could in principle also predict the opposite: a *dispersion bias*. The reason is that when payoffs are dispersed, they become smaller per date, and both $\max_x c_t(x)$ and $\bar{c}_t$ are closer to zero. With strong diminishing sensitivity, this can reverse the effect of a small distance $|c_t(x) - \bar{c}_t|$, and the dispersed payoffs could become particularly salient. Since we assume here that utility is linear in monetary payoffs and since we abstract from discounting, a dispersion *bias* cannot occur due to the ceiling effect: it is rational to choose $x = 1$ and shift all payoffs to the future.

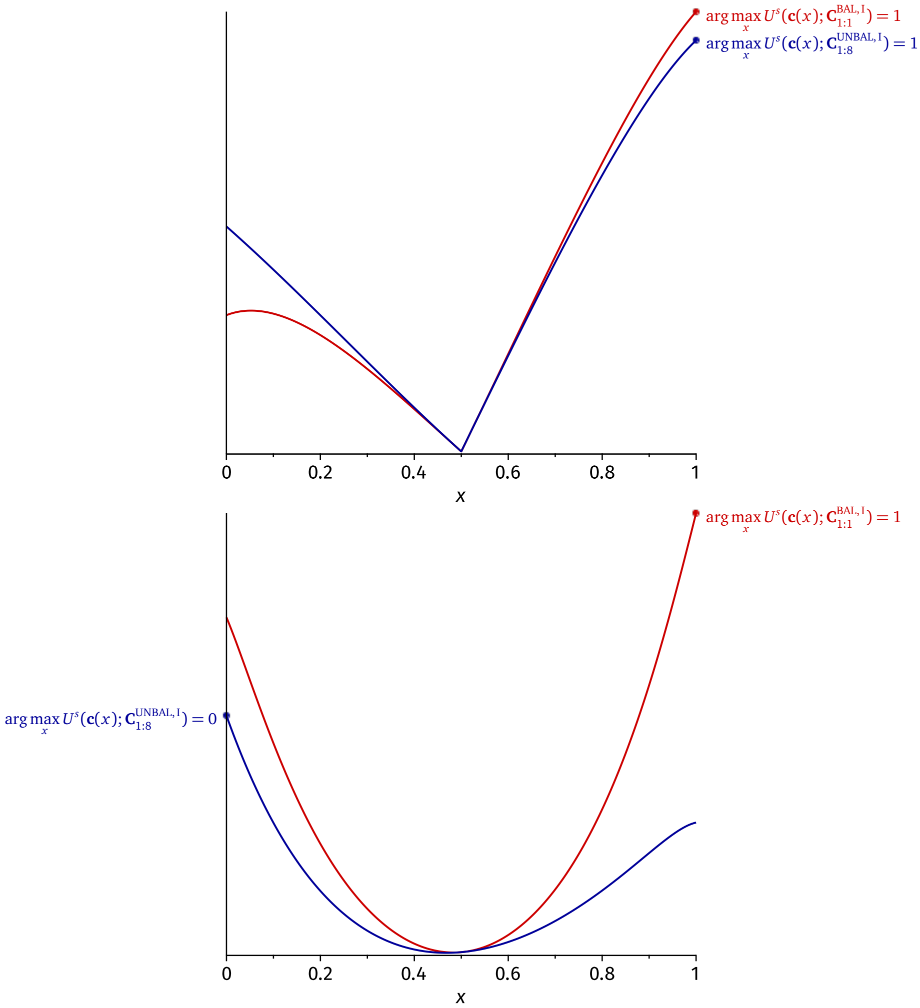


Figure A.1. A salient thinker's choice-relevant intertemporal utility in treatments $\mathrm{BAL}_{1:1}^{\mathrm{I}}$ (red line) and $\mathrm{UNBAL}_{1:8}^{\mathrm{I}}$ (blue line) assessed via $\sigma_0 = |x-y|/(|x|+|y|)$ (upper panel) and $\sigma_1 = (x-y)^2/(|x|+|y|)$ (lower panel). For σ_0, no concentration bias arises, since the utility-maximizing choice is $x=1$ in both cases. By contrast, for σ_1, concentration bias can be observed, since the utility-maximizing choice is $x=0$ for $\mathrm{UNBAL}_{1:8}^{\mathrm{I}}$, while it is $x=1$ for $\mathrm{BAL}_{1:1}^{\mathrm{I}}$.

Note: Parameters used for this illustration are $B = 11$ and $R = 1.15$.

payoff $1 + B\,(1-x)$ on date 1 to the average payment $1 + \frac{B}{2}$, payment 1 to an average of 1 for the dates 2–8, and payment $1 + RBx$ to the average payment on date 9 of $1+\frac{RB}{2}$. Thus, she chooses

$$\begin{aligned} x^s(\mathbf{C}_{1:1}^{\mathrm{BAL,I}};B,R) &:= \arg\max_{x\in\mathbf{X}} U^S(\mathbf{c}(x),\mathbf{C}_{1:1}^{\mathrm{BAL,I}}) \\ &= \Delta^{-\sigma\left(1+B\,(1-x),1+\frac{B}{2}\right)}\cdot[1+B\,(1-x)]+ \\ &\quad \Delta^{-\sigma(1,1)}\cdot 7+ \\ &\quad \Delta^{-\sigma\left(1+RBx,1+\frac{RB}{2}\right)}\cdot[1+RBx]. \end{aligned} \tag{A.6}$$

Second, consider treatment $\mathrm{UNBAL}_{1:8}^{\mathrm{I}}$. $U^S(\mathbf{c}(x),\mathbf{C}_{1:n}^{\mathrm{UNBAL,I}})$ is the utility of saving fraction x of the endowment B for later payment when being confronted with budget set $\mathbf{C}_{1:n}^{\mathrm{UNBAL,I}}$ as individuals face it in condition $\mathrm{UNBAL}_{1:n}^{\mathrm{I}}$. Here, for the first payment date, a salient thinker makes the same comparison as in treatment $\mathrm{BAL}_{1:1}^{\mathrm{I}}$, but for the remaining dates, she compares payoff $1+\frac{RBx}{8}$ to an average of $1+\frac{RB}{16}$. Accordingly, a salient thinker chooses

$$\begin{aligned} x^s(\mathbf{C}_{1:8}^{\mathrm{UNBAL,I}};B,R) &:= \arg\max_{x\in\mathbf{X}} U^S(\mathbf{c}(x),\mathbf{C}_{1:8}^{\mathrm{UNBAL,I}}) \\ &= \Delta^{-\sigma\left(1+B\,(1-x),1+\frac{B}{2}\right)}\cdot(1+B\,(1-x))+ \\ &\quad \Delta^{-\sigma\left(1+\frac{RBx}{8},1+\frac{RBx}{16}\right)}\cdot\left[1+\frac{RB}{8}\right]\cdot 8. \end{aligned} \tag{A.7}$$

An agent reveals the concentration bias whenever s/he is more patient in $\mathrm{BAL}_{1:1}^{\mathrm{I}}$ than in $\mathrm{UNBAL}_{1:8}^{\mathrm{I}}$, that is, $x^s(\mathbf{C}_{1:1}^{\mathrm{BAL,I}};B,R) > x^s(\mathbf{C}_{1:8}^{\mathrm{UNBAL,I}};B,R)$. As we have argued before, it depends on the relative strength of diminishing sensitivity whether this relation holds or not. Figure A.1 (upper panel) depicts $U^S(\mathbf{c}(x),\mathbf{C}_{1:1}^{\mathrm{BAL,I}})$ and $U^S(\mathbf{c}(x),\mathbf{C}_{1:8}^{\mathrm{UNBAL,I}})$ for salience function σ_0. As diminishing sensitivity is strong, a salient thinker accords with rational choice and shifts all payoffs to the future in both treatments: $x^s(\mathbf{C}_{1:1}^{\mathrm{BAL,I}};11,1.15) = x^s(\mathbf{C}_{1:8}^{\mathrm{UNBAL,I}};11,1.15)=1$. If, in contrast, salience is assessed via salience function σ_1, a salient thinker shifts all payoffs to the future in treatment $\mathrm{BAL}_{1:1}^{\mathrm{I}}$, but not in $\mathrm{UNBAL}_{1:8}^{\mathrm{I}}$ (see Figure A.1, lower panel). Here, the ordering property yields a bias toward concentration: a salient thinker chooses $x=0$ if later payoffs are dispersed, while she is patient when the later payoff is concentrated.

In summary, this illustrates that with sufficiently weak diminishing sensitivity, salience theory also predicts a concentration bias and can qualitatively explain the findings of our experiment.

Appendix B Alternative Experimental Approaches

B.1 Choice Lists

To show that our findings are not specific to one single method, we use choice lists as an additional robustness check for Hypothesis 1. Each subject completed 24 trials in which they were endowed with different choice lists. Each choice list included nine earnings sequences, $\mathbf{C} = \{\mathbf{c}(1), \ldots, \mathbf{c}(9)\}$. That is, as before, we denote the set of earnings sequences from which subjects could choose with $\mathbf{C}$. We use $k \in \mathbf{K}$ with $\mathbf{K} = \{1, \ldots, 9\}$ to index the elements $\mathbf{c}(k)$ of $\mathbf{C}$.

Just like the earnings sequences in the budget sets, each option included in the choice lists consisted of nine money transfers to subjects' bank accounts at payment dates $t = 1, \ldots, 9$. Also here, there was a €1 fixed payment at each date plus an additional amount of money which depended on the chosen earnings sequence.

w weeks $\qquad$ w weeks $\qquad$ t

$\mathbf{c}^{\text{BAL}}_{\text{CL}}(1)$:	$1 + B$	1	1	1	1	1	1	1	1
$\mathbf{c}^{\text{BAL}}_{\text{CL}}(2)$:	1	$1 + B + i$	1	1	1	1	1	1	1
$\mathbf{c}^{\text{BAL}}_{\text{CL}}(3)$:	1	1	$1 + B + 2i$	1	1	1	1	1	1
$\mathbf{c}^{\text{BAL}}_{\text{CL}}(4)$:	1	1	1	$1 + B + 3i$	1	1	1	1	1
$\mathbf{c}^{\text{BAL}}_{\text{CL}}(5)$:	1	1	1	1	$1 + B + 4i$	1	1	1	1
$\mathbf{c}^{\text{BAL}}_{\text{CL}}(6)$:	1	1	1	1	1	$1 + B + 5i$	1	1	1
$\mathbf{c}^{\text{BAL}}_{\text{CL}}(7)$:	1	1	1	1	1	1	$1 + B + 6i$	1	1
$\mathbf{c}^{\text{BAL}}_{\text{CL}}(8)$:	1	1	1	1	1	1	1	$1 + B + 7i$	1
$\mathbf{c}^{\text{BAL}}_{\text{CL}}(9)$:	1	1	1	1	1	1	1	1	$1 + B + 8i$

Figure B.2. Choice Lists: Earnings Sequences Included in $\mathbf{C}^{\text{BAL}}_{\text{CL}}$

Note: For the values of B, i, and w that we used, see Section B.1.

	w weeks						w weeks		t
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(1)$:	$1 + B$	1	1	1	1	1	1	1	1
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(2)$:	$1 + \frac{B+i}{2}$	$1 + \frac{B+i}{2}$	1	1	1	1	1	1	1
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(3)$:	$1 + \frac{B+2i}{3}$	$1 + \frac{B+2i}{3}$	$1 + \frac{B+2i}{3}$	1	1	1	1	1	1
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(4)$:	$1 + \frac{B+3i}{4}$	$1 + \frac{B+3i}{4}$	$1 + \frac{B+3i}{4}$	$1 + \frac{B+3i}{4}$	1	1	1	1	1
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(5)$:	$1 + \frac{B+4i}{5}$	$1 + \frac{B+4i}{5}$	$1 + \frac{B+4i}{5}$	$1 + \frac{B+4i}{5}$	$1 + \frac{B+4i}{5}$	1	1	1	1
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(6)$:	$1 + \frac{B+5i}{6}$	$1 + \frac{B+5i}{6}$	$1 + \frac{B+5i}{6}$	$1 + \frac{B+5i}{6}$	$1 + \frac{B+5i}{6}$	$1 + \frac{B+5i}{6}$	1	1	1
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(7)$:	$1 + \frac{B+6i}{7}$	$1 + \frac{B+6i}{7}$	$1 + \frac{B+6i}{7}$	$1 + \frac{B+6i}{7}$	$1 + \frac{B+6i}{7}$	$1 + \frac{B+6i}{7}$	$1 + \frac{B+6i}{7}$	1	1
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(8)$:	$1 + \frac{B+7i}{8}$	$1 + \frac{B+7i}{8}$	$1 + \frac{B+7i}{8}$	$1 + \frac{B+7i}{8}$	$1 + \frac{B+7i}{8}$	$1 + \frac{B+7i}{8}$	$1 + \frac{B+7i}{8}$	$1 + \frac{B+7i}{8}$	1
$\mathbf{c}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}(9)$:	$1 + \frac{B+8i}{9}$	$1 + \frac{B+8i}{9}$	$1 + \frac{B+8i}{9}$	$1 + \frac{B+8i}{9}$	$1 + \frac{B+8i}{9}$	$1 + \frac{B+8i}{9}$	$1 + \frac{B+8i}{9}$	$1 + \frac{B+8i}{9}$	$1 + \frac{B+8i}{9}$

Figure B.3. Choice Lists: Earnings Sequences Included in $\mathbf{C}_{\mathrm{CL}}^{\mathrm{UNBAL,I}}$

Note: For the values of B, i, and w that we used, see Section B.1.

Subjects faced three different types of choice lists: $\mathrm{BAL}_{\mathrm{CL}}$ with one concentrated payoff for each option k, illustrated in Figure B.2; $\mathrm{UNBAL}_{\mathrm{CL}}^{\mathrm{I}}$ with an increasing degree of dispersion of the payoff, see Figure B.3; and $\mathrm{UNBAL}_{\mathrm{CL}}^{\mathrm{II}}$ with a decreasing degree of dispersion, see Figure B.4. The degree of dispersion was equal to k in $\mathrm{UNBAL}_{\mathrm{CL}}^{\mathrm{I}}$, while it was $10-k$ in $\mathrm{UNBAL}_{\mathrm{CL}}^{\mathrm{II}}$.

We varied time w (in weeks) between two consecutive payment dates, budget B (in €) that could be paid out on the first payment date and interest i (in €) additionally paid when picking choice k instead of $k-1$. In the first wave, each of the ($N = 93$) subjects faced 8 choice lists ($w \in \{3,6\}$; $B \in \{8,11\}$; $i \in \{0.2, 0.5\}$) each for $\mathrm{BAL}_{\mathrm{CL}}$, $\mathrm{UNBAL}_{\mathrm{CL}}^{\mathrm{I}}$, and $\mathrm{UNBAL}_{\mathrm{CL}}^{\mathrm{II}}$.[22] In the second wave, all ($N = 92$) subjects made 8 decisions ($w \in \{2,3\}$; $B \in \{8,11\}$; $i \in \{0.2, 0.8\}$) each for $\mathrm{BAL}_{\mathrm{CL}}$ and $\mathrm{UNBAL}_{\mathrm{CL}}^{\mathrm{I}}$ and another 8 decisions ($w \in \{4,6\}$; $B \in \{8,11\}$; $i \in \{0.2, 0.8\}$) for $\mathrm{UNBAL}_{\mathrm{CL}}^{\mathrm{II}}$.

[22] Choices with $w = 3$ could not be used for the analysis, see Section B.1.1.

w weeks $\qquad\qquad$ w weeks $\qquad$ t

$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(1)$:	$1+\frac{B}{9}$	$1+\frac{B}{9}$	$1+\frac{B}{9}$	$1+\frac{B}{9}$	$1+\frac{B}{9}$	$1+\frac{B}{9}$	$1+\frac{B}{9}$	$1+\frac{B}{9}$	$1+\frac{B}{9}$
$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(2)$:	1	$1+\frac{B+i}{8}$	$1+\frac{B+i}{8}$	$1+\frac{B+i}{8}$	$1+\frac{B+i}{8}$	$1+\frac{B+i}{8}$	$1+\frac{B+i}{8}$	$1+\frac{B+i}{8}$	$1+\frac{B+i}{8}$
$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(3)$:	1	1	$1+\frac{B+2i}{7}$	$1+\frac{B+2i}{7}$	$1+\frac{B+2i}{7}$	$1+\frac{B+2i}{7}$	$1+\frac{B+2i}{7}$	$1+\frac{B+2i}{7}$	$1+\frac{B+2i}{7}$
$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(4)$:	1	1	1	$1+\frac{B+3i}{6}$	$1+\frac{B+3i}{6}$	$1+\frac{B+3i}{6}$	$1+\frac{B+3i}{6}$	$1+\frac{B+3i}{6}$	$1+\frac{B+3i}{6}$
$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(5)$:	1	1	1	1	$1+\frac{B+4i}{5}$	$1+\frac{B+4i}{5}$	$1+\frac{B+4i}{5}$	$1+\frac{B+4i}{5}$	$1+\frac{B+4i}{5}$
$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(6)$:	1	1	1	1	1	$1+\frac{B+5i}{4}$	$1+\frac{B+5i}{4}$	$1+\frac{B+5i}{4}$	$1+\frac{B+5i}{4}$
$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(7)$:	1	1	1	1	1	1	$1+\frac{B+6i}{3}$	$1+\frac{B+6i}{3}$	$1+\frac{B+6i}{3}$
$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(8)$:	1	1	1	1	1	1	1	$1+\frac{B+7i}{2}$	$1+\frac{B+7i}{2}$
$\mathbf{c}_{\text{CL}}^{\text{UNBAL, II}}(9)$:	1	1	1	1	1	1	1	1	$1+B+8i$

Figure B.4. Choice Lists: Earnings Sequences Included in $\mathbf{C}_{\text{CL}}^{\text{UNBAL, II}}$

Note: For the values of B, i, and w that we used, see Section B.1.

As above, we compare within-subject average choices between BAL_{CL} and $\text{UNBAL}_{\text{CL}}^{\text{I}}$ and between BAL_{CL} and $\text{UNBAL}_{\text{CL}}^{\text{II}}$, respectively. We consider choice of a higher option k as more patient.

B.1.1 Predictions. We start with the predictions for standard discounted utility and then derive predictions for the "focusing model" by Kőszegi and Szeidl (2013).

Discounted Utility. Individuals compare intertemporal utility $U(\mathbf{c})$ of each earnings sequence $\mathbf{c}$ included in a choice list $\mathbf{C}$ and pick the option $k^\star$ with the highest utility. We examine the comparison between BAL_{CL} and $\text{UNBAL}_{\text{CL}}^{\text{I}}$ first. In BAL_{CL}, individuals pick the option

$$k^\star(\mathbf{C}_{\text{CL}}^{\text{BAL}}; B, i) := \arg\max_{k \in \mathbf{K}} \sum_{t=1, t\neq k}^{9} D(t)\, u(1) + D(k)\, u(1 + B + (k-1)\, i).$$

In $\text{UNBAL}^{\text{I}}_{\text{CL}}$, they choose

$$k^\star(\mathbf{C}^{\text{UNBAL, I}}_{\text{CL}}; B, i) \; := \; \arg\max_{k\in\mathbf{K}} \sum_{t=1}^{k} D(t)\, u\Big(1 + \frac{B + (k-1)\,i}{k}\Big) + \sum_{t=k+1}^{9} D(t)\, u(1).BAL$$

When comparing utilities between BAL_{CL} and $\text{UNBAL}^{\text{I}}_{\text{CL}}$ for some specific option k, utility for the latter is always higher due to the (weak) concavity of the utility function u and due to less discounting for a large part of the dispersed payments:

$$\begin{aligned}
& \sum_{t=1,t\neq k}^{9} D(t)\, u(1) + D(k)\, u(1 + B + (k-1)\, i) \\
\leq \quad & \sum_{t=1}^{k} D(t)\, u\Big(1 + \frac{B + (k-1)\,i}{k}\Big) + \sum_{t=k+1}^{9} D(t)\, u(1) \\
\Longleftrightarrow \quad & \sum_{t=1}^{k-1} D(t)\, u(1) + D(k)\, u(1 + B + (k-1)\, i) \\
\leq \quad & \sum_{t=1}^{k} D(t)\, u\Big(1 + \frac{B + (k-1)\,i}{k}\Big).
\end{aligned}$$

As a consequence, individuals are (weakly) better off in $\text{UNBAL}^{\text{I}}_{\text{CL}}$ and choose an at least as patient option as in BAL_{CL}. That is,

$$d^\star_{1,\text{CL}} \; := \; k^\star(\mathbf{C}^{\text{BAL}}_{\text{CL}}) - k^\star(\mathbf{C}^{\text{UNBAL, I}}_{\text{CL}}) \; \leq \; 0.$$

We consider BAL_{CL} and $\text{UNBAL}^{\text{II}}_{\text{CL}}$ next. We use the same concentrated treatment as above as the benchmark. The optimal choice for $\text{UNBAL}^{\text{II}}_{\text{CL}}$ is

$$k^\star(\mathbf{C}^{\text{UNBAL, II}}_{\text{CL}}; B, i) \; := \; \arg\max_{k\in\mathbf{K}} \sum_{t=1}^{k-1} D(t)\, u(1) + \sum_{t=k}^{9} D(t)\, u\Big(1 + \frac{B + (k-1)\,i}{9 - (k-1)}\Big).$$

When comparing utility between BAL_{CL} and $\text{UNBAL}^{\text{II}}_{\text{CL}}$ for some option k, the (weak) concavity of the utility function u makes the individual in the dispersed case better off. However, at the same time, payments occur later and are thus discounted more strongly. To weaken the second motive, we doubled the time w between consecutive payment dates in $\text{UNBAL}^{\text{II}}_{\text{CL}}$.[23] One can show for the parameters of our experiment (i.e., the durations and interest payments that we used) that under exponential discounting and linear utility, individuals should be more patient in BAL_{CL} than

[23] Due to this extension, a robustness check as performed in Section 3 is not possible for a comparison between BAL_{CL} and $\text{UNBAL}^{\text{II}}_{\text{CL}}$.

in $\text{UNBAL}^{\text{II}}_{\text{CL}}$:

$$d^{\star}_{2,\text{CL}} \;:=\; k^{\star}(\mathbf{C}^{\text{UNBAL, II}}_{\text{CL}}) - k^{\star}(\mathbf{C}^{\text{BAL}}_{\text{CL}}) \;\leq\; 0.$$

Concentration Bias. When investigating the predictions of Kőszegi and Szeidl's (2013) focusing model, keep in mind that each choice list consists of nine possible earnings sequences which span the entire utility range for each payment date. For BAL_{CL}, the minimum possible utility is always $u_t(1)$, while the maximum possible utility for payment date t is $u_t(1 + B + (k-1)\,i)$ (which corresponds to option $k = t$).

We again start with the comparison between BAL_{CL} and $\text{UNBAL}^{\text{I}}_{\text{CL}}$. In BAL_{CL}, the utility difference $\Delta_t(\mathbf{C}^{\text{BAL}}_{\text{CL}})$ ranges from $u_1(1+B) - u_1(1)$ to $u_9(1+B+8i) - u_9(1)$. In contrast, the utility difference $\Delta_t(\mathbf{C}^{\text{UNBAL, I}}_{\text{CL}})$ ranges from $u_1(1+B) - u_1(1+\frac{B+8i}{9})$ to $u_9(1+\frac{B+8i}{9}) - u_9(1)$. As one can see, the relative weighting of the last date is greater in BAL_{CL}, i.e., $g_9(\mathbf{C}^{\text{BAL}}_{\text{CL}})\,/\,g_1(\mathbf{C}^{\text{BAL}}_{\text{CL}}) > g_9(\mathbf{C}^{\text{UNBAL, I}}_{\text{CL}})\,/\,g_1(\mathbf{C}^{\text{UNBAL, I}}_{\text{CL}})$. If this relative underweighting in $\text{UNBAL}^{\text{I}}_{\text{CL}}$ is sufficiently strong, focus-weighted utility predicts a less patient choice in $\text{UNBAL}^{\text{I}}_{\text{CL}}$ than in BAL_{CL}, and one gets

$$d^{\star\star}_{1,\text{CL}} \;:=\; k^{\star\star}(\mathbf{C}^{\text{BAL}}_{\text{CL}}) - k^{\star\star}(\mathbf{C}^{\text{UNBAL, I}}_{\text{CL}}) \;>\; 0.$$

We study the implications of focus-weighted utility on the comparison between BAL_{CL} and $\text{UNBAL}^{\text{II}}_{\text{CL}}$ next. For $\text{UNBAL}^{\text{II}}_{\text{CL}}$, the reverse of $\text{UNBAL}^{\text{I}}_{\text{CL}}$—which featured increasing dispersion—holds: the utility range of the first payment date is much smaller than that of the last date, $u_1(1+B/9) - u_1(1)$ versus $u_9(1+B+8i) - u_9(1+B/9)$. That results in a stronger relative overweighting of the last payment date for $\text{UNBAL}^{\text{II}}_{\text{CL}}$, i.e., $g_9(\mathbf{C}^{\text{UNBAL, II}}_{\text{CL}})\,/\,g_1(\mathbf{C}^{\text{UNBAL, II}}_{\text{CL}}) > g_9(\mathbf{C}^{\text{BAL}}_{\text{CL}})\,/\,g_1(\mathbf{C}^{\text{BAL}}_{\text{CL}})$. Here, if this relative underweighting is sufficiently strong, focus-weighted utility predicts a more patient choice in $\text{UNBAL}^{\text{II}}_{\text{CL}}$ than in BAL_{CL}:

$$d^{\star\star}_{2,\text{CL}} \;:=\; k^{\star\star}(\mathbf{C}^{\text{UNBAL, II}}_{\text{CL}}) - k^{\star\star}(\mathbf{C}^{\text{BAL}}_{\text{CL}}) \;>\; 0.$$

B.1.2 Results. Subjects made multiple allocation decisions in our experiment. In particular, they made 8 allocation decisions for each of the three different treatments, BAL_{CL}, $\text{UNBAL}^{\text{I}}_{\text{CL}}$, and $\text{UNBAL}^{\text{II}}_{\text{CL}}$. This allows us to calculate for each individual the average difference of choices between BAL and associated UNBAL choice lists. Denote by $\hat{d}_{1,\text{CL}}$ and $\hat{d}_{2,\text{CL}}$ the empirical counterparts of the variables $d^{\star\star}_{1,\text{CL}}$ and $d^{\star\star}_{2,\text{CL}}$.

We find evidence in support of Hypothesis 1 in both directions. Subjects chose a lower option of $\hat{d}_{1,\text{CL}} = 0.573$ in $\text{UNBAL}^{\text{I}}_{\text{CL}}$, compared to BAL_{CL}, and a higher option of $\hat{d}_{2,\text{CL}} = 0.933$ in $\text{UNBAL}^{\text{II}}_{\text{CL}}$ than in BAL_{CL}. These treatment effects are statistically significant in both a t-test and a signed-rank test ($p < 0.001$ according to both tests).

B.2 An Alternative Experimental Approach

Besides the experiment described in the main text, we have employed a different experimental approach, a revealed-preference approach, to investigate the concentration bias in intertemporal choice. Also when using this approach, we find evidence for concentration bias—which demonstrates that our support of focusing and the concentration bias does not hinge on our specific experimental setup.

This experimental approach consists of the following two steps: In a first phase, we elicit time preferences in an environment where focus-weighted thinking is aligned with rational choice. In a second phase, we use these elicited time preferences to construct decision environments in which subjects should exhibit a bias according to the focusing model. More precisely, in the first phase, subjects have to decide between concentrated options only for which focusing theory predicts rational behavior, while in the second phase, we use unbalanced decision situations for which focusing predicts distorted choices.

B.2.1 The Experimental Design. For half of the subjects, we tested for present-biased choices that are predicted by focusing ("Present Frame"), and for the other half, we tested for future-oriented choices that are also predicted by focusing ("Future Frame"). All payments were made via bank transfers.

First Stage. In the Present Frame, we elicited the largest amount $x \in \{€5.00, €5.50, \ldots, €8.00\}$ to be received today for which subjects would just prefer to receive €8 in one month. In the Future Frame, we elicited the largest amount $x \in \{€5.00, €5.50, \ldots, €8.00\}$ to be received in one month for which subjects would just prefer to receive €8 in two months.

Second Stage. For all subjects, we elicited the smallest amount $y \in \{€5.00, €5.50, \ldots, €10.00\}$ to be paid in two months for which subjects would just prefer receiving €5 today.

Third Stage: Focusing question. Making use of the values x and y elicited in the first two stages, we tested for the focusing bias. We did so by asking subjects

- in the Present Frame whether they preferred (A) $€x + €5$ today or (B) €8 in one month and $€y + €0.50$ in two months; and
- in the Future Frame whether they preferred (A') €5 today and $€x + €0.50$ in one month or (B') $€8 + €y$ in two months.

Table B.1. Experimental Schedule

First phase	Elicitation of switching points via price lists
Second phase	Focusing decision
Control questions	Cognitive Reflection Test Memory Demographics

Whoever opts for (A) in the Present Frame violates her preferences revealed in the prior tasks in the direction predicted by focusing again under monotonously increasing and non-convex utility for money. Analogously, whoever chooses the concentrated payout (B') in the Future Frame decides in line with concentration bias and forgoes surplus. We hypothesized that in both groups a significant share of subjects would exhibit concentration bias. That is, we test the following hypothesis:

Hypothesis 3. *A substantial share of subjects in both groups opt for the inferior option which gives a concentrated reward.*

B.2.2 Controls and Filler Tasks. To enhance subjects' concentration on the decision-making tasks, we included two filler tasks. After the first choice from the price lists, subjects in each treatment had to count all "1"s in a binary code of 1,000 digits within six minutes. The closer the subject's result to the true value, the higher her resulting payoff. The next filler task, in which eight trivia questions had to be answered, was scheduled prior to the task that tests for the focusing bias.

In addition, we included two controls which we tested for after all the other tasks had been accomplished (the order of the tasks is given in Table B.1). As in our main experiment, we included the cognitive reflection test. Second, we tested for subjects' memory by repeating the second price list choice task in which subjects chose between €5 today and an overall future sum of €5 up to €10. We explicitly asked them to remember their *previous* decision and rewarded them with €5 if their answer exactly matched the decision pattern and nothing otherwise. Hence, in the "Memory" task, we expected subjects to match their decisions made during the preference elicitation task. We included this task in order to make sure that subjects did not decide randomly but made their decisions consciously in a way that choices could be remembered afterwards. The more honestly a person revealed her true preferences during the task's first iteration, the more likely we would expect her to remember her choices. Ensuring that subjects revealed their true preferences earlier is important for this approach because establishing the concentration bias relies on subjects' earlier choices.

B.2.3 Incentivization. To avoid differences in the payment mechanism between the options, all sums were paid via bank transfers. All points in time mentioned throughout our experiment do not indicate the date on which the respective sum is received, but the date on which it is transferred to their bank account. To enhance our trustworthiness, we adopted the procedure by Andreoni and Sprenger (2012) and provided all subjects with the contact information (phone and e-mail) of one of the authors (Riener) and encouraged them to contact him immediately if any payments were delayed. As Andreoni and Sprenger state, this invitation to inconvenience a professor was intended to boost confidence that future payments would arrive as promised.

Table B.2. Classification of Choices in Percentages

Frame	Present ($n = 43$)	Future ($n = 46$)	Total ($n = 89$)
Focusing bias	41.9%	41.3%	41.5%
No focusing bias	46.5%	52.2%	49.4%
Not classifiable/Inconsistent	11.6%	6.5%	9.0%

One task was selected by the computer at random for payout. Each of the price lists plus the counting, trivia, memory, and CRT questions were chosen with equal probability. Only this chosen task was paid. The price lists were incentivized via the standard procedure that one line was randomly selected and the option selected in that row was paid at the indicated point in time. For the other tasks, correct answers were paid.

B.2.4 Implementation. The experiment was conducted at the DICE Laboratory at the University of Düsseldorf in June 2014 and December 2015. In total, 89 student subjects—recruited via ORSEE (Greiner, 2015)—participated in the experiment. The experiment was programmed in z-Tree. An average session lasted 45 minutes, and the average payout was €9, with a minimum of €3 and a maximum of €16.50.

B.2.5 Results. First, we categorize subjects according to their answers in these three tasks as follows. A subject is "inconsistent" if s/he revealed more than one switching point in at least one of the multiple price list tasks. If a subject always opted for the earlier payout in at least one of the multiple price list tasks, then our procedure does not allow to construct the inferior option we need, such that we categorize her as "not classifiable" as—if present—we cannot identify the focusing bias for this subject. We find that three of the subjects behaved inconsistently and nine were not classifiable.

We find substantial evidence of the descriptive power of focusing theory for actual decisions (Table B.2). When excluding subjects who are not classifiable, 47.4% of the subjects exhibit a concentration bias (test of probabilities, $p < 0.001$; 95% confidence interval, $[0.364, 0.585]$).[24] Performing robustness checks using a linear probability model accounting for heteroskedasticity and controlling for session fixed effects estimates a constant of 0.5 (standard error, 0.137). This constitutes robust evidence in favor of our hypothesis.

[24] We do not think that our results are driven by noise in subjects' decisions. First, noisy choices would be plausible if subjects were confused. The decision situations, however, are all very easy to grasp. Second, the results of task "Memory" demonstrate that most subjects could remember all their decisions: in fact, subjects can on average correctly remember 10.76 out of their 11 decisions of the second price list. This supports our assumption that decisions are deliberate with little noise.

Appendix C Instructions

These are the instructions (translated from the German original) for the main experiment and the control experiment. While the text of the instructions for the main and control experiment were the same, the income sequences displayed on the respective screens were different. See Section 3 for details.

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 gain nine €1 payments for sure, which will be transferred to your bank account at various dates in the future. Furthermore, you receive one or multiple additional payment(s) for the first part of the experiment. For the latter one you can decide by yourself when these additional payment(s) will be transferred. The following is always the case: **If you choose a later payment, you receive, in total, more money than when choosing an earlier payment.**

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

Your payment for part one will be transferred to your bank account. All orders for transfers will be transmitted to the bank today. We will send you an e-mail with all the data transmitted to the bank, such that you can check whether all payments are ordered correctly!

After the last transfer you will receive another e-mail which reminds you of all different payments and dates.

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

Part 2

In the second part of the experiment, we would like to ask you to perform a task. You will receive money for doing this task. We will provide information about the exact payment right before the beginning of the second part. 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. *On this screen, subjects enter their banking data.*

Screen 4—Choice Lists.[25]

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, in total more money later in time.

In the following, before the experiment starts, we show you two possible payment-alternatives of a decision such that you get familiar with the decision screens of this experiment.

Screen 5—Example 1. In this example, the first alternative has been chosen. The slider is positioned in a way that payment-alternative 1 is displayed. In this example, payment-alternative 1 corresponds to a payment of €8 at 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 chosen. The slider is positioned in a way that payment-alternative 6 is displayed. In this example, payment-alternative 6 corresponds to a multiple payment of €1.50 at each highlighted date. 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 distinguish themselves in the total amount of money and the points in time where transfers are realised. The following is always the case: If you choose a later payment, you receive, in total, more money than choosing an earlier payment.

At the next screen, all nine payment-alternatives of this decision are 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 can choose your most preferred alternative.

This hint will be shown for the first four decisions.

Screen 8—Budget Sets.[26]

Part 1b
In part 1b you have to make 36 decisions.

In each decision you have the possibility to divide a certain amount of money between earlier and later dates. The less money you allocate to earlier dates, the more money you receive later. In other words, the total amount of money received is higher when a bigger part is allocated to later dates.

[25] We analyze the intertemporal decisions with respect to the the choice lists in Section B.1.
[26] We analyze the intertemporal decisions with respect to the the budget sets in the main text of this paper.

You make the decisions by using a slider with your mouse.

You can practice the use of the slider:

You move a red marker by moving your mouse over the dark-grey bar (do not click!). If you click at the red marker, your choice is loged and can be saved afterwards. There will appear a red Button "Record choice!". After clicking this button, your current choice is saved.

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

Screen 9—End of Part 1. This was the last decision of the first part 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 on the button "Continue."

Screen 10—Part 2. In this part we would like to ask you to add up figures as often as you can manage.

You have 5 minutes time for exercising this task.

You receive a base payment of €1 for this part.

The more numbers you can sum up correctly, the more money you can gain: You receive €0.20 for each correct summation.

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

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

Screen 11. You have solved X tasks correctly and entered X times a wrong solution in all three attempts.

You receive €Y for this task. You will receive the money in a few minutes.

Screen 12. The experiment will be over soon. Finally, we would like to ask you to answer ten questions. After answering these ten questions, you will learn your payment for the first part and get paid for the second part.

Screen 13—CRT 1. A bat and a ball cost €1.10. 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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