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Louis Mick Strang

aus Bergisch Gladbach

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Dekan:	Prof. Dr. Jürgen von Hagen
Erstreferent:	Prof. Dr. Lorenz Götte
Zweitreferent:	Prof. Dr. Sebastian Kube
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Introduction

Decision making and interacting with each other is part of humans' everyday life. In the course of this, humans often use heuristics and are prone to biases in their choices (Tversky and Kahneman, 1974; Kahneman, 2003). Furthermore, they deviate in their behavior from the rational agent by caring for others, sacrificing own resources such as money or time. Behavioral economics has developed to i) model this *real behavior* of individuals and groups with its imperfections and inconsistencies–from the perspective of classical models–and ii) to empirically investigate how people behave in a specific situation as well as to test the credibility of this new class of models. The latter is often conducted in the controlled setting of a laboratory or field experiment. The exogenous control over the environment enables the experimenter to provide causal evidence for the sources of human behavior.

This is also the tool I apply in my dissertation. I examine various areas of human life and decision making: Do people focus on certain periods in intertemporal choice and does this alter their patience? Does the interdependence of individual success and prosperity matter for fairness perceptions? Do people care about others' judgement of their moral choices; and do they alter their prosocial attitudes over time based on earlier observability?

In Chapter 1 (joint with Markus Dertwinkel-Kalt, Holger Gerhardt, Gerhard Riener, and Frederik Schwerter), I examine asymmetries of intertemporal trade-offs and their implications for the patience of individuals. According to the "focusing model" by Kőszegi and Szeidl (2013), the concentration in time of utility outcomes is critical for intertemporal choices. People *disproportionately* focus on periods with large differences in possible utility outcomes and neglect those points in time with dispersed utility consequences. This focus results in choices of concentrated benefit and the neglection of dispersed costs and can lead to both "present-biased" and "future-biased" behavior, based on a so-called concentration bias. We use two laboratory experiments, either using consumption (real effort and a restaurant voucher) or money as an utility event. In both experiments, we find substantial evidence in favor of concentration bias. Moreover, we provide additional conditions that investigate two potential channels of concentration bias. These are concentration in time, reflecting the fundamental assumption of the focusing model of time points being different attributes, and another interpretation of the model, that is accessibility.

Accessibility takes into account how tangible and easy to grasp a consequence happens to be and, hence, the difficulty of processing information into utility outcomes. Conditional on the setup, we find that accessibility accounts for 50 to 64% of the overall concentration-bias effect.

In Chapter 2 (joint with Sebastian Schaube), I investigate the importance of the interdependence of people's success and payoffs for preferences for redistribution. Does it matter whether life is a zero-sum game or that everyone is the architect of her own fortune? Economically speaking, this translates to: Either high outcomes for a given individual directly result in low outcomes for others, or multiple persons can be successful and obtain high outcomes at once. We examine differences in fairness perceptions between these two opposing systems in a laboratory experiment. We let two subjects work on a real-effort task, where their performance maps into chances of winning a prize. Between treatments, we implement either a zero-sum setting where only one subject can win a prize, or both can potentially win a prize simultaneously. After the realization of outcomes, a third subject acts as a spectator and may redistribute earnings between the two former subjects. We compare these redistribution decisions across treatments for an identical level of inequality. If payoffs are not directly interdependent, the average amount redistributed decreases by 14-22%. In these treatments, subjects can only influence their *chances* of winning a prize, thus randomness affects the outcomes. In additional treatments, we remove this random component and prize allocation is solely determined by performance. The impact of payoff interdependence remains unchanged. Finally, comparing the settings with and without randomness, we find that its mere presence increases redistribution, even though there is no uncertainty about the (relative) performance of the two subjects.

In Chapter 3 (joint with Jana Hofmeier), I study the dynamic effect of observability and social image on prosocial behavior. We conjecture that social image not only has a direct effect on observed actions but also caries its (positive) impact beyond that to future moral decisions. We therefore hypothesize a twofold positive effect. First, people should act more prosocially when being observed, as has been shown in previous work (e.g., Ariely, Bracha, and Meier, 2009; DellaVigna, List, and Malmendier, 2012). Second, this increased level of prosociality should motivate an ongoing elevated altruistic attitude. This spillover is captured with the concept of altruistic capital formation, which states that people form a habit of behaving morally whenever showing an altruistic attitude. We test our predictions with two laboratory experiments in which subjects have to make two donation decisions. In the first donation decision, we exogenously vary whether subjects are observed when making their decision or are acting anonymously. Subsequently, all subjects face a second anonymous donation decision. The two experiments differ in the currency of giving, that is either effort or money, as well as in the implementation of the observability. In general, we observe high rates of altruistic behavior. However, this is true regardless of being observed or not, hence, we find only weak positive effects of observability on first stage prosocial behavior. Moreover, as this overall altruistic attitudes carry on to the second decision, we do not observe any positive effects of social image on second stage prosocial behavior.

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Chapter 1

Concentration Bias in Intertemporal Choice*

Joint with Markus Dertwinkel-Kalt, Holger Gerhardt, Gerhard Riener, and Frederik Schwerter

1.1 Introduction

A pervasive feature of intertemporal trade-offs is their *unbalanced* nature: the benefit of an action and its associated cost often affect different numbers of periods. For instance, a benefit may occur concentrated in a few time periods, while the cost is dispersed over many time periods: the prospect of receiving a large bonus payment at the end of a year may come at the cost of working half an hour overtime each day that year; choosing a financing plan instead of paying lump-sum for an expensive durable consumer good splits the high cost into less tangible instalments; and avoiding the considerable effort of exercising in the gym today only negligibly deteriorates one's physical well-being each day in the future.

This observation raises the question whether differential degrees to which utility benefits and costs are concentrated in time affect individual decision making. Kőszegi and Szeidl (2013) propose a "model of focusing" in which a decision maker does not pay appropriate attention to all time periods with relevant utility outcomes. In particular, her focus is drawn disproportionately to those time periods for which the utility outcomes of her options differ most. As a consequence, the appeal of an option with utility benefits in these time periods increases. Formally, the Kőszegi–Szeidl model maintains the assumption of additively separable utility, as it is made in standard models of exponential and (quasi-)hyperbolic discounting (Samuelson, 1937;

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Loewenstein and Prelec, 1992; Laibson, 1997), and introduces a second weight that scales per-period utility on top of standard temporal discounting. These "focusing" weights increase in the per-period difference between the minimum and maximum attainable utility outcome.

The implications of the focusing model for intertemporal choice crucially depend on whether utility outcomes are differentially concentrated in time. In a *balanced* choice, when the utility benefit and cost of an option affect the same number of periods, the Kőszegi–Szeidl model corresponds to standard discounting: focusing only reinforces preexisting differences captured by standard discounting. In unbalanced choices, by contrast, the decision maker is too prone to choose the option with utility benefits concentrated in time, potentially neglecting substantial dispersed costs. Crucially, such concentration bias can lead to both "present-biased" and "future-biased" behavior—judged by the decision maker's time preferences and per-period utility functions captured by standard discounting: a late concentrated benefit may lead to a future-biased choice, while an early concentrated benefit may lead to a present-biased choice.[1]

Despite the prevalence of unbalanced intertemporal trade-offs in various economic applications and their potentially important implications for behavior, direct empirical evidence on whether and how the differential degree of concentration in time affects individuals' intertemporal decisions is scarce. To make progress, we investigate the causal effect of concentration in time on intertemporal choice with two complementary laboratory experiments: one involving dated consumption events, the other involving dated monetary payments. Besides identifying and quantifying concentration bias in intertemporal choice, our designs also allow us to study the mechanisms behind concentration bias, as advocated by Fudenberg (2006). We directly test the driving force of the Kőszegi–Szeidl model—the assumption that differences in time attract attention. In doing so, we also explore whether and how our findings relate to other models of bounded rationality (especially, Bordalo, Gennaioli, and Shleifer, 2012; Bordalo, Gennaioli, and Shleifer, 2013; Bushong, Rabin, and Schwartzstein, 2017) and contribute to a more unified understanding of related empirical phenomena in the heuristics-and-biases literature (Kahneman, 2003a; Kahneman, 2003b).

Our consumption and money experiments are both based on comparisons between unbalanced and balanced choices to measure concentration bias. In the following, we primarily focus on the preregistered consumption experiment. We briefly summarise our money experiment at the end of this section and in Section 1.6.2 and provide a detailed description in Appendix 1.F.

1. Individuals may: (i) *over*work each day in a year to secure a concentrated bonus at the end of the year; (ii) *under*exercise by avoiding the concentrated hassle of going to the gym today, resulting in health impairments dispersed over future dates; (iii) *over*spend by avoiding attention-grabbing large lump-sum payments in favour of instalments to finance durable goods.

In the Main-Treatment condition of the consumption experiment, the comparison between balanced and unbalanced intertemporal choices is conducted within subjects. This allows us to measure concentration bias on the individual level. In each choice, options consist of: (i) a work plan that includes positive numbers of real-effort tasks on *eight* workdays; (ii) a compensation in the form of a non-cashable, personalized restaurant voucher that is valid for a *single* day after the last workday. In *balanced* choices, we elicit each subject's willingness to complete additional tasks on *one* workday in exchange for a *small* raise of the value of the restaurant voucher. In *unbalanced* choices, we elicit each subject's willingness to complete additional tasks on all *eight* workdays in exchange for a *large* raise of the voucher's value.

The choices are structured into one unbalanced choice block and eight balanced choice blocks. The eight balanced choice blocks constitute a decomposed version of the unbalanced choice block from the perspective of models of exponential and (quasi-)hyperbolic discounting. Consequently, standard discounting makes a point prediction regarding subjects' behavior in the unbalanced choice block: subjects should state the same willingness to complete additional tasks per workday in the unbalanced choice block as in the eight balanced choice blocks. The Kőszegi–Szeidl model, by contrast, predicts that subjects' willingness to work is greater in the unbalanced choice block than in the balanced choice blocks. In the balanced choice blocks, the Kőszegi–Szeidl model predicts, like standard discounting models, that only subjects' time preference and per-period utility functions determine their willingness to work. In the unbalanced choice block, however, subjects focus on, and hence overweight, the raise in the restaurant voucher that is concentrated in time. Relative to their individual time preferences and per-period utility function, subjects behave future-biased: they commit to too much work to redeem the more valuable restaurant voucher afterwards.

Our within-subject assessment of concentration bias in Main-Treatment does not rely on any assumptions regarding subjects' degree of exponential or (quasi-)hyperbolic discounting and on the curvature of their per-period utility function. However, the degree to which subjects' preferences are consistent between choice blocks is crucial. In the condition Main-Control, we provide a benchmark regarding subjects' consistency in our experimental setup. Mirroring the setup of Main-Treatment, we measure whether and how closely subjects implement the point prediction of standard discounting when concentration bias does not interfere. We identify the quantitative effect of concentration bias adjusted for potential inconsistencies by conducting between-subject comparisons of conditions Main-Treatment and Main-Control.

Our main results provide evidence for concentration bias in intertemporal choice. In Main-Treatment, when the workload is dispersed over eight workdays and the benefit of increasing effort provision is concentrated in a single day, subjects are willing to complete 23% more tasks per workday than is predicted by standard discounting. This positive deviation from the point prediction is consistent with concentration bias. In MAIN-CONTROL, when concentration bias does not interfere with the point prediction of standard discounting, subjects' deviation is relatively small and negative, −2%. Therefore, our between-subject treatment effect identifies a concentration bias of 25% that is statistically significant at all conventional levels. In additional analyses, we show: (i) our finding is robust to including individual-level controls in the analysis; (ii) potential noise in subjects' choices is unable to explain our findings; (iii) the results of two further between-subjects conditions, DONATION-TREATMENT and DONATION-CONTROL, replicate the evidence for concentration bias when using donations to a social cause instead of restaurant vouchers.

Behind the average behavior lies substantial heterogeneity between subjects. We explore this heterogeneity (as preregistered) by regressing concentration bias on three measures of cognitive skills (Cognitive Reflection Test, Frederick, 2005; Raven Progressive Matrices IQ test, Raven, 1941; and an arithmetic test) and on subjects' response times. We find (i) that better cognitive skills go along with a very small and statistically *in*significant reduction in concentration bias and (ii) that longer response times are accompanied by smaller concentration bias, which is statistically significant. Taken together, these correlations support the focusing interpretation of concentration bias: Concentration bias is unlikely the result of constraints in cognitive performance to deal with unbalanced intertemporal choices. Instead, concentration bias seems to result from spending less time and, hence, arguably paying less attention to the "decision problem" at hand.

According to the Kőszegi–Szeidl model, concentration bias in intertemporal choice results from concentrated utility outcomes in time. We conjecture that a complementary mechanism may be at play: accessibility (Kahneman, 2003a,b). Concentrated utility outcomes attract attention because they readily reveal their entirety and are hence easy to grasp; dispersed utility outcomes, by contrast, divert attention as they demand effortful mental aggregation to allow for a functional cognitive representation. In that sense, accessibility could simply be another interpretation of the Kőszegi–Szeidl model. However, accessibility and concentration in time do not have to go hand-in-hand. The framing of utility outcomes may affect accessibility without changing the underlying degree with which utility outcomes are concentrated in time. For instance, displaying the total magnitude of an instalment plan may increase the accessibility of the money-equivalent utility cost of the instalment plan, while leaving the degree of dispersion of the actual instalments (and their actual effect on utility) unaffected. By exploring accessibility as a factor that potentially contributes to concentration bias, we investigate whether concentration bias goes beyond the Kőszegi–Szeidl model.

We investigate (as preregistered) the relative contributions of concentration in time and of accessibility to concentration bias in two further between-subjects conditions of our consumption experiment. The MECHANISM-CONTROL condition consists of the same balanced choices as the MAIN-CONTROL condition, while the MECHANISM-TREATMENT condition consists of the same balanced and unbalanced choices as the Main-Treatment condition. Mechanism-Treatment and Mechanism-Control implement subtle changes vis-à-vis their Main counterparts in how the numbers of real-effort tasks and the values of the restaurant voucher are displayed to subjects. These display changes affect only the accessibility of the utility benefit and the cost of increasing effort provision. The actual timing of the utility outcomes is left unchanged: the utility benefit in the form of the restaurant voucher is still concentrated on a single day, while the utility cost is still dispersed over eight workdays. Therefore, the Kőszegi–Szeidl model—based on the assumption that differences in time attract attention—predicts the same treatment effects for Mechanism-Treatment and Mechanism-Control as for Main-Treatment and Main-Control.

The display changes decrease the accessibility of the utility benefit and increase the accessibility of the utility cost. We achieve the former by displaying the raise in the value of the restaurant voucher as a disaggregated sum instead of stating the highly accessible outcome of the sum like in Main-Treatment,[2] and we achieve the latter by directly stating the per-workday difference in the number of real-effort tasks between options. In Main-Treatment, we merely stated both absolute numbers side-by-side, which requires subjects to calculate differences before the per-workday change is accessible. If concentration bias is driven at least partly by differential accessibility, reducing the accessibility of the utility benefit while increasing the accessibility of the utility costs should diminish concentration bias. We hence expect a smaller treatment effect in the Mechanism-Treatment and Mechanism-Control conditions than in the Main-Treatment and Main-Control conditions.

The treatment effect of the Mechanism-Treatment and Mechanism-Control conditions yields a concentration bias of 9% that is statistically significant at all conventional levels, yet smaller than the treatment effect of the Main-Treatment and Main-Control conditions. This difference in treatment effects is significant at all conventional levels and robust to the inclusion of individual-level controls. The difference-in-differences analysis suggests that 38% of the observed overall concentration bias relates to concentration in time and that 62% relates to accessibility.

In our money experiment, we corroborate and extend the results of the consumption experiment. While the consumption experiment only pertains to concentration bias leading to future-biased intertemporal choices, the money experiment shows that concentration bias can induce both future bias and present bias. Moreover, the money experiment yields similar implications regarding the mechanisms behind concentration bias. We find that concentration in time and accessibility explain, respectively, roughly 60% and 40% of the concentration bias measured in our money experiment. Overall, we interpret our results as evidence that both concen-

2. That is, the raise is displayed as €7.50 + €2.90 + €2.70 + €3.10 + …+ €3.20 instead of €31.10.

tration in time and accessibility are important determinants of concentration bias and that their relative contributions may depend on the exact context.

In summary, our central contribution is an experimental analysis of whether and how the degree to which utility outcomes are concentrated in time affects intertemporal choice. We find evidence for the Kőszegi–Szeidl model and its central prediction of concentration bias. Our investigation of the mechanisms behind concentration bias reveals that at least two forces contribute to producing concentration bias: differences in time attract attention, and accessibility of outcomes attracts attention. Thus, we provide evidence for the main assumption of the Kőszegi–Szeidl model and at the same time enrich the understanding of concentration bias by relating it to accessibility and, thereby, to other empirical phenomena described in the literature on cognitive heuristics and biases (Kahneman, 2003a,b). Consistent with our findings, Kahneman (2003a,b) declares in his discussion of extension neglect and prototype heuristics that assessments which require mental aggregation are low in accessibility to individuals in various judgement and decision-making tasks unrelated to intertemporal choice and often give rise to systematic biases.

Our results also shed some light on the assumptions of other related models of bounded rationality. While Rubinstein (1988) and Rubinstein (2003), like Kőszegi and Szeidl (2013), assumes that larger differences in some attribute of a multi-attribute choice receive larger decision weights, Bushong, Rabin, and Schwartzstein (2017) assume the opposite: changes in an attribute draw attention to that attribute if its range is smaller rather than greater. Salience theory (Bordalo, Gennaioli, and Shleifer, 2012, 2013), in turn, assumes that larger differences make an attribute more salient but that there is diminishing sensitivity in salience as well. Apart from the clear relation to the differences-attract-attention assumption, these models of bounded rationality, however, differ from the Kőszegi–Szeidl model in other aspects such that they do not readily provide clear predictions for our context.[3] In as much as we find that differences in time attract attention, our results are not compatible with the assumption stated in Bushong, Rabin, and Schwartzstein (2017), yet are compatible with the assumption that Kőszegi and Szeidl (2013) shares with Rubinstein (1988, 2003) and with versions of salience theory (Bordalo, Gennaioli, and Shleifer, 2012, 2013) in which diminishing sensitivity is relatively weak. In addition, our findings relate to the theoretical literature that studies the implications of the differences-attract-attention assumption for economic application, e.g., in industrial organization (Dertwinkel-Kalt, Köster, and Peiseler, 2019) and political economy (Nunnari and Zápal, 2017).

Our results also relate to several empirical literatures: (i) Closest to us, Kettle, Trudel, Blanchard, and Häubl (2016) study whether a concentrated or dispersed

3. For instance, Bushong, Rabin, and Schwartzstein (2017) assume that individual's mental representation of intertemporal choices leads them to collapse future periods into an average future period against which the presence is compared.

framing of debt reduction causally affects effort provision in the lab. Subjects have multiple induced debt accounts and provide more effort to reduce debt when the reduction is displayed account-wise than when the reduction is displayed dispersed over multiple accounts. Our findings contribute by showing that such accessibility effects of a concentrated framing play also an important role for intertemporal choice (involving money as well as consumption trade-offs). In addition, our findings show behavioral consequences of concentration bias based on concentration in time that goes beyond accessibility effects.

(ii) The managerial literature shows that goals can have various adverse consequences, such as unwanted risk taking and unethical behaviors (Ordóñez, Schweitzer, Galinsky, and Bazerman, 2009). Our results contribute in suggesting that the adverse consequences of goals are more prevalent when the utility benefit of the goal is concentrated and its costs are dispersed (over time or in terms of reduced accessibility).

(iii) Recent lab studies find little to none evidence for present bias (Andreoni and Sprenger, 2012), while field studies typically find substantial present bias (DellaVigna and Malmendier, 2006; Paserman, 2008; Laibson, Maxted, Repetto, and Tobacman, 2018). DellaVigna (2018) argues that this may be driven by the use of monetary payments in most lab studies in contrast to real consumption in the field. Indeed, Augenblick, Niederle, and Sprenger (2015) find greater degrees of present bias in the lab with real-effort tasks than when using monetary incentives. This provides evidence for pure self-control-based present bias. Our evidence suggests that concentration bias may additionally contribute to the lab–field disparity: Choices in previous lab studies have been exclusively balanced, while choices in the field are often unbalanced with costs of present-biased behavior being dispersed over many periods.

(iv) We also contribute to the understanding of the annuity puzzle: Many people prefer a concentrated lump-sum payment over an annuity with a substantially higher expected present value. Besides the various explanations that have been discussed in the literature Yaari (see, e.g., 1965), Warner and Pleeter (2001), Davidoff, Brown, and Diamond (2005), and Benartzi, Previtero, and Thaler (2011), concentration bias may be a complementary factor according to our findings.

We proceed in Section 1.2 with a brief overview of the Kőszegi–Szeidl model and discuss the design of our consumption experiment in Section 1.3. We present our main results in Section 3.5. Our investigation of the mechanisms behind concentration bias follows in Section 1.5. We present the evidence from the Donation conditions and the money experiment in Section 1.6. Section 1.7 discusses potential policy implications and concludes.

1.2 Focusing

This section introduces the focusing model by Kőszegi and Szeidl (2013), on which our experimental investigation of concentration bias is based. Our exposition builds on the following exemplary intertemporal choice. Consider an employee who can decide to work extra time each day of the year to earn a bonus or not to work extra time, forgoing the bonus. She has to exert baseline effort $\boldsymbol{e} = (e_1, \ldots, e_{T-1})$ as part of her contract which, in return, pays her a consumption-equivalent compensation $\underline{v}$ in period T. The employee can decide to work $\boldsymbol{w} = (w_1, \ldots, w_{T-1})$ on top of the mandatory effort $\boldsymbol{e}$ to receive $\overline{v} = \underline{v} + b$, which is the consumption-equivalent of the sum of her baseline compensation and the bonus b in period T. Assume that for each case she plans to consume $\overline{v}$ or $\underline{v}$, respectively, immediately upon receipt.

She decides by comparing the aggregate utility of each option $\boldsymbol{c}$ in the choice set $\boldsymbol{C}$. Here, $\boldsymbol{c}$ is the consumption profile over all periods t, with c_t being the t^{th} entry. It collects all workloads, which enter negatively, as well as the compensation, which enters positively. In the case that the employee decides to work extra time, $\boldsymbol{c} = (-e_1 - w_1, \ldots, -e_{T-1} - w_{T-1}; \overline{v}_T)$. We assume that $u'(c) > 0$, that is, u is decreasing in labour and increasing in the compensation.

Standard models of exponential and (quasi-)hyperbolic discounting assume that intertemporal utility is additively separable, with per-period utility weighted depending on the time distance of the period. Hence, consumption utility at a future date t can be expressed as $u_t(c_t) := D(t)\, u(c_t)$.[4] Here, $D(t)$ denotes the employee's discount function. Under discounted utility, the present-valued utility of any option $\boldsymbol{c}$ is given by $U(\boldsymbol{c}) := \sum_{t=1}^{T} u_t(c_t) = \sum_{t=1}^{T} D(t)\, u(c_t)$.

Kőszegi and Szeidl (2013) maintain the assumption of additively separable utility of standard discounting and add period-t focus weights g_t that scale period-t consumption utility u_t. The employee is assumed to maximize focus-weighted utility, which is defined as

$$\tilde{U}(\boldsymbol{c}, \boldsymbol{C}) \quad := \quad \sum_{t=1}^{T} g_t(\boldsymbol{C})\, u_t(c_t).$$

In contrast to discounted utility $U(\boldsymbol{c})$, whose only argument is $\boldsymbol{c}$, focus-weighted utility $\tilde{U}(\boldsymbol{c}, \boldsymbol{C})$ has two arguments: the option $\boldsymbol{c}$ and the choice set $\boldsymbol{C}$. The latter dependence arises through the weights g_t. These are given by a strictly increasing weighting function g that takes as its argument the difference between the maximum and the minimum attainable utility in period t over all possible options in set $\boldsymbol{C}$:

$$g_t(\boldsymbol{C}) \;:=\; g[\Delta_t(\boldsymbol{C})] \quad \text{with} \quad \Delta_t(\boldsymbol{C}) \;:=\; \max_{c \in \boldsymbol{C}} u_t(c_t) - \min_{c \in \boldsymbol{C}} u_t(c_t).$$

4. Allowing $u(c)$ to take on different shapes in the negative and positive domain ensures that labour and compensation can be measured on different scales. Using $u(c)$ for both hence comes without loss of generality.

That is, focused thinkers put the more weight on a period t, the larger the utility range spanned by the best and worst alternative in that period.

The utility differences for the first $T-1$ periods are thus given by $\Delta_t(\boldsymbol{C}) = u_t(-e_t) - u_t(-e_t - w_t)$. Each difference might be relatively small compared to the utility range in period T, which is $\Delta_T(\boldsymbol{C}) = u_T(\underline{v} + b) - u_T(\underline{v})$. If the weighting function g is sufficiently steep, this leads to an overweighting of the last period, T, and thus to a disproportionate focus on the bonus, causing the individual to behave future-biased by committing to too much extra work relative to her time preferences and per-period utility function as captured in standard discounting.

In so-called *unbalanced choices*, like the example above, utility in some periods is traded off with utility in *a different number* of periods. Here, focusing distorts choices towards alternatives with concentrated benefits. In the example above, the utility benefit of committing to too much extra work is concentrated in time and arises *after* the dispersed costs of working extra time. This leads to concentration-bias–induced future-biased behavior. If, however, concentrated utility benefits arise before their dispersed costs, concentration bias can also lead to present-biased behavior.

An important implication of the focusing model is that concentration bias is not predicted in *balanced* pairwise choices. In a *balanced* choice, utility in some periods is traded off with utility in *the same number* of different periods. Focusing merely amplifies any utility difference that is also present according to standard discounted utility, so that the focusing model and standard discounted utility predict the same decision for balanced pairwise choices (see Proposition 3 of Kőszegi and Szeidl, 2013).

1.3 Experimental Design

In order to identify concentration bias in intertemporal choice and provide for a robust quantitative measure of concentration bias, our consumption experiment combines within-subject and between-subject comparisons of intertemporal choices:

(i) We employ a within-subject comparison of balanced choices—for which the focusing model and standard discounting models make the same prediction—and unbalanced choices—for which the focusing model and standard discounting models make different predictions—in condition MAIN-TREATMENT. The balanced choices provide a benchmark of each subject's time preferences and per-period utility function against which a comparison with the unbalanced choices measures concentration bias on the individual level. Our within-subject comparison hence assesses concentration bias without relying on assumptions regarding subjects' particular degree of exponential or (quasi-)hyperbolic discounting and per-period utility function.

(ii) We employ a between-subject comparison between conditions MAIN-TREATMENT and MAIN-CONTROL to adjust our within-subject measure of concentration bias for potential within-subject inconsistencies. Our assessment of concentration bias in Main-Treatment may over- or underestimate if subjects' preferences are inconsistent between balanced and unbalanced choice blocks. In Main-Control, we provide for a benchmark of consistency in a within-subject measurement of subjects' consistency when concentration bias does not interfere. In our between-subject comparison of our within-subject measure of concentration bias from Main-Treatment with our within-subject measure of consistency from Main-Control, we hence identify the quantitive effect of concentration bias in our experimental setup.

1.3.1 Condition Main-Treatment

General characteristics. Subjects make multiple pairwise intertemporal choices between two options, which are called "A" and "B" in every trial. Each option consists of: (i) a work plan that includes strictly positive numbers of real-effort tasks on eight workdays; (ii) a strictly positive non-cashable and personalized restaurant voucher that is valid on a single day after the final workday.[5] In each choice, option A involves a higher value of the restaurant voucher than option B. In balanced choices, option A differs from option B in the number of real-effort tasks on *one* workday. In unbalanced choices, to the contrary, option A differs from option B on all *eight* workdays.

The intertemporal choices are grouped into nine choice blocks, with each block consisting of multiple choices, see Table 1.1. The first eight choice blocks involve balanced choices and the last choice block involves unbalanced choices. Figure 1.1 displays an exemplary choice from one of the eight balanced choice blocks, and Figure 1.2 displays an exemplary choice from the unbalanced choice block.

Balanced choice blocks. In each balanced choice block $j \in \{1, 2, \dots, 8\}$, option B of each individual choice requires subjects to complete the baseline work plan $\boldsymbol{e} = (e_1, \dots, e_8)$ on workdays $t \in \{1, 2, \dots, 8\}$ in exchange for a restaurant voucher of value v^j. The baseline work plan consists of a positive number of mandatory real-effort tasks e_t on each workday t. Each option A in choice block j requires subjects to work on w_t tasks on a single workday $t = j$ in addition to the mandatory tasks. In return, option A grants an increase b^j in the value of the restaurant voucher, so that the voucher associated with A is worth $v^j + b^j$.[6] We henceforth refer to the additional real-effort tasks w_t simply as "tasks".

5. The number of days involving utility consequences are constant across all options to rule out that differences in potentially associated transaction costs affect subjects' choices differentially (Augenblick and Rabin, 2019).

6. The mandatory work plan $\boldsymbol{e}$ requires subjects to complete a different number of real-effort task per workday (between 115 and 143). The increases of the voucher values vary across choice blocks between €2.70 and €3.20. Both slight variations are included to make each choice block different from the previous one, with the objective that participants would think anew about each decision. We chose to have a rather high number of mandatory tasks so that transaction costs for completing all

Table 1.1. Choice blocks in the conditions MAIN-TREATMENT and MAIN-CONTROL

Choice block	Option	Real-effort tasks on day t								Voucher value
		$t=1$	$t=2$	$t=3$	$t=4$	$t=5$	$t=6$	$t=7$	$t=8$	$t=9$
Both conditions:		Balanced choice blocks								
$j=1$	B^1	e_1	e_2	e_3	e_4	e_5	e_6	e_7	e_8	v^1
	A^1	$e_1 + w_1$	e_2	e_3	e_4	e_5	e_6	e_7	e_8	$v^1 + b^1 = v^2$
$j=2$	B^2	e_1	e_2	e_3	e_4	e_5	e_6	e_7	e_8	v^2
	A^2	e_1	$e_2 + w_2$	e_3	e_4	e_5	e_6	e_7	e_8	$v^2 + b^2 = v^3$
$j=3$	B^3	e_1	e_2	e_3	e_4	e_5	e_6	e_7	e_8	v^3
	A^3	e_1	e_2	$e_3 + w_3$	e_4	e_5	e_6	e_7	e_8	$v^3 + b^3 = v^4$
$j=4$	B^4	e_1	e_2	e_3	e_4	e_5	e_6	e_7	e_8	v^4
	A^4	e_1	e_2	e_3	$e_4 + w_4$	e_5	e_6	e_7	e_8	$v^4 + b^4 = v^5$
$j=5$	B^5	e_1	e_2	e_3	e_4	e_5	e_6	e_7	e_8	v^5
	A^5	e_1	e_2	e_3	e_4	$e_5 + w_5$	e_6	e_7	e_8	$v^5 + b^5 = v^6$
$j=6$	B^6	e_1	e_2	e_3	e_4	e_5	e_6	e_7	e_8	v^6
	A^6	e_1	e_2	e_3	e_4	e_5	$e_6 + w_6$	e_7	e_8	$v^6 + b^6 = v^7$
$j=7$	B^7	e_1	e_2	e_3	e_4	e_5	e_6	e_7	e_8	v^7
	A^7	e_1	e_2	e_3	e_4	e_5	e_6	$e_7 + w_7$	e_8	$v^7 + b^7 = v^8$
$j=8$	B^8	e_1	e_2	e_3	e_4	e_5	e_6	e_7	e_8	v^8
	A^8	e_1	e_2	e_3	e_4	e_5	e_6	e_7	$e_8 + w_8$	$v^8 + b^8 = v^9$
MAIN-TREATMENT:		Unbalanced choice block, based on blocks 1–8								
$j=9$	B^9	e_1	e_2	e_3	e_1	e_5	e_6	e_7	e_8	v^1
	A^9	$e_1 + w_1^9$	$e_2 + w_2^9$	$e_3 + w_3^9$	$e_1 + w_4^9$	$e_5 + w_5^9$	$e_6 + w_6^9$	$e_7 + w_7^9$	$e_8 + w_8^9$	v^9
MAIN-CONTROL:		Balanced choice block, based on block 1, 4, or 8—for instance,								
$j=9$	B^9	e_1	e_2	e_3	e_1	e_5	e_6	e_7	e_8	v^4
	A^9	e_1	e_2	e_3	$e_4 + w_4^9$	e_5	e_6	e_7	e_8	$v^4 + b^4 = v^5$

Notes: This table presents a summary of the nine choice blocks used in conditions MAIN-TREATMENT and MAIN-CONTROL. Subjects first complete the balanced choice blocks 1–8 in random order. Each balanced choice block j stands for multiple choices between which w_t, $t=j$, is varied according to the set of integers in the range $[0, 125]$. This permits determining in each balanced block j the value $w_t^\star$, $t=j$, for which a participant is indifferent between choosing A^j and B^j. The set of alternatives w_t^9 in choice block 9 is constructed by adding the same integer $k \in [-63, +62]$ to $w_t^\star$—that is, $w_t^9 \in \{w_t^\star - 63, \ldots, w_t^\star, \ldots, w_t^\star + 62\}$—on each workday t included in A^9. This permits determining the values $w_t^{9\star}$ for which a participant is indifferent between A^9 and B^9 and, ultimately, comparing $w_t^{9\star}$ with $w_t^\star$.

tasks would be equalized between options A and B as much as possible. The lowest attainable voucher value is $v_1 = €7.50$, which ensures that subjects can purchase a main course at the restaurant.

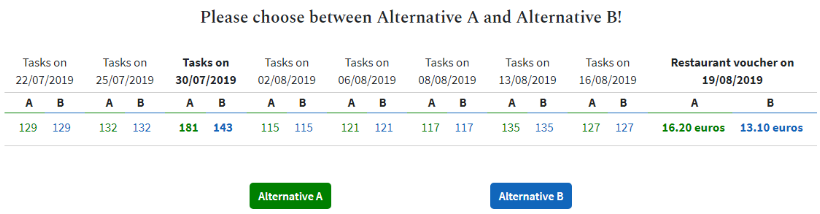


Figure 1.1. Screenshot of a decision screen for a balanced decision in the MAIN-TREATMENT condition.

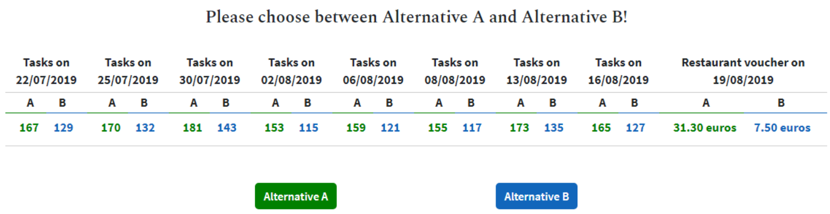


Figure 1.2. Screenshot of a decision screen for an unbalanced decision in the MAIN-TREATMENT condition.

The number of tasks required by A varies across trials within each balanced choice block, $w_t \in \{0, 1, 2, \ldots, 125\}$. This allows us to elicit subjects' indifference points $w_t^\star$: the maximum number of tasks w_t that they are willing to complete on t in exchange for the value of the restaurant voucher going up from v^j to $v^j + b^j$. We collect these indifference points from the balanced choices in the vector $\boldsymbol{w}^\star = (w_1^\star, \ldots, w_8^\star)$.

Subjects complete the eight balanced choice blocks in random order, which is explained below. They differ from each other in the following aspects: (i) the workday $t = j$ on which the tasks w_t arise, and (ii) the baseline voucher value v^j. The baseline voucher value is always given by $v^j = v^{j-1} + b^{j-1}$; that is, the voucher value of option B in choice block j is equal to the voucher value of option A in choice block $j - 1$. These features are critical for our design, since they link the indifference points from the balanced choice blocks and from the unbalanced choice block, as is discussed below.

Note that we did *not* set up the balanced choice blocks to estimate utility parameters such as the curvature of per-period utility and the strength of discounting. We use the balanced choice blocks "merely" to serve as a benchmark of each subject's time preferences and per-period utility functions against which we can measure the quantitative effect of concentration bias, as is discussed below.

Unbalanced choice block. In all trials of the unbalanced choice block, option B requires subjects to complete the baseline work plan in exchange for a restaurant voucher of value v^1, the *lowest* voucher value attainable across all choice blocks. Each option A in the unbalanced choice block, in turn, requires subjects to work on

$\boldsymbol{w}^9 = (w_1^9, \ldots, w_8^9)$ tasks in exchange for receiving a restaurant voucher of value v^9, the *highest* attainable value across all balanced choice blocks.

Within the unbalanced choice block, we vary $\boldsymbol{w}^9$ across trials, in steps of one task per workday. This allows us to elicit a subject's indifference point $\boldsymbol{w}^{9\star}$: the maximum number of task w_t^9 she is willing to complete at each t in exchange for receiving the most valuable restaurant voucher, v^9.

Link between the balanced choice blocks and the unbalanced choice block. We designed the balanced and unbalanced choices blocks to be linked on the individual level such that each subject can implement: (i) exactly the same per-workday indifference point in the unbalanced choice block as in the eight balanced choice blocks, that is, $\boldsymbol{w}^{9\star} = \boldsymbol{w}^\star$; (ii) a greater willingness to work in the unbalanced choice block than in the balanced choice blocks, $\boldsymbol{w}^{9\star} > \boldsymbol{w}^\star$; (iii) a lower willingness to work in the unbalanced choice block than in the balanced choice blocks, $\boldsymbol{w}^{9\star} < \boldsymbol{w}^\star$.

We achieve this individual link between choice blocks, by letting the balanced choice blocks precede the unbalanced choice block and by varying $\boldsymbol{w}^9$ in the unbalanced choice block across trials such that

$$w_t^9 \quad \in \quad \{w_t^\star - 63,\ \ldots,\ w_t^\star - 1,\ \ w_t^\star,\ \ w_t^\star + 1,\ \ldots,\ w_t^\star + 62\},$$

that is, for each workday t, the variation of w_t^9 is centred around each subject's own indifference point $w_t^\star$. Note also that by construction, each subjects' deviation is one value that applies to all eight workdays in condition Main-Treatment and to one workday in Main-Control.

Our particular implementation of linking balanced and unbalanced choices on the individual level has the following implications:

(i) By construction of the centring, each subjects' deviation from the point prediction yields a constant value d that applies to all eight workdays, that is, $\boldsymbol{w}^{9\star} - \boldsymbol{w}^\star = (d, \ldots, d)$.
(ii) Subjects have complete leeway to implement their point prediction, $d = 0$, or deviate in either direction from the point prediction of standard discounting, $d > 0$ and $d < 0$.
(iii) The centring avoids that the across-subject average of the within-subject comparison between balanced and unbalanced choices can be skewed by noise in participants' responses in either direction.
(iv) We rule out that our centring leads to "middle option bias". Choices were *not* displayed in the form of choice lists that would contain "middle options". As is discussed in greater detail in Section 1.B.1 of Section 1.B, subjects instead make each pairwise choice on a separate decision screen (see Figures 1.1 and 1.2) in a nonmonotone order.
(v) In order to ensure incentive compatibility for the first eight choice blocks, we followed Halevy, Persitz, and Zrill (2018) in not making subjects aware

that their choices in the first eight choice blocks influenced the bounds $[w_t^\star - 63, w_t^\star + 62]$ in-between which w_t^9 was varied in the final choice block.

(vi) By construction, our way of linking balanced and unbalanced choice blocks rests on subjects actually stating indifference points in the first eight choice blocks. We hence preregistered to exclude all subjects with at least one corner choice, that is always preferring A or B in a choice block, from our sample.[7]

Predictions. This individual-specific link between balanced and unbalanced choices allows us to provide a within-subject assessment of concentration bias: We show below that standard discounting predicts subjects' indifference points in the unbalanced choice block to be equal to their indifference points in the balanced choice blocks, that is, $\boldsymbol{w}^{9\star} = \boldsymbol{w}^\star$ or $d = 0$. However, from the perspective of the focusing model, as we show below, subjects are predicted to increase their willingness to provide effort in the unbalanced choice block relative to their eight balanced indifference points, that is, $\boldsymbol{w}^{9\star} > \boldsymbol{w}^\star$ or $d > 0$.

All decisions used to elicit the indifference points in the eight balanced trade-offs have the same underlying structure: Each binary choice belonging to block $j \in \{1, \ldots, 8\}$ is between option $A^j = (e_1, \ldots, e_j + w_j, \ldots, e_8; v^{j+1})$ and option $B^j = (e_1, \ldots, e_8; v^j)$. It holds that $w_j \geq 0$ and $v^{j+1} > v^j$.

According to Proposition 3 of Kőszegi and Szeidl (2013), focusing lets a decision maker choose in balanced pairwise decisions option A over option B whenever she would do the same under discounted utility, that is, if and only if $u_j(-e_j - w_j) + u_9(v^{j+1}) > u_j(-e_j) + u_9(v^j)$. The reason is that in this balanced trade-off the two options span the per-period range of utilities for all periods. The utility difference is larger for the period with the greater advantage, and focusing merely amplifies this advantage of the preferred option via the associated focus weight.

As a consequence, focusing does not alter the indifference point of the individual: At indifference, the focus weights of the two periods involved in the pairwise balanced decision are of identical size for both periods. Hence, they exactly cancel each other, so that indifference coincides for discounted utility and focusing in balanced pairwise choices. A more detailed derivation of this statement can be found in Section 1.A.1 of Section 1.A. This gives us the following corollary:

Corollary 1. *Standard discounted utility models and the focusing model predict the same indifference points in the balanced choice blocks.*

We denote by $w_t^\star$ the number of tasks that makes an individual indifferent between options A and B in a balanced decision. That is, $w_t^\star$ for $t = 1, \ldots, 8$ are the

7. As described in Section 1.B.2, we asked subjects with upper corner choices in the first eight balanced choice blocks to hypothetically state their indifference points manually. We then constructed the centring in the unbalanced choice block partly based on these hypothetical values. In Appendix 1.D we show that our results are virtually the same when including subjects with corner choices.

workloads that fulfil the condition

$$u_t(-e_t - w_t^\star) + u_9(v^{t+1}) \quad = \quad u_t(-e_t) + u_9(v^t).$$

In the final, unbalanced decision block, individuals trade off greater effort in all eight periods simultaneously with a more valuable restaurant voucher in the ninth period. Here, individuals choose between options

$$\begin{aligned} \mathrm{A}^9 \quad &= \quad (e_1 + w_1^9,\ e_2 + w_2^9,\ e_3 + w_3^9,\ e_4 + w_4^9,\ e_5 + w_5^9,\ e_6 + w_6^9,\ e_7 + w_7^9,\ e_8 + w_8^9;\ v^9) \\ \text{and} \quad \mathrm{B}^9 \quad &= \quad (e_1,\ e_2,\ e_3,\ e_4,\ e_5,\ e_6,\ e_7,\ e_8;\ v^1). \end{aligned}$$

According to discounted utility, individuals should be indifferent between options A and B in the unbalanced decision block for the exact same amounts $w_t^\star$, $t = 1, \dots, 8$, as in the balanced decisions. This is because standard discounted utility assumes total utility to be additively separable over time. This assumption implies that the eight balanced choice blocks are simply a stepwise, decomposed elicitation of the same information that is elicited in the unbalanced choice block. A formal derivation of this prediction is provided in Section 1.A.2 of Section 1.A. We therefore obtain the following corollary:

Corollary 2. *Under discounted utility, subjects choose the same indifference point in the unbalanced choice block as in the balanced blocks:* $\boldsymbol{w}^{9\star} = \boldsymbol{w}^\star$ *or* $d = 0$.

The focusing model, by contrast, predicts that $w_t^{9\star} > w_t^\star$ for $t = 1, \dots, 8$. The formal derivation of this prediction is provided in Section 1.A.3 of Section 1.A. Explained verbally, the effect comes about because the trade-off is unbalanced in a particular way: Take the point prediction of standard discounting, $w_t^{9\star} = w_t^\star$, as the point of departure. Compared to the first eight choice blocks, the utility range in the unbalanced block increases in period $t = 9$, while it remains the same on the eight workdays between choice blocks. Hence, the concentrated consequence at date $t = 9$ receives a larger focus weight, while the focus weights are unchanged for the first eight periods. This gives rise to our prediction that in condition Main-Treatment:

Prediction 1. Subjects choose a higher indifference point in the unbalanced choice block than in the balanced choice blocks: $\boldsymbol{w}^{9\star} > \boldsymbol{w}^\star$ or $d > 0$.

1.3.2 Condition Main-Control

Under the assumption that subjects' preferences as captured in standard discounting are consistent, the within-subject comparison between the unbalanced choice block and the eight balanced choice blocks in condition Main-Treatment would suffice to identify concentration bias and to measure its strength. However, if subjects' time preference or per-period utility function would suddenly be different in the unbalanced choice block compared to the other choice blocks, our within-subject assessment may under- or overestimate concentration bias. In order to correct our within-subject measure of concentration bias for such potential inconsistencies, we devised the between-subject condition MAIN-CONTROL to provide a benchmark of subjects' scope for inconsistencies in our particular experimental setup.

Design. In the MAIN-CONTROL condition, subjects first complete the balanced choice blocks in random order like in MAIN-TREATMENT. Subjects then complete either choice block 1, 4, or 8 a second time. In this last balanced choice block, we vary the number of tasks in the same way as in the unbalanced choice block of MAIN-TREATMENT: subjects can deviate from the indifference point $w_t^\star$ elicited in block 1, 4, or 8 in both directions, again by $[-63, +62]$. Both the focusing model and standard discounting models predict that participants should be indifferent between option A and option B in this repeated block for the same workload as in the first iteration. This allows us to directly isolate potential inconsistencies in subjects' willingness to work for receiving a more valuable restaurant voucher.

Stratified assignment to conditions. Assignment to the two conditions is randomised within-session under the following restriction: We stratify assignment based on the average number of tasks chosen in the balanced choice blocks. All participants who fulfil the inclusion criterion, see below, are ranked according to the average number of tasks chosen in blocks 1–8, and pairs of neighbouring ranks are formed for whom conditions are randomly and mutually exclusively assigned. This is done to make subjects as comparable as possible between the two conditions.

1.3.3 Outcome variables and hypothesis

Both conditions test for within-subject deviations from the point prediction of standard discounting. In condition MAIN-TREATMENT, this deviation pertains to how many tasks subjects are willing to work more or less on each of the eight workdays—as elicited in the unbalanced choice block—in comparison to the point prediction of standard discounting. The focusing model predicts deviations to be positive, see Prediction 1. In condition MAIN-CONTROL, the deviation pertains to how many tasks subjects are willing to work more or less on one workday—as elicited in the repeated balanced choice block—in comparison to the point prediction of standard discounting. Deviations in condition MAIN-CONTROL show how severe within-subject inconsistencies are in our experimental setup and allow us to assess whether condition MAIN-TREATMENT over- or underestimates concentration bias.

We state our hypothesis in terms of the individual outcomes that we use to analyse the data. Recall that by construction, each subjects' deviation is one value that applies to all eight workdays in condition MAIN-TREATMENT and to one workday in MAIN-CONTROL. In comparing conditions, we hence simply use the per-workday deviations, in particular, (i) the absolute per-workday deviation and (ii) the average relative per-workday deviation or "ARD". The former is captured in d, the raw deviation in tasks per-workday. The latter is formally for each subject:

$$\text{ARD} \quad := \quad \frac{1}{8}\sum_{t=1}^{8}\frac{w_t^{9\star} - w_t^{\star}}{e_t + w_t^{\star}} \quad = \quad \frac{1}{8}\sum_{t=1}^{8}\left(\frac{e_t + w_t^{9\star}}{e_t + w_t^{\star}} - 1\right), \qquad (1.1)$$

that is, we calculate for each workday the relative deviation of a subject's indifference point in the last choice block from the associated indifference point in the respective balanced choice block, taking the mandatory real effort tasks into account. We then average over all workdays.[8]

We hypothesize that subjects display concentration bias in their choice, that is d and ARD are positive in MAIN-TREATMENT and, importantly greater than potential inconsistencies measured in MAIN-CONTROL. We hence state the following hypothesis:

Hypothesis 1. *The deviation from the point prediction of standard discounting in MAIN-TREATMENT is (i) positive and (ii) greater than in MAIN-CONTROL, both in terms absolute per-workday deviations, d, and average relative per-workday deviations, ARD.*

1.3.4 Protocol

Prior to completing the nine choice blocks, subjects received computerized instructions, gained experience in working on the real-effort task, completed comprehension questions, made multiple practice choices, and were told the price ranges of different product categories that can be purchased at the restaurant for which the voucher was valid.

Additionally, subjects chose their preferred individual 8 workdays from a set of 20 dates and one day from a set of seven dates on which the restaurant voucher would be valid. The 20 dates for working comprised all business days during the four weeks following the week in which the experiment was conducted. The dates for visiting the restaurant comprised the seven days of the week after the last potential workday.[9]

After the nine choice blocks, subjects answered a questionnaire regarding demographic information, which are used as control variables in our regression analyses. Subjects then completed three additional tasks related to their cognitive abilities (i.e., Raven Progressive Matrices IQ test (Raven, 1941), an arithmetic test, and Cognitive Reflection Test (Frederick, 2005)) that we used both as control variables and to investigate heterogeneity.[10]

8. Instead of stating the average relative per-workday deviation, we could also state the relative aggregated deviation, RAD $:= \left(\sum_{t=1}^{8}(e_t + w_t^{9\star})\right)\Big/\left(\sum_{t=1}^{8}(e_t + w_t^{\star})\right)$. Our results are virtually the same for this alternative measure and average relative per-workday deviation, see Appendix 1.E.

9. Subjects selected, on average, their first workday to be 6.65 days after the experiment in MAIN-TREATMENT and 6.5 days in MAIN-CONTROL. The average distance between workdays, including weekends, was 1.84 days in MAIN-TREATMENT and 1.92 days in MAIN-CONTROL. The average distance between the last workday and the day for which the restaurant voucher is valid is 15.73 days in MAIN-TREATMENT and 15.34 in MAIN-CONTROL. All these differences are small and insignificant.

10. In the arithmetic test, subjects had 8 minutes to calculate the sum of 8 distinct numbers in as many trials as possible out of 20. For each 5 correct sums, they received €1.

Each real-effort task consists of transcribing a sequence of six numbers into a sequence of six letters, see Figure 1.C.1 in Appendix 1.C. Participants did not have to return to the lab for completing their work plan but worked on the real-effort tasks online.

All subjects received €10 in cash, plus their earnings from the arithmetic task (on average €0.94). On top of this, three subjects per session (with a size of 27–32 participants) were randomly determined at the end of each session, following Attema, Bleichrodt, Gao, Huang, and Wakker (2016). These three subjects were required to implement the work plan associated with their choice in the decision that counts. After completion of their entire work plan, these subjects received their restaurant voucher that resulted from their choice in the decision that counts as well as an additional lump-sum compensation of €100, which they knew in advance. Attrition was very low: not a single subject failed to complete their work plan in conditions MAIN-TREATMENT and MAIN-CONTROL.

Each session lasted up to 75 minutes. Subjects were invited using ORSEE (Greiner, 2015), and the experiment was programmed in oTree (Chen, Schonger, and Wickens, 2016).

1.3.5 Preregistration

The consumption experiment was preregistered in the AEA RCT registry. The preregistration includes (i) the design of the conditions MAIN-CONTROL and MAIN-TREATMENT described in Section 1.3.2; (ii) the design of the conditions MECHANISM–CONTROL and MECHANISM-TREATMENT discussed in Section 1.5; (iii) an analysis plan, including the heterogeneity and robustness analyses reported in Section 3.5; (iv) the sample size of $N = 100$ for each condition as well as the exclusion restriction. The pre-registration is available at https://doi.org/10.1257/rct.4446. A separate preregistration was filed for the conditions DONATION-CONTROL and DONATION-TREATMENT and is available at https://doi.org/10.1257/rct.4341.

1.4 Results

1.4.1 Evidence for concentration bias

1.4.1.1 Preliminaries

Before we present our main results, we inspect behavior in the first eight, balanced choice blocks. Recall that we did *not* set up the balanced choice blocks to estimate utility parameters, but to serve as a benchmark of each subject's time preferences and per-period utility functions against which we can measure concentration bias. We hence limit our summary to subjects' average behavior across choice blocks.

Subjects are willing to increase their workload by 37 real-effort tasks on average on one workday in exchange for an average increase of €3 in the value of the restaurant voucher. Split by conditions, the mean values are 38 tasks in Main-Treatment and 36 tasks in Main-Control, which is not significantly different ($p = 0.438$).[11] Across the eight balanced choice blocks, the average increase in the willingness to work varies between 34 and 40 tasks, potentially reflecting nonlinear appreciation of more valuable restaurant vouchers, differences in the number of mandatory real-effort tasks, the time span until the date on which the tasks have to be completed, and the possibility that subjects have more time to work on some dates than on others.

In the following, we analyse our within-subject comparisons between the first eight choice blocks and the last choice block by referring to our outcomes as specified in Section 1.3.3: (i) subjects' absolute per-workday deviation from the point prediction of standard discounting in the last choice block, that is, d; (ii) subjects' average relative per-workday deviation from the point prediction, that is, ARD.

For subjects with upper corner choices in the final blocks we recorded intervals of our outcome measures. The lower bound of the interval results from assuming that their indifference point is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound of the interval, we assume that their indifference point is the value that they state manually, as described in Section 1.B.2 of Section 1.B. While we report the results for the lower and the upper bound in our main tables for completeness, we refer to subjects' mean of the lower and upper bound of d and ARD in the text for brevity.

1.4.1.2 Condition Main-Treatment

In a first step, we focus on the Main-Treatment condition and compare subjects' decisions in the unbalanced choice block with those in the balanced choice blocks. Recall that, given the way in which the unbalanced trade-off is constructed, standard discounting makes for each subject the point prediction that they are indifferent in the unbalanced choice block for the same work plan as in the balanced choice blocks. In other words, the absolute per-workday deviation from the point prediction in real-effort tasks is zero, that is, $d = 0$ and ARD $= 0$. Concentration bias, by contrast, predicts that individuals are willing to work more when facing the unbalanced trade-off, that is, $d > 0$ and ARD > 0.

Result 1. *Subjects in the Main-Treatment condition report on average a per-workday deviation from the point prediction, d, of 38 tasks, and an average relative per-workday deviation, ARD, of 22%. Both measures are significantly greater than zero* ($p < 0.001$, respectively). *This provides evidence in favour of Hypothesis 1 (i).*

Our first results present support for Hypothesis 1 (i). Subjects' average willingness to work beyond the mandatory tasks in the unbalanced choices is on average

11. The difference in the mean values is driven by outliers. The median value is 30 tasks in both cases.

Table 1.2. Estimates of concentration bias: average absolute per-workday deviation

	Dependent variable: Average absolute per-workday deviation of real-effort tasks from point prediction of standard discounting						
	OLS			Tobit	Median regression		
	(1) Lower bound	(2) Upper bound	(3) Mean	(4)	(5) Lower bound	(6) Upper bound	(7) Mean
1 if TREATMENT 0 if CONTROL	36.180***	48.120***	42.150***	39.581***	31.000***	35.000***	35.000***
	(3.074)	(4.917)	(3.876)	(3.543)	(4.895)	(4.895)	(4.895)
Constant	−4.540**	−4.540**	−4.540**	−4.623	−1.000	−1.000	−1.000
	(1.496)	(1.496)	(1.496)	(2.465)	(3.461)	(3.461)	(3.461)
Observations	200	200	200	200	200	200	200
Adjusted R^2	0.409	0.323	0.371				
Pseudo R^2				0.056	0.193	0.151	0.169

Notes: This table presents estimates of the treatment difference for the average absolute per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. Columns (1), (2) and (3) present OLS regressions of our dependent variable on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. Column (4) presents an analogous Tobit regression. Columns (5), (6) and (7) present analogous median regressions. The table provides estimates for a lower bound in columns (1) and (5), for an upper bound in columns (2) and (6) and for the mean of the two in columns (3) and (7) for the OLS and median regressions. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

76 tasks *per workday*. Recall that the average willingness to work in the balanced choice blocks—and thus the point prediction of standard discounting—is on average 38 tasks beyond the mandatory tasks per workday in MAIN-TREATMENT. The difference yields the absolute per-workday deviation between the unbalanced choice block and the point prediction, d, of, on average, 38 tasks. The corresponding relative per-workday deviation, ARD, is 22%. Both are significantly greater than zero ($p < 0.001$).

1.4.1.3 Condition MAIN-TREATMENT versus condition MAIN-CONTROL

As argued in Section 1.3.2, if we solely relied on the balanced and unbalanced choices in the MAIN-TREATMENT condition in our empirical assessment of concentration bias, the analysis would rest on the assumption that subjects' preferences are consistent across balanced and unbalanced choice blocks. In order not to have to make this assumption, we devised the between-subject condition MAIN-CONTROL to

Table 1.3. Quantitative estimates of concentration bias: average relative per-workday deviation

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting					
	OLS			Median regression		
	(1) Lower bound	(2) Upper bound	(3) Mean	(4) Lower bound	(5) Upper bound	(6) Mean
1 if TREATMENT	0.212***	0.281***	0.247***	0.236***	0.240***	0.236***
0 if CONTROL	(0.018)	(0.029)	(0.023)	(0.029)	(0.030)	(0.030)
Constant	−0.023**	−0.023**	−0.023**	−0.008	−0.008	−0.008
	(0.008)	(0.008)	(0.008)	(0.021)	(0.021)	(0.021)
Observations	200	200	200	200	200	200
Adjusted R^2	0.411	0.323	0.373			
Pseudo R^2				0.204	0.161	0.180

Notes: This table presents OLS and median regression estimates of the treatment difference for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean of the two. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

control for potential within-subject inconsistencies. In condition MAIN-CONTROL, we measured subjects' inconsistencies in the form of deviations from the point prediction of standard discounting when concentration bias does not interfere. We hence identify concentration bias from a greater per-workday deviation of the point prediction in condition MAIN-TREATMENT than in condition MAIN-CONTROL.

Result 2. *Subjects in MAIN-TREATMENT report on average a greater absolute and relative per-workday deviation from the point prediction than subjects in MAIN-CONTROL. The treatment effect on the absolute per-workday deviation amounts to 42 tasks and on the relative per-workday deviation amounts to 25%. These treatment effects are both significant ($p < 0.001$) and provide further evidence in favour of Hypothesis 1 (ii).*

We also find support for Hypothesis 1 (ii). Our between-subject comparisons shows that average absolute and relative per-period deviations from the point prediction differ across conditions: They are greater when driven by concentration bias (MAIN-TREATMENT) than when merely possible due to inconsistencies (MAIN-CONTROL). Our results show that indeed subjects' preferences are somewhat inconsistent between choice blocks in condition Main-Control: they decrease their willingness to work in the last choice block. However, this inconsistency goes in the opposite direction of concentration bias and is relatively small.

Table 1.2 depicts treatment effects for the lower and upper bounds as well as their mean of the absolute per-period deviation d in OLS and median regressions (columns 1–3 and 5–7, respectively). In column (4), we also report the treatment effect when looking at the lower bound for d in a Tobit regression, which offers a different approach to address corner choices in our outcome measure (other than exploiting the manual indifference statements of subjects with corner choices in the last choice block). For all specifications, these treatment effects are significant ($p < 0.001$), indicating strong evidence for concentration bias. Table 1.3 depicts analogous treatment effects for the lower and upper bounds as well as their mean of the average relative per-period deviation ARD. All measures of the treatment effect are significant at all conventional levels.[12]

In the following, we investigate the robustness of our findings and conduct a heterogeneity analysis. In doing so, we focus on the average relative per-period deviation outcome measure.

1.4.2 Robustness

1.4.2.1 Robustness with respect to assignment to the treatments

Recall that we stratified within each session the randomized assignment to the conditions Main-Treatment and Main-Control by subjects' average indifference points over the balanced choice blocks (see Section 1.3.2). This was done to maximize the similarity of the subjects in the two conditions. In the following, we investigate potential differences regarding other personal characteristics. We thus investigate whether the treatment effect is robust to the inclusion of variables that capture individual heterogeneity—fixed effects for age, gender, and experimental session, average response time in the balanced choice blocks, and measures of cognitive ability: performance in the mental-arithmetic task ("Math score"), number of correct responses in the Cognitive Reflection Test ("CRT score"), and correct answers in the Raven Progressive Matrices task ("Raven score").

Result 3. *The measure of concentration bias reported in Result 2 is robust to controlling for individual differences.*

We find that the treatment effect is robust to the inclusion of the mentioned covariates: their inclusion does not impact the treatment effect. This is evident from the first row of Table 1.4: the coefficient on the treatment dummy across all specifications as well as its standard error remain virtually unchanged compared to the

12. Note that the Tobit regression is not directly feasible as ARD is not naturally censored.

Table 1.4. Quantitative estimates of concentration bias: average relative per-workday deviation, including controls for between-subject heterogeneity

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting					
	(1)	(2)	(3)	(4)	(5)	(6)
1 if TREATMENT, 0 if CONTROL	0.246*** (0.023)	0.245*** (0.024)	0.243*** (0.024)	0.245*** (0.024)	0.246*** (0.023)	0.242*** (0.025)
Avg. RT in blocks 1–8 (standardised)		0.003 (0.011)				0.001 (0.012)
Math score (standardised)			−0.009 (0.015)			−0.009 (0.017)
CRT score (standardised)				−0.005 (0.012)		−0.004 (0.013)
Raven score (standardised)					0.001 (0.011)	0.005 (0.013)
Constant	−0.022* (0.010)	−0.022* (0.010)	−0.021* (0.010)	−0.022* (0.010)	−0.022* (0.010)	−0.020 (0.010)
Age group FE	Yes	Yes	Yes	Yes	Yes	Yes
Gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	200	200	200	200	200	200
Adjusted R^2	0.364	0.360	0.362	0.361	0.360	0.352

Notes: This table presents OLS estimates of the treatment difference for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for the mean and includes standardised controls for between-subject heterogeneity. Columns (2)–(5) include the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, respectively. Column (6) includes all four controls concurrently. All columns additionally include fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

specification without controls in Table 1.3, column (3). Consequently, the treatment is significant ($p < 0.001$) in all specifications that include subject-level covariates.

Table 1.5. Quantitative estimates of concentration bias: average relative per-workday deviation, accounting for noise in participants' choices

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting	
	(1)	(2)
1 if TREATMENT, 0 if CONTROL	0.247***	0.244***
	(0.023)	(0.025)
Initial choice row in block 9 for MAIN-TREATMENT (standardised)	−0.018	−0.017
	(0.023)	(0.023)
Initial choice row in block 9 for MAIN-CONTROL (standardised)	−0.009	−0.014
	(0.008)	(0.010)
Constant	−0.022**	−0.020*
	(0.008)	(0.010)
Cognitive controls		Yes
Age group FE		Yes
Gender FE		Yes
Session FE		Yes
Observations	200	200
Adjusted R^2	0.371	0.350

Notes: This table presents OLS estimates of the treatment difference for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for the mean and controls for the noise in participants' choice by including the standardised initial choice row in decision block 9 for the respective condition. Column (2) additionally includes cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

1.4.2.2 Robustness with respect to noise in participants' choices

Recall that the decisions in the unbalanced choice block were not only based on but also centred around the point prediction of standard discounting. This means that noise in participants' decisions that is symmetric around zero cannot explain systematic deviations of the indifference points elicited in the unbalanced choice block from the indifference points elicited in the balanced choice blocks.

To be on the safe side, we additionally analyse whether a potential random component in participants' choices has an influence on our estimated degree of concentration bias. We do so by exploiting the fact that the first pairwise choice that participants face in a given choice block is randomly drawn from all pairwise choices included in that choice block. This means that some participants will by chance be confronted with an initial pairwise choice in which option A includes substantially more tasks than the option A that would lead to indifference according to standard discounting, while others will be confronted with an option A that is close to the point prediction of standard discounting, and yet others with an option A that includes much fewer tasks than the point prediction. As a consequence, if participants made random decisions, their estimated indifference point in the unbalanced choice block would be influenced by the pairwise choice that they initially faced in that block. Hence, if we include an index of how much the option A of the initial choice in the unbalanced choice block deviates from the point prediction as an explanatory variable in the analysis of the treatment effect Main-Treatment vs. Main-Control, a positive coefficient on that regressor would indicate the presence of randomness in participants' decisions.

Result 4. *The measure of concentration bias reported in Result 2 is robust to controlling for randomness in participants' choices.*

Table 1.5 shows that random choice in choice block 9 cannot explain the treatment effect: the coefficient on the deviation of the option A included in the initial choice from the point prediction of standard discounting is very close to zero and not statistically significant in any of the specifications. The same is true for the coefficient on the interaction of the initial choice and the treatment dummy. Therefore, noise in participants' decisions seems negligible in the sense that it is not correlated with the size of the concentration bias.

1.4.3 Heterogeneity

The histogram depicted in Figure 1.3 shows that there is substantial heterogeneity in subjects' deviations from the point prediction of standard discounting in the unbalanced choice block. In order to shed light on the mechanism behind the observed concentration bias, we investigate this between-subject heterogeneity by including individual-level control variables separately for each condition.

We find that better cognitive ability—the Cognitive Reflection Test (Frederick, 2005), a Raven Progressive Matrices IQ test (Raven, 1941), and a self-developed mental-arithmetic task—go along with a reduced concentration bias. This is expressed in Table 1.6 by the coefficient on the interaction of the respective variable with the treatment dummy being negative in columns (3)–(8) of Table 1.6. However, none of these relations is statistically significant.

We turn next to subjects' response time. Our response time measure is subjects' average response time for the last choice block relative to their average response time for the first eight choice blocks. In condition Main-Treatment, this could be

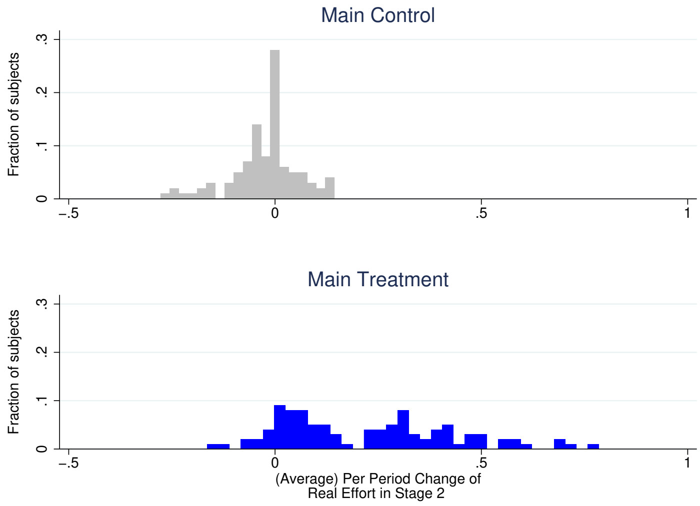


Figure 1.3. Histogram depicting the deviation from the point prediction of standard discounting in choice block 9 for MAIN-TREATMENT and MAIN-CONTROL.

Table 1.6. Heterogeneity of concentration bias

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if TREATMENT, 0 if CONTROL	0.247***	0.245***	0.247***	0.246***	0.247***	0.246***	0.247***	0.246***
	(0.021)	(0.021)	(0.023)	(0.023)	(0.023)	(0.023)	(0.023)	(0.024)
Avg. RT in block 9 relative to blocks 1–8 for MAIN-TREATMENT (standardised)	−0.095***	−0.095***						
	(0.020)	(0.021)						
Avg. RT in block 9 relative to blocks 1–8 for MAIN-CONTROL (standardised)	0.012	0.012						
	(0.008)	(0.010)						
Math score for MAIN-TREATMENT (standardised)			−0.017	−0.015				
			(0.026)	(0.028)				
Math score for MAIN-CONTROL (standardised)			−0.006	−0.003				
			(0.008)	(0.013)				
CRT score for MAIN-TREATMENT (standardised)					−0.017	−0.015		
					(0.021)	(0.021)		
CRT score for MAIN-CONTROL (standardised)					0.009	0.006		
					(0.008)	(0.010)		
Raven score for MAIN-TREATMENT (standardised)							−0.010	0.001
							(0.020)	(0.022)
Raven score for MAIN-CONTROL (standardised)							−0.000	0.001
							(0.008)	(0.010)
Constant	−0.023**	−0.022*	−0.023**	−0.022*	−0.023**	−0.022*	−0.023**	−0.022*
	(0.008)	(0.010)	(0.008)	(0.010)	(0.008)	(0.010)	(0.008)	(0.010)
Age group FE		Yes		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes		Yes
Session FE		Yes		Yes		Yes		Yes
Observations	200	200	200	200	200	200	200	200
Adjusted R^2	0.481	0.472	0.370	0.359	0.371	0.360	0.368	0.357

Notes: This table presents OLS estimates of the treatment difference for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for the mean and within-condition heterogeneity of the effect. Columns (1) and (2) include the average response time in block 9 relative to the first eight decision blocks, columns (3) and (4) a measure for math ability, columns (5) and (6) a measure for the Cognitive Reflection Test, and columns (7) and (8) a measure for the Raven Progressive Matrices task, all standardised by condition. Columns (2), (4), (6) and (8) additionally include fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

substantial, since the unbalanced choices are more complex. Subjects who adjust their response time only very little may hence be more prone to disproportional focusing on the concentrated benefit and, hence, commit to too much effort provision.

Consistent with this view, our response time measure correlates negatively with the magnitude of concentration bias. The negative relation is expressed by the coefficient on the interaction of the response time regressor with the treatment dummy in columns (1) and (2) of Table 1.6 being negative ($p < 0.001$). That is, longer deliberation seems to lead to a smaller bias: subjects who take more time fall prey to concentration bias in the main treatment to a lower degree.

Result 5. *Measures of cognitive ability do not explain variation (within MAIN-TREATMENT) in the strength of concentration bias. Longer response times in the unbalanced choice block, by contrast, go along with less pronounced concentration bias* ($p < 0.001$).

Our heterogeneity analyses suggest that concentration bias is unlikely the result of constraints in cognitive performance to deal with unbalanced intertemporal choices. Instead, concentration bias seems to result from constraints in cognitive devotion to the "decision problem" at hand. We interpret this as a first piece of (suggestive) evidence for the focusing interpretation (Kőszegi and Szeidl, 2013) of concentration bias. We turn to a more involved treatment of the mechanisms behind concentration bias in the next section.

1.5 Mechanism

Kahneman (2003b, p. 1452) uses the term "accessibility" to describe "the ease with which mental contents come to mind". The differential accessibility of the characteristics of a decision situation operates through qualitatively different cognitive processes in the human brain. The fundamental distinction is between fast autonomous processes ("System 1") and slower deliberative processes ("System 2"). Research has shown that the fast autonomous processes readily compute various specific representations of the objects that humans attend to—including, for instance, accurate impressions of averages. By contrast, use of the slower effortful processes is required to compute representations relating to the aggregation of characteristics—including, for instance, sums.

Concentration bias in intertemporal choice describes behavior in a situation in which utility outcomes in a few periods—in our experiment on a single day—are traded off with utility outcomes that affect considerably more periods—in our experiment eight days. Taking the research described by Kahneman (2003b) into account, the concentrated–dispersed asymmetry raises the question whether outcomes that are concentrated in a few periods are more accessible to humans than outcomes that are dispersed over multiple periods. Put differently, it might be the case that

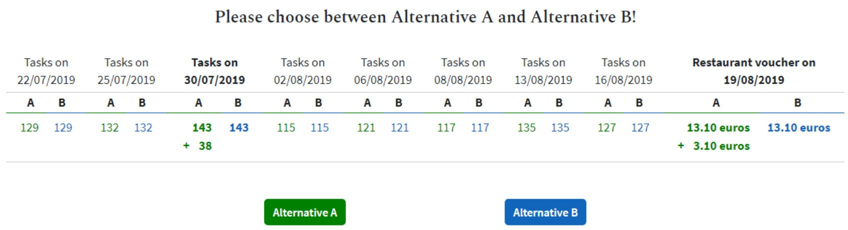


Figure 1.4. Screenshot of a decision screen for a balanced choice in the MECHANISM-TREATMENT condition.

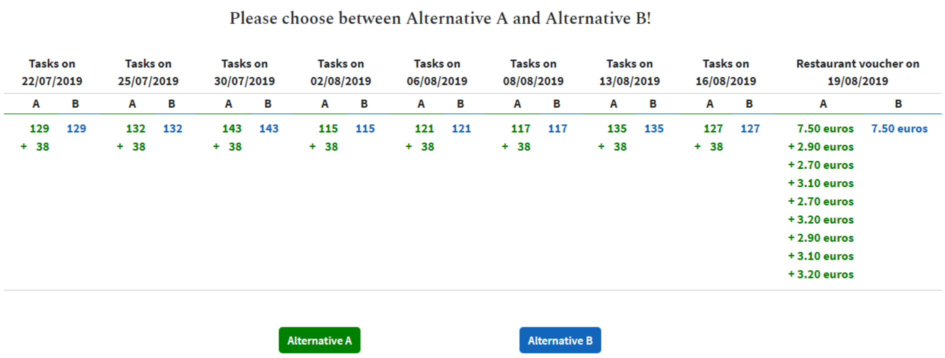


Figure 1.5. Screenshot of a decision screen for an unbalanced choice in the MECHANISM-TREATMENT condition.

concentration bias, at least partially, results from differential accessibility of the concentrated and dispersed outcomes.

To investigate this possibility, we devised additional conditions of the consumption experiment (also pre-registered). Conditions MECHANISM-TREATMENT and MECHANISM-CONTROL were conducted contemporaneously with conditions MAIN-TREATMENT and MAIN-CONTROL.

Conditions MECHANISM-TREATMENT and MECHANISM-CONTROL differ from MAIN-TREATMENT and MAIN-CONTROL only in the way in which the workplans and the compensation are displayed: Just like in the two MAIN conditions, a pairwise choice would be, for example, between option A with 181 tasks on the 3rd date (and the mandatory tasks on the remaining 7 dates) and option B with 143 tasks on the 3rd date (and the mandatory tasks on the remaining 7 dates). The associated compensations would be a €16.20 voucher for A and a €13.10 voucher for B.

The way in which the values are displayed changes, however, in our additional experiment: the values for A in this example are now shown as the sum "143 + 38" instead of the resulting "181" and as the sum "€13.10 + €3.10" instead of the resulting "€16.20" (see Figure 1.4). In the unbalanced choice block 9, the display would be, for instance, 129 + 38, 132 + 38, 143 + 38, 115 + 38, . . . for the number of tasks in exchange for a voucher of value €7.50 + €2.90 + €2.70 + €3.10 + . . . + €3.20 (see Figure 1.5).

Table 1.7. Quantitative estimates of concentration bias: Difference-in-differences analysis for the conditions Main-Treatment, Main-Control, Mechanism-Treatment, and Mechanism-Control

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting	
	(1)	(2)
1 if Treatment, 0 if Control	0.090***	0.095***
	(0.021)	(0.021)
1 if Main, 0 if Mechanism	−0.008	−0.052
	(0.013)	(0.060)
{1 if Treatment, 0 if Control} × {1 if Main, 0 if Mechanism}	0.157***	0.151***
	(0.031)	(0.032)
Constant	−0.014	0.006
	(0.010)	(0.031)
Cognitive controls		Yes
Age group FE		Yes
Gender FE		Yes
Session FE		Yes
Observations	400	400
Adjusted R^2	0.286	0.279

Notes: This table presents OLS estimates of the treatment difference, as well as the difference between Main and Mechanism, for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if Treatment and equals 0 if Control, a second dummy that equals 1 if Main and equals 0 if Mechanism, and an interaction dummy that equals 1 if Main-Treatment and equals 0 otherwise. The table provides estimates for the mean. Column (2) additionally includes cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, respectively, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from Main-Treatment, Main-Control, Mechanism-Treatment, and Mechanism-Control. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

This means that in the balanced choices, the evaluation of both the additional workload and the additional compensation is simpler in the Mechanism conditions than in the Main conditions. For the unbalanced choice block 9, however, the Mechanism-Treatment condition facilitates evaluating the additional workloads and simultaneously makes evaluating the additional compensation harder, both compared to the Main-Treatment condition.

Based on the notion that the need to calculate the sum of the compensation in Mechanism-Treatment reduces the accessibility of the concentrated benefit and that the reduced accessibility impacts the focus weight, we hypothesised that concentration bias would be smaller in the Mechanism conditions than in the Main conditions. This effect may even be enhanced by the increased accessibility of additional workload because this increased accessibility makes working less attractive.

Hypothesis 2. *Reflecting the reduced accessibility of the concentrated compensation and the enhanced accessibility of the dispersed costs of obtaining that compensation, concentration bias measured in the Mechanism conditions is smaller than in the Main conditions.*

We find support for Hypothesis 2. During the repetition of the balanced choice blocks, the alternative way of displaying the workloads and the compensation had no statistically significant effect (see the coefficient on 1 if Main, 0 if Mechanism in row 2 of Table 1.7). The measure of concentration bias for conditions Mechanism-Treatment and Mechanism-Control amounts to 9%, which is significant ($p < 0.001$), robust to individual controls and significantly smaller by 16 percentage point than for conditions Main-Treatment and Main-Control ($p < 0.001$).[13] The results that we present in Table 1.7 suggest that 38% of the overall concentration-bias effect estimated in our consumption experiment can be attributed to concentration in the temporal dimension *per se* and that 62% can be attributed to accessibility.

Result 6. *Making the dispersed costs of working in exchange for higher compensation more accessible and simultaneously making the concentrated higher compensation less accessible reduces the size of concentration bias* ($p < 0.001$). *This supports Hypothesis 2. The remaining effect is still sizable and significant* ($p < 0.001$).

In comparison to the numbers reported above, we find that concentration in time and accessibility explain roughly 60% and 40%, respectively, of the concentration bias observed in our money experiment (see Section 1.6.2, in particular Section 1.6.2.4). Taken together, we interpret our results as evidence that both concentration in time and accessibility are important determinants of the measured total concentration bias and that the relative contribution of the two components may depend on the exact context.

1.6 Additional Evidence

1.6.1 Donation conditions

The results of two further between-subjects conditions of our consumption experiment allow us to provide additional evidence regarding the robustness of our

13. We also find treatment effects identifying concentration bias in conditions Mechanism-Treatment and Mechanism-Control for the lower and upper bound, see Table 1.C.1 in Appendix 1.C.

findings of concentration bias. Conditions Donation-Treatment and Donation-Control are like conditions Main-Treatment and Main-Control, respectively, except for one main difference: instead of using a restaurant voucher as the utility benefit of working, subjects' work generates a donation to a good cause[14]. A comparison of conditions Donation-Treatment and Donation-Control identifies a concentration-bias effect of 16% ($p < 0.001$), see Table 1.8.

Result 7. *The measure of concentration bias reported in Result 2 is robust to changing the concentrated utility benefit that subjects receive in exchange for increased effort provision.*

While the Donation conditions allow us to test the robustness of our results on concentration bias, we did not intend to thoroughly compare the Donation conditions with the Main conditions, as a thorough comparison is not warranted: Conditions Donation-Treatment and Donation-Control were conducted two weeks apart from the other conditions at a close but different laboratory (BonnEconLab versus Cologne Laboratory for Economic Research). Furthermore, the donation values are slightly lower than the restaurant voucher[15].

However, since the average chosen effort levels in the balanced decisions of the first eight choice blocks are reasonably similar in all conditions, we point out that the estimated concentration bias is significantly smaller in the Donation conditions than in the Main conditions ($p < 0.01$), 64% of the concentration-bias measure of the Main conditions.

A potential explanation is that subjects may anticipate not to derive the utility from making a donation in concentrated in the day the donation is conducted, but rather dispersed over more than one time period—potentially because of immediate feelings of warm glow and anticipation of receiving social approval for their good deed in the future. If the focus weighting function is sufficiently steep and the use of donations actually diminishes how concentrated in time the positive consumption utility is, a smaller concentration-bias effect would be expected for conditions Donation-Treatment and Donation-Control than for conditions Main-Treatment and Main-Control.

Because of accessibility-based concentration bias—the donation values were displayed equally accessible to subjects as the values of the restaurant vouchers in conditions Main-Treatment and Main-Control—a positive treatment effect would still be predicted, as is observed. This comparison relates well to our difference-in-differences analyses performed to investigate the contributions of concentration in

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

15. We used greater values for the restaurant vouchers, because we wanted to ensure that subjects were always able to consume some food in the restaurant. In addition, we elicited a partly different set of tasks to assess cognitive ability in the Donation conditions.

Table 1.8. Quantitative estimates of concentration bias: Analysis for the DONATION conditions

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting					
	Lower Bound		Upper Bound		Mean	
	(1)	(2)	(3)	(4)	(5)	(6)
1 if TREATMENT, 0 if CONTROL	0.138*** (0.019)	0.137*** (0.020)	0.177*** (0.025)	0.176*** (0.026)	0.158*** (0.022)	0.157*** (0.022)
Constant	−0.023** (0.007)	−0.023** (0.008)	−0.023** (0.007)	−0.022* (0.009)	−0.023** (0.007)	−0.022** (0.009)
Cognitive controls		Yes		Yes		Yes
Age group FE		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes
Session FE		Yes		Yes		Yes
Observations	200	200	200	200	200	200
Adjusted R^2	0.201	0.196	0.193	0.190	0.201	0.197

Notes: This table presents OLS estimates of the treatment difference in the DONATION conditions for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if DONATION-TREATMENT and equals 0 if DONATION-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean of the two. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Columns (2), (4), and (6) additionally include cognitive controls with the average response time for the first eight decision blocks and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from DONATION-TREATMENT and DONATION-CONTROL. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

time and accessibility to total concentration bias in our MAIN conditions (see Section 1.5). We view this finding, however, to be merely of suggestive value for the aforementioned reasons.

1.6.2 Convex Time Budgets

1.6.2.1 General setup

We conducted an additional experiment that is based on the "Convex Time Budgets" method of Andreoni and Sprenger (2012) and uses dated money transfers to subjects' bank accounts as an outcome.

The money experiment is also based on comparisons between balanced and unbalanced intertemporal choices. It differs from the consumption experiment with respect to the following features: (i) The balanced and unbalanced choices were chained in the consumption experiment; the money experiment, by contrast, was designed to confront every subject with exactly the same set of choices. (ii) In the money experiment, we investigate two different comparisons between balanced and unbalanced choices to test for both concentration-bias–based present bias and future bias. (iii) We vary the degree of dispersion among the unbalanced choices so that we can investigate whether a greater degree of dispersion leads to stronger concentration bias. (iv) While our identification of concentration bias in the consumption experiment is free of any assumptions regarding the curvature of subjects' per-period utility, the money experiment rests on the assumption of nonconvex per-period utility. (v) Since the Kőszegi–Szeidl model is written over consumption and subjects may anticipate *not* to consume immediately upon receipt of the dated experimental payments, the money experiment may underestimate the effect of concentration bias. Consistent with this view, we find a greater effect of concentration bias in our consumption experiment than in our money experiment—which, however, may also be the result of other design features that differ between the two experiments.

The money experiment consists of two conditions, Money-Main and Money-Mechanism. Money-Main tests for concentration bias that is potentially generated by both components, concentration in time *per se* and accessibility. By contrast, Money-Mechanism focuses on the latter channel by reducing the accessibility of a concentrated payoff.

This section provides an overview of the design and the main results of this experiment. For a description that includes all the details, see Appendix 1.F.

1.6.2.2 Design

In this experiment, each participant repeatedly allocates a monetary budget to an earlier and a later payoff. More precisely, subjects' decisions are of two different types: (i) Some decisions are balanced, with both payoffs being concentrated on a single payment date. (ii) Some decisions are unbalanced, where either the *earlier* or the *later* payoff is dispersed over 2, 4, or 8 payment dates. Importantly, all payments of the dispersed payoffs occur no later than the corresponding concentrated payment, and they sum up to the concentrated payoff. In other words, the present value of each dispersed payoff is higher than that of the corresponding concentrated payoff. In each decision, subjects choose their "savings rate", that is, the share of the awarded budget that they allocate to the later payoff. Comparing this savings rate between the two types of decisions identifies concentration bias.

The sum total of the payoffs is the greater, the more subjects save. This means that we implement an intertemporal budget constraint with a strictly positive nominal interest rate. By including dispersed payoffs, we extend the "Convex Time Budget" approach introduced by Andreoni and Sprenger (2012) to settings in which individuals face more than two payment dates. To equalize transaction costs, we hold the number of transfers constant across conditions. To this end, subjects receive an additional fixed amount of €1 on each of 9 payment dates. That is, each budget set gives rise to a series of 9 money transfers to subjects' bank accounts at given dates in the future. However, the budgets materially differ with respect to the degree of dispersion of the payoffs that are under a participant's control.

The comparison of a balanced decision with an unbalanced decision in which the *later* payoff is dispersed tests the following: Do subjects behave differently when the benefits of being *patient* are concentrated on a single future date (as in the balanced trade-off) than when they are dispersed over multiple future dates (as in the unbalanced trade-off)? Concentration bias predicts that individuals underweight dispersed consequences relative to concentrated consequences. Thus, individuals are predicted to display a "present bias", which reveals itself in a savings rate that is *lower* for the unbalanced than for the balanced trade-off.

The reverse direction is tested by comparing a balanced trade-off with an unbalanced decision in which the *earlier* payoff is dispersed. Here, the benefits of being *impatient*—that is, of choosing a small savings rate—are concentrated in the balanced trade-off while they are dispersed in the unbalanced one. Therefore, concentration bias predicts that individuals exhibit a "future bias" that reveals itself in the form of a savings rate that is *higher* for the unbalanced trade-off.

To identify concentration bias, we need to ensure that transaction costs are the same for all conditions. As already mentioned above, we therefore transferred a baseline payment at each date to the subjects, which is the modern way to equalize transaction costs across decisions (Balakrishnan, Haushofer, and Jakiela, 2017; Andreoni and Sprenger, 2012). In addition, each subject received two individualized e-mail messages after the experiment that included a complete listing of all payments. Before making any decisions in the experiment, the written instructions informed subjects in detail about these two messages. Hence, subjects knew in advance that they would be informed when exactly they would have to inspect their bank statements to check that they had received the promised amounts. Moreover, the predictions are based on the assumption that utility of money is non-convex. This is in line with previous findings in the literature. Andreoni and Sprenger (2012) and Augenblick, Niederle, and Sprenger (2015), for instance, estimate that utility is concave (albeit close to linear).

1.6.2.3 Results

Since subjects ($n = 185$) make several allocation decisions for each comparison, we can calculate for each individual the average difference in the savings rate between the unbalanced and the associated balanced budget sets. On average, subjects allocate 6.3 percentage points (p.p.) more money to payoffs that are concentrated than to the associated dispersed payoffs. This treatment effect is statistically significant ($p < 0.01$), using a t-test, with standard errors corrected for potential clustering on the subject level. This result provides evidence for concentration bias as predicted by Kőszegi and Szeidl (2013).

This overall effect is driven by concentration bias that leads to "present bias" as well as to "future bias": Subjects allocate, on average, 5.7 p.p. (= 9.12%) more money to later payment dates when the later payoff is concentrated rather than dispersed. They also allocate, on average, 6.8 p.p. (= 9.65%) more money to later payment dates in the unbalanced decision in which the earlier payoff is dispersed compared with the associated balanced decision. Both differences are significantly greater than zero in a t-test (both $p < 0.01$). This demonstrates that the temporal structure of the outcomes can indeed influence individuals' decisions in both directions, consistent with the central prediction of the focusing model.

We furthermore find that the size of the concentration bias depends on the degree to which the dispersed payoff is spread over time. Recall that subjects make allocation decisions for three degrees of dispersion: payoffs are spread over 8, 4, or 2 payment dates. Our measure of concentration bias is 8.10 p.p. for 8 payment dates, 6.56 p.p. for 4 payment dates, and 3.67 p.p. for 2 payment dates. All three treatment effects are significantly larger than zero according to both t-tests and signed-rank tests ($p < 0.01$ for dispersion over 8 and 4 dates; $p < 0.05$ for 2 dates in both tests). Moreover, concentration bias in the case that payoffs are dispersed over 4 or 8 payment dates is significantly greater than when payoffs are dispersed over 2 payment dates.

Note that we also conducted a between-subject heterogeneity analysis in the money experiment, like in the consumption experiment (with the exception that we did not implement the Raven Progressive Matrices task). We find no correlation between our cognitive ability measures in the money experiment, like in the consumption experiment. We also do not find a correlation between response time and concentration bias in the money experiment, while we do find a significant correlation in the consumption experiment.

1.6.2.4 Mechanism

In analogue to the MECHANISM-TREATMENT condition in our consumption experiment, we also included a condition to investigate the cognitive mechanisms behind concentration bias in the CTB-based experiment. We call this condition MONEY-

MECHANISM. This investigation is done as a between-subjects design (additional $n = 189$).

A potential mechanism contributing to the concentration bias reported in Section 1.6.2.3 is accessibility, as already discussed in Section 1.5. We therefore devised a condition which is analogous to the one described in Section 1.6.2.2, with the difference that the dispersed payoffs are not genuinely dispersed but only displayed as such. We call these conditions "dispersed within a day". As before, we compare savings rates for trade-offs between two concentrated payoffs with savings rates for trade-offs between a concentrated payoff and a payoff that is displayed as dispersed.

This means that for the balanced trade-offs nothing changes. The unbalanced trade-offs in MONEY-MECHANISM differ from those in MONEY-MAIN in the sense that the dispersion is not over 2, 4, or 8 dates any more but only in the way the payoff is displayed to participants: in the unbalanced trade-offs, either the earlier or the later payoff is displayed as the sum of 2, 4, or 8 smaller payments that all occur on a single day. This day is exactly the same day as in the associated balanced trade-off. That is, participants in MONEY-MECHANISM see exactly the same numbers in their unbalanced decisions as those in MONEY-MAIN; at the same time, the dispersed payoffs in MONEY-MECHANISM are identical in both magnitude *and* timing to those in the associated balanced decisions.

This allows us to answer the following two questions: (i) Does decreasing the accessibility of a payoff by simply splitting up its amount into the sum of several smaller amounts have an effect on individuals' decisions? (ii) If such an effect exists, does it completely or only partially account for the concentration bias reported in Section 1.6.2.3?

Regarding question (i), we find that decisions for unbalanced trade-offs indeed differ from decisions in balanced ones. On average, subjects allocate 2.6 p.p. more of their budget to concentrated than to "dispersed-within-a-day" payoffs ($p < 0.01$, $n = 189$). Thus, making the evaluation of an outcome cognitively more demanding by merely splitting up a number into a sum of smaller numbers has an effect on behavior—in the direction expected by accessibility.

This effect, however, explains no more than half of the effect of 6.3 p.p. reported in Section 1.6.2.3. A difference-in-differences analysis reveals that the total effect of 6.3 p.p. is significantly larger than the "dispersed-within-a-day" effect ($p < 0.01$, $n = 185 + 189$; for details see Table 1.F.4). Thus, regarding question (ii), we find that in this setup, accessibility of a payoff accounts for roughly 40% of the observed concentration bias.

Result 8.

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

(ii) *Depending on the temporal structure of the dispersed and concentrated consequences of the available alternatives, concentration bias can lead to both present-*

biased and future-biased behavior, as predicted by the focusing model (Kőszegi and Szeidl, 2013).

(iii) *The strength of concentration bias is monotonously increasing in the degree to which the dispersed outcomes are spread over time, as predicted by the focusing model (Kőszegi and Szeidl, 2013).*

(iv) *Also in this different setting, reduced accessibility of the concentrated outcome reduces concentration bias.*

1.7 Conclusion

In two laboratory experiments, we investigated whether and how the degree to which utility outcomes are concentrated in time affects intertemporal choices. In both experiments—which involved trade-offs between real effort and restaurant vouchers or charity donations or between smaller–sooner and larger–later monetary payoffs—we find significant concentration bias. This confirms the key prediction of the focusing model by Kőszegi and Szeidl (2013). In our Main conditions of the consumption experiment, we find a concentration bias of about 25%, meaning that the mere concentration of the reward (here, a restaurant voucher) induces subjects to work 25% more each of the eight work days relative to what standard discounting models could explain. In further between-subject conditions, we provide evidence that at least two components contribute to concentration bias: differences in time *per se* and the accessibility of the outcomes.

The standard economic approach to intertemporal decision making relies on the assumption that subjects discount future outcomes, either exponentially or (quasi-)hyperbolically. By investigating the importance of concentration and dispersion in time, our results suggest a determinant of intertemporal choice beyond what is captured by standard discounting models. In particular, concentration bias can—depending on the particular context—lead to future- and present-biased behavior relative to standard discounting. This means that individuals who are rather patient according to their underlying time preferences appear present-biased and vice versa.

This has direct implications for the design of economic experiments and the estimation of model parameters. For instance, Attema et al. (2016) used dispersed payoffs in the form of multiple bank transfers to remunerate subjects. Attema et al. propose an elegant method in which they measure discounting without measuring utility.[16] Given that the degree of curvature of the utility function is a topic of intense debate, being able to elicit an individual's degree of discounting without any regard to per-period utility is an attractive feature of this method. Since their method

16. The basic idea of their method is intriguingly simple: Imagine an individual who is indifferent between receiving \$10 today and receiving \$10 in one year plus \$10 in two years. With a constant 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 $1 = \delta + \delta^2$.

makes use of unbalanced trade-offs, however, our evidence implies that the discount rate elicited by Attema et al.'s method is actually a quantity jointly determined by an individual's "genuine" discount rate and their degree of concentration bias.

In providing evidence for concentration bias in intertemporal choice, our paper contributes to the broader recent experimental literature on the bounded rationality of reduced-form behavioral biases (Esponda and Vespa, 2014; Enke, 2019; Enke and Graeber, 2019; Enke and Zimmermann, 2019; Frydman and Jin, 2019). More closely related, our findings fit to the theoretical literature on bounded rationality and as-if discounting (Rubinstein, 2003; Gabaix and Laibson, 2017). In particular, our finding that concentration bias partly hinges on the accessibility of utility outcomes may link to Gabaix and Laibson (2017), who assume that individuals discount future outcomes because they can only imperfectly access their utility impact and have to simulate it.

Moreover, our findings have potential implications for economic policy. The success of policy interventions in mitigating biases in intertemporal choice crucially depends on the origin of the biases. Our findings call for different policies than standard discounting models as both the degree of concentration and the accessibility—the framing—of outcomes crucially affect intertemporal choice. The unifying idea of the suggestions that we make below is that policies designed to improve individuals' decision making have to counteract the differential weights that decision outcomes receive according to focusing. A way to achieve this is using frames in order to let unbalanced trade-offs appear more like balanced trade-offs: highlighting the *overall* consequences of individual decisions might lead to—seemingly—more balanced choice situations. This would reduce the bias of an individual whose behaviour is described well by the focusing model (Kőszegi and Szeidl, 2013) but is irrelevant for a standard economic agent.

As a first example, to encourage people to increase pension savings, the total value of the retirement savings at the time of entering retirement should be reported as a lump sum instead of being reported as an annuity—even if both options are available, that is, if the savings can be paid lump-sum or be annuitized. Second, and similarly, campaigns for healthy life styles could focus on quantifying the consequences of unhealthy behavior in terms of the total treatment costs over one's entire life—for instance, the cost of treating chronic diseases such as diabetes to reduce excessive consumption of sugar. Consistent with this view, making pictures on cigarette packs that illustrate the severe health consequences of smoking mandatory may reduce smoking by highlighting concentrated consequences. Third, our findings on intertemporal choice also may be applicable to other domains in which choice attributes are not points in time, but, for instance, different price components. Splitting prices into small portions can be regarded as some form of shrouding that tricks individuals who behave in line with the Kőszegi–Szeidl model into excessive consumption. These individuals may make better decisions if the reporting of total prices of product bundles is enforced.

In summary, policy goals might be reached by regulation that targets the frame in which choice consequences are presented. Thereby, framing interventions might nudge individuals who exhibit focusing toward better decisions.

Appendix 1.A Derivation of Theoretical Predictions

1.A.1 Theorem 1: Identical predictions by standard discounting and focusing for the balanced choice blocks

All decisions used to elicit the indifference points in the eight balanced trade-offs have the same underlying structure: Each binary choice belonging to block $j \in \{1, \ldots, 8\}$ is between option $\mathrm{A}^j = (e_1, \ldots, e_j + w_j, \ldots, e_8; v^{j+1})$ and option $\mathrm{B}^j = (e_1, \ldots, e_8; v^j)$. It holds that $w_j \geq 0$ and $v^{j+1} > v^j$.

According to the focusing model by Kőszegi and Szeidl (2013), choice amounts to comparing focus-weighted utility for option A^j,

$$\tilde{U}(\mathrm{A}^j, \{\mathrm{A}^j, \mathrm{B}^j\}) \quad = \quad g_j\, u_j(-e_j - w_j) \; + \left(\sum_{t=1,\, t \neq j}^{8} g_t\, u_t(-e_t) \right) + g_9\, u_9(v^{j+1}), \quad (1.\mathrm{A}.1)$$

with focus-weighted utility for option B^j,

$$\tilde{U}(\mathrm{B}^j, \{\mathrm{A}^j, \mathrm{B}^j\}) \quad = \quad g_j\, u_j(-e_j) \qquad + \left(\sum_{t=1,\, t \neq j}^{8} g_t\, u_t(-e_t) \right) + g_9\, u_9(v^j). \quad (1.\mathrm{A}.2)$$

Here, the focus weights are given by

$$g_j \quad = \quad g[u_j(-e_j) - u_j(-e_j - w_j)] \quad (1.\mathrm{A}.3)$$

for day j, $g_9 = g[u_9(v^{j+1}) - u_9(v^j)]$ for day 9, and $g_t = g[u_t(-e_t) - u_t(-e_t)] = g[0]$ for all other periods. Since the outcomes for these latter periods are identical for both options, the comparison boils down to

$$g_j\, u_j(-e_j - w_j) + g_9\, u_9(v^{j+1}) \quad \lesseqqgtr \quad g_j\, u_j(-e_j) + g_9\, u_9(v^j). \quad (1.\mathrm{A}.4)$$

According to Proposition 3 of Kőszegi and Szeidl (2013, p. 66), a focussed thinker chooses option A over option B whenever she would do the same under discounted utility, that is if and only if $u_j(-e_j - w_j) + u_9(v^{j+1}) > u_j(-e_j) + u_9(v^j)$. The reason is that these two options represent a balanced trade-off and span the per-period range of utilities for all periods. The decision maker obtains a larger utility difference for the period with the greater advantage, and focusing merely amplifies this advantage of the preferred option via the respective focus weight.

Similarly, focusing does not alter the indifference point of the individual: At indifference according to standard discounting, it holds that

$$u_j(-e_j - w_j) + u_9(v^{j+1}) \;=\; u_j(-e_j) + u_9(v^j) \quad \Longleftrightarrow \quad u_9(v^{j+1}) - u_9(v^j) \;=\; u_j(-e_j) - u_j(-e_j + w_j).$$

Since the left-hand side of the latter expression is the utility range for period 9 and the right-hand side is the utility range for period j, focus weights are also of identical size for both periods. Hence, they can be dropped from the equation (see Kőszegi and Szeidl, 2013, p. 92). Thus, indifferences coincide for discounted utility and focusing in balanced pairwise choices. This gives Theorem 1.

We denote by $w_t^\star$ the number of tasks that makes an individual indifferent between options A and B in a balanced decision. That is, $w_t^\star$ for $t = 1, \ldots, 8$ are the workloads that fulfil the condition

$$u_t(-e_t - w_t^\star) + u_9(v^{t+1}) \quad = \quad u_t(-e_t) + u_9(v^t). \tag{1.A.5}$$

1.A.2 Theorem 2: Prediction of standard discounting for the unbalanced choice block

According to discounted utility, individuals should be indifferent between options A and B in the ninth, unbalanced decision for the exact same amounts $w_t^\star$ as in the balanced decisions. Denote by $w_t^{9\star}$, $t = 1, \ldots, 8$, the number of tasks that makes an individual indifferent between options A and B in the unbalanced decision. To see why $w_t^{9\star} = w_t^\star$ gives rise to indifference according to standard discounting also in the unbalanced choice, consider the following equation that expresses the indifference in the unbalanced trade-off:

$$\left(\sum_{t=1}^{8} u_t(-e_t - w_t^{9\star})\right) + u_9(v^9) \quad = \quad \left(\sum_{t=1}^{8} u_t(-e_t)\right) + u_9(v^1). \tag{1.A.6}$$

By inserting Equation 1.A.5 iteratively for $t = 1$, $t = 2$, …, the right-hand side of Equation 1.A.6 can be written as $\left(\sum_{t=1}^{8} u_t(-e_t - w_t^\star)\right) + u_9(v^9)$ so that Equation 1.A.6 becomes

$$\sum_{t=1}^{8} u_t(-e_t - w_t^{9\star}) \quad = \quad \sum_{t=1}^{8} u_t(-e_t - w_t^\star). \tag{1.A.7}$$

This is true if (but not only if) $w_t^{9\star} = w_t^\star$ for $t = 1, \ldots, 8$, that is, $\boldsymbol{w}^{\text{unbal}\star} = \boldsymbol{w}^{\text{bal}\star}$. Since we only allow uniform changes of all w_t^9 by adding the same integer to $w_t^\star$ on all workdays t, we can ignore indifference points where $w_t^{9\star}$ deviates in different directions from $w_t^\star$ for some t. We therefore obtain Theorem 2.

1.A.3 Prediction 1: Prediction of focusing for the unbalanced choice block

To prove Prediction 1—that focussed thinkers are indifferent for a higher workload in the unbalanced choice block than in the balanced choice blocks, $\boldsymbol{w}^{\text{unbal}\star} > \boldsymbol{w}^{\text{bal}\star}$—we take the indifference points that are predicted by standard discounted utility,

$w_t^{9\star} = w_t^\star$, as the point of departure. According to the focusing model (Kőszegi and Szeidl, 2013), the comparison at $w_t^9 = w_t^\star$ is

$$\left(\sum_{t=1}^{8} g_t\, u_t(-e_t - w_t^\star)\right) + g_9\, u_9(v^9) \quad \lesseqqgtr \quad \left(\sum_{t=1}^{8} g_t\, u_t(-e_t)\right) + g_9\, u_9(v^1), \qquad \text{(1.A.8)}$$

where $g_t = g[u_t(-e_t) - u_t(-e_t - w_t^\star)]$ for $t = 1,\dots,8$ and $g_9 = g[u_9(v^9) - u_9(v^1)]$. Rearranging yields

$$g_9 \times (u_9(v^9) - u_9(v^1)) \quad \lesseqqgtr \quad \sum_{t=1}^{8} g_t \times (u_t(-e_t) - u_t(-e_t - w_t^\star)). \qquad \text{(1.A.9)}$$

Note that for $t = 1,\dots,8$, the utility range of period t in Equation 1.A.9 coincides with the utility range of the same period t in the corresponding balanced decision, see Equation 1.A.3, when (1.A.3) is evaluated at the indifference point $w_t = w_t^\star$. Thus, using Equation 1.A.5, we can replace the right-hand side of (1.A.9) by

$$\sum_{t=1}^{8} g[u_9(v^{t+1}) - u_9(v^t)] \times (u_9(v^{t+1}) - u_9(v^t)). \qquad \text{(1.A.10)}$$

Given that

$$g[u_9(v^{t+1}) - u_9(v^t)] \;<\; g[u_9(v^9) - u_9(v^1)] \;=\; g_9,$$

expression (1.A.10) is less than

$$\sum_{t=1}^{8} g_9 \times (u_9(v^{t+1}) - u_9(v^t)) \;=\; g_9 \times \sum_{t=1}^{8} (u_9(v^{t+1}) - u_9(v^t)) \;=\; g_9 \times (u_9(v^9) - u_9(v^1)).$$

This, in turn, is nothing else than the left-hand side of Equation 1.A.9. Hence, we have established that

$$\sum_{t=1}^{8} g_t\, u_t(-e_t - w_t^\star) + g_9\, u_9(v^9) \quad > \quad \sum_{t=1}^{8} g_t\, u_t(-e_t) + g_9\, u_9(v^1). \qquad \text{(1.A.11)}$$

Put into words, focussed individuals prefer Option A over B for efforts $w_t^\star$ in the unbalanced decision—in contrast to the prediction of standard discounting, which postulates indifference between A and B for $w_t^\star$.

For individuals to become indifferent under focusing, the effort required by Option A has to be greater. Hence, for the indifference point $w_t^{9\star}$ it holds that $w_t^{9\star} > w_t^\star$ for $t = 1,\dots,8$, that is, $\boldsymbol{w}^{\text{unbal}\star} > \boldsymbol{w}^{\text{bal}\star}$. Recall that we only allow simultaneous and uniform changes of all w_t^9 in our experiment so that we can ignore indifference points where $w_t^{9\star}$ deviates in different directions from $w_t^\star$ for some t. By how much the indifference point $w_t^{9\star}$ exceeds $w_t^\star$ depends on the strength of focusing—that is, the steepness of the function g—in combination with the marginal disutility due to increased effort. Increasing the effort in itself makes Option A less attractive but also boosts the weighting of the period(s) in which effort is increased. Indifference under focusing is reached when these two components offset the disproportionately large focus weight attached to the bonus on date 9. This gives rise to Prediction 1.

Appendix 1.B Procedural Details of the Experimental Design

1.B.1 Elicitation of indifference points via repeated pairwise choice

Each choice block consists of 126 choices. However, subjects only make a subset of up to nine choices *directly*, while the remaining choices are made *indirectly* according to the following rule: if a subject accepts option A for a particular number of tasks, then she will also accept option A for any lower number of tasks; conversely, if a subject rejects option A for a particular number of tasks, then she will also reject option A for any larger number of tasks. This procedure has the advantage that it allows us to elicit indifference points with a low number of direct choices despite being able to cover a wide range of individual preferences regarding the work–voucher trade-off. In addition, the low number of direct choices prevents that subjects get bored and tired over the course of the experiment. The method simultaneously ensures single switching so that indifference is unambiguously determined.

The first direct choice of each choice block is randomly chosen by the computer from all 126 possible pairwise choices in that block. This feature allows us to assess to which degree there is randomness in subjects' decisions: if their preferences are well-defined, the initial pairwise choice should have no influence on the elicited indifference point. The remaining direct choices are determined on the fly so that the expected number of direct choices that a subject has to make until she reaches her indifference point is minimized.[17] Importantly, one of all direct *and* indirect choices is randomly selected to be the payoff-relevant "decision that counts" in the end. All choices have the same probability of being selected, irrespective of being a direct or indirect choice. Hence, subjects are incentivized to report truthfully.[18]

1.B.2 Conditional manual elicitation

Participants who are extremely eager to work—that is, they always choose option A in a choice block—are asked to state their indifference manually for that choice block (see Figure 1.C.2 in Appendix 1.C for a screenshot).

In the eight *balanced* choice blocks, we applied this conditional manual elicitation despite the exclusion criterion in order to collect a data set that is as comprehensive as possible. This is because the manual elicitation of the indifference point allows us to construct the unbalanced choice block also for subjects with upper corner choices in the first eight choice blocks. The findings that we report in Section 3.5 are robust to including these subjects in our analyses.

17. This is achieved by selecting as the next direct choice the midpoint of the interval that has not been covered by the indirect choices yet.

18. Our procedure is logically equivalent to making choices in a choice list with 126 rows and enforced unique switching. We avoided using a choice list in order not to induce a potential tendency to switch close to the list's centre.

In the final, *unbalanced* choice block, applying the manual elicitation of indifference to subjects with corner choices allows us to bound their deviation from the point prediction of standard discounting. That is, in the regression tables included in Section 3.5, the estimates labelled "Lower bound" are based on the final direct choice from the set of 126 pairwise choices, while the estimates labelled "Upper bound" are based on subjects' manually stated indifference points.

Appendix 1.C Additional Table and Figures

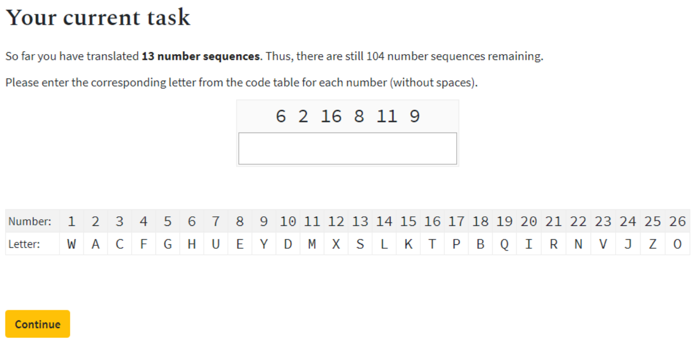
1.C.1 Additional table

Table 1.C.1. Quantitative estimates of concentration bias: Analysis for the MECHANISM conditions

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting					
	Lower bound		Upper bound		Mean	
	(1)	(2)	(3)	(4)	(5)	(6)
1 if TREATMENT, 0 if CONTROL	0.083*** (0.020)	0.090*** (0.019)	0.097*** (0.023)	0.105*** (0.023)	0.090*** (0.021)	0.098*** (0.021)
Constant	−0.014 (0.010)	−0.018 (0.011)	−0.014 (0.010)	−0.018 (0.011)	−0.014 (0.010)	−0.018 (0.011)
Cognitive controls		Yes		Yes		Yes
Age group FE		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes
Session FE		Yes		Yes		Yes
Observations	200	200	200	200	200	200
Adjusted R^2	0.074	0.082	0.076	0.077	0.077	0.082

Notes: This table presents OLS estimates of the treatment difference in the MECHANISM conditions for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MECHANISM-TREATMENT and equals 0 if MECHANISM-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean of the two. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Columns (2), (4), and (6) additionally include cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MECHANISM-TREATMENT and MECHANISM-CONTROL. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

1.C.2 Additional figures



Your current task

So far you have translated **13 number sequences**. Thus, there are still 104 number sequences remaining.

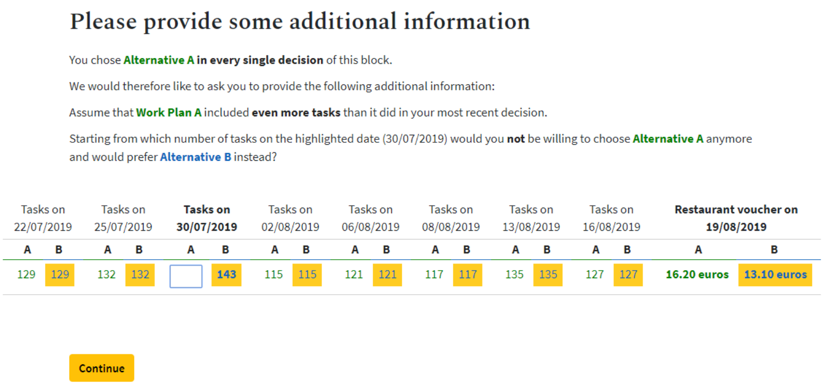
Please enter the corresponding letter from the code table for each number (without spaces).

6 2 16 8 11 9

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

Continue

Figure 1.C.1. A screenshot of the real-effort task. The solution would be “HATEMY” in this case.



Please provide some additional information

You chose **Alternative A in every single decision** of this block.

We would therefore like to ask you to provide the following additional information:

Assume that **Work Plan A** included **even more tasks** than it did in your most recent decision.

Starting from which number of tasks on the highlighted date (30/07/2019) would you **not** be willing to choose **Alternative A** anymore and would prefer **Alternative B** instead?

Tasks on 22/07/2019		Tasks on 25/07/2019		**Tasks on 30/07/2019**		Tasks on 02/08/2019		Tasks on 06/08/2019		Tasks on 08/08/2019		Tasks on 13/08/2019		Tasks on 16/08/2019		**Restaurant voucher on 19/08/2019**	
A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B	A	B
129	129	132	132		143	115	115	121	121	117	117	135	135	127	127	16.20 euros	13.10 euros

Continue

Figure 1.C.2. A screenshot of a screen for entering an indifference point manually.

Appendix 1.D Complete Sample

Table 1.D.1. Estimates of concentration bias for the complete sample: average absolute per-workday deviation

	Dependent variable: Average absolute per-workday deviation of real-effort tasks from point prediction of standard discounting						
	OLS			Tobit	Median regression		
	(1) Lower bound	(2) Upper bound	(3) Mean	(4)	(5) Lower bound	(6) Upper bound	(7) Mean
1 if TREATMENT 0 if CONTROL	35.767*** (3.294)	50.794*** (5.571)	43.281*** (4.261)	43.373*** (4.209)	48.000*** (4.869)	50.000*** (4.970)	48.000*** (4.920)
Constant	0.659 (2.127)	3.015 (3.152)	1.837 (2.535)	1.145 (2.883)	0.000 (3.449)	0.000 (3.521)	0.000 (3.485)
Observations	271	271	271	271	271	271	271
Adjusted R^2	0.302	0.233	0.274				
Pseudo R^2				0.041	0.247	0.181	0.209

Notes: This table presents estimates of the treatment difference for the average absolute per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. Columns (1), (2), and (3) present OLS regressions of our dependent variable on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. Column (4) presents an analogous Tobit regression. Columns (5), (6), and (7) present analogous median regressions. The table provides estimates for a lower bound in columns (1) and (5), for an upper bound in columns (2) and (6) and for the mean of the two in columns (3) and (7) for the OLS and median regressions. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL, also containing those subjects that have at least one corner choice within the first eight choice blocks. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.D.2. Quantitative estimates of concentration bias for the complete sample: average relative per-workday deviation

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting					
	OLS			Median regression		
	(1) Lower bound	(2) Upper bound	(3) Mean	(4) Lower bound	(5) Upper bound	(6) Mean
1 if TREATMENT	0.212***	0.281***	0.247***	0.236***	0.240***	0.236***
0 if CONTROL	(0.018)	(0.029)	(0.023)	(0.029)	(0.030)	(0.030)
Constant	−0.023**	−0.023**	−0.023**	−0.008	−0.008	−0.008
	(0.008)	(0.008)	(0.008)	(0.021)	(0.021)	(0.021)
Observations	200	200	200	200	200	200
Adjusted R^2	0.411	0.323	0.373			
Pseudo R^2				0.204	0.161	0.180

Notes: This table presents OLS and median regression estimates of the treatment difference for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean of the two. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL, also containing those subjects that have at least one corner choice within the first eight choice blocks. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.D.3. Quantitative estimates of concentration bias for the complete sample: average relative per-workday deviation, including controls for between-subject heterogeneity

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting					
	(1)	(2)	(3)	(4)	(5)	(6)
1 if TREATMENT, 0 if CONTROL	0.230*** (0.021)	0.231*** (0.021)	0.228*** (0.021)	0.229*** (0.021)	0.230*** (0.021)	0.228*** (0.021)
Avg. RT in blocks 1–8 (standardised)		−0.001 (0.011)				−0.003 (0.011)
Math score (standardised)			−0.008 (0.011)			−0.006 (0.013)
CRT score (standardised)				−0.009 (0.011)		−0.007 (0.012)
Raven score (standardised)					−0.005 (0.009)	−0.001 (0.010)
Constant	−0.000 (0.010)	−0.001 (0.010)	0.001 (0.011)	0.000 (0.011)	−0.000 (0.010)	0.001 (0.011)
Age group FE	Yes	Yes	Yes	Yes	Yes	Yes
Gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	271	271	271	271	271	271
Adjusted R^2	0.334	0.331	0.333	0.333	0.332	0.326

Notes: This table presents OLS estimates of the treatment difference for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for the mean and includes standardised controls for between-subject heterogeneity. Columns (2)–(5) include the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, respectively. Column (6) includes all four controls concurrently. All columns additionally include fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL, also containing those subjects that have at least one corner choice within the first eight choice blocks. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.D.4. Quantitative estimates of concentration bias for the complete sample: average relative per-workday deviation, accounting for noise in participants' choices

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting	
	(1)	(2)
1 if TREATMENT, 0 if CONTROL	0.232***	0.229***
	(0.020)	(0.021)
Initial choice row in block 9 for MAIN-TREATMENT (standardised)	−0.016	−0.018
	(0.018)	(0.019)
Initial choice row in block 9 for MAIN-CONTROL (standardised)	−0.013	−0.018
	(0.009)	(0.010)
Constant	−0.001	0.001
	(0.010)	(0.011)
Cognitive controls		Yes
Age group FE		Yes
Gender FE		Yes
Session FE		Yes
Observations	271	271
Adjusted R^2	0.335	0.329

Notes: This table presents OLS estimates of the treatment difference for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for the mean and controls for the noise in participants' choice by including the standardised initial choice row in decision block 9 for the respective condition. Column (2) additionally includes cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL, also containing those subjects that have at least one corner choice within the first eight choice blocks. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.D.5. Heterogeneity of concentration bias for the complete sample

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if TREATMENT, 0 if CONTROL	0.232***	0.231***	0.232***	0.231***	0.232***	0.230***	0.232***	0.230***
	(0.019)	(0.019)	(0.020)	(0.021)	(0.020)	(0.021)	(0.020)	(0.021)
Avg. RT in block 9 relative to blocks 1–8 for MAIN-TREATMENT (standardised)	−0.082***	−0.078***						
	(0.016)	(0.018)						
Avg. RT in block 9 relative to blocks 1–8 for MAIN-CONTROL (standardised)	0.018*	0.014						
	(0.009)	(0.010)						
Math score for MAIN-TREATMENT (standardised)			−0.016	−0.013				
			(0.019)	(0.020)				
Math score for MAIN-CONTROL (standardised)			−0.011	−0.004				
			(0.008)	(0.011)				
CRT score for MAIN-TREATMENT (standardised)					−0.023	−0.018		
					(0.018)	(0.018)		
CRT score for MAIN-CONTROL (standardised)					−0.001	0.000		
					(0.010)	(0.010)		
Raven score for MAIN-TREATMENT (standardised)							−0.016	−0.003
							(0.015)	(0.017)
Raven score for MAIN-CONTROL (standardised)							−0.012	−0.007
							(0.009)	(0.009)
Constant	−0.001	−0.001	−0.001	−0.001	−0.001	−0.000	−0.001	−0.000
	(0.010)	(0.011)	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)
Age group FE		Yes		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes		Yes
Session FE		Yes		Yes		Yes		Yes
Observations	271	271	271	271	271	271	271	271
Adjusted R^2	0.419	0.406	0.335	0.331	0.336	0.333	0.335	0.329

Notes: This table presents OLS estimates of the treatment difference for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for the mean and within-condition heterogeneity of the effect. Columns (1) and (2) include the average response time in block 9 relative to the first eight decision blocks, columns (3) and (4) a measure for math ability, columns (5) and (6) a measure for the Cognitive Reflection Test, and columns (7) and (8) a measure for the Raven Progressive Matrices task, all standardised by condition. Columns (2), (4), (6), and (8) additionally include fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL, also containing those subjects that have at least one corner choice within the first eight choice blocks. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.D.6. Quantitative estimates of concentration bias for the complete sample: Analysis for the MECHANISM conditions

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting					
	Lower bound		Upper bound		Mean	
	(1)	(2)	(3)	(4)	(5)	(6)
1 if TREATMENT, 0 if CONTROL	0.084*** (0.019)	0.092*** (0.018)	0.101*** (0.022)	0.110*** (0.022)	0.093*** (0.020)	0.101*** (0.020)
Constant	−0.014 (0.010)	−0.018 (0.011)	−0.012 (0.011)	−0.016 (0.011)	−0.013 (0.010)	−0.017 (0.011)
Cognitive controls		Yes		Yes		Yes
Age group FE		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes
Session FE		Yes		Yes		Yes
Observations	248	248	248	248	248	248
Adjusted R^2	0.073	0.070	0.076	0.076	0.076	0.075

Notes: This table presents OLS estimates of the treatment difference in the MECHANISM conditions for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MECHANISM-TREATMENT and equals 0 if MECHANISM-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean of the two. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Columns (2), (4), and (6) additionally include cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MECHANISM-TREATMENT and MECHANISM-CONTROL, also containing those subjects that have at least one corner choice within the first eight choice blocks. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table 1.D.7. Quantitative estimates of concentration bias for the complete sample: Difference-in-differences analysis for the conditions MAIN-TREATMENT, MAIN-CONTROL, MECHANISM-TREATMENT, and MECHANISM-CONTROL

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting	
	(1)	(2)
1 if TREATMENT, 0 if CONTROL	0.093***	0.098***
	(0.020)	(0.020)
1 if MAIN, 0 if MECHANISM	0.012	0.028
	(0.014)	(0.056)
{1 if TREATMENT, 0 if CONTROL} ×	0.139***	0.132***
{1 if MAIN, 0 if MECHANISM}	(0.028)	(0.029)
Constant	−0.013	−0.022
	(0.010)	(0.029)
Cognitive controls		Yes
Age group FE		Yes
Gender FE		Yes
Session FE		Yes
Observations	519	519
Adjusted R^2	0.271	0.267

Notes: This table presents OLS estimates of the treatment difference, as well as the difference between MAIN and MECHANISM, for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if TREATMENT and equals 0 if CONTROL, a second dummy that equals 1 if MAIN and equals 0 if MECHANISM, and an interaction dummy that equals 1 if MAIN-TREATMENT and equals 0 otherwise. The table provides estimates for the mean. Column (2) additionally includes cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT, MAIN-CONTROL, MECHANISM-TREATMENT, and MECHANISM-CONTROL, also containing those subjects that have at least one corner choice within the first eight choice blocks. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.D.8. Quantitative estimates of concentration bias for the complete sample: Analysis for the DONATION conditions

	Dependent variable: Average relative per-workday deviation of real-effort tasks from point prediction of standard discounting					
	Lower Bound		Upper Bound		Mean	
	(1)	(2)	(3)	(4)	(5)	(6)
1 if TREATMENT, 0 if CONTROL	0.135***	0.136***	0.181***	0.182***	0.158***	0.159***
	(0.018)	(0.018)	(0.023)	(0.023)	(0.020)	(0.020)
Constant	−0.010	−0.010	−0.010	−0.010	−0.010	−0.010
	(0.009)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Cognitive controls		Yes		Yes		Yes
Age group FE		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes
Session FE		Yes		Yes		Yes
Observations	250	250	250	250	250	250
Adjusted R^2	0.195	0.200	0.202	0.196	0.204	0.203

Notes: This table presents OLS estimates of the treatment difference in the DONATION conditions for the average relative per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if DONATION-TREATMENT and equals 0 if DONATION-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Columns (2), (4), and (6) additionally include cognitive controls with the average response time for the first eight decision blocks and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from DONATION-TREATMENT and DONATION-CONTROL, also containing those subjects that have at least one corner choice within the first eight choice blocks. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Appendix 1.E Alternative Quantitative Measure

Table 1.E.1. Alternative quantitative estimates of concentration bias: relative aggregated per-workday deviation

	Dependent variable: Relative aggregated per-workday deviation of real-effort tasks from point prediction of standard discounting					
	OLS			Median regression		
	(1) Lower bound	(2) Upper bound	(3) Mean	(4) Lower bound	(5) Upper bound	(6) Mean
1 if TREATMENT	0.215***	0.283***	0.248***	0.235***	0.239***	0.236***
0 if CONTROL	(0.018)	(0.029)	(0.023)	(0.028)	(0.030)	(0.030)
Constant	−0.027**	−0.027**	−0.025**	−0.007	−0.007	−0.008
	(0.009)	(0.009)	(0.008)	(0.020)	(0.021)	(0.021)
Observations	200	200	200	200	200	200
Adjusted R^2	0.412	0.327	0.374			
Pseudo R^2				0.203	0.160	0.179

Notes: This table presents OLS and median regression estimates of the treatment difference for the relative aggregated per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MAIN-TREATMENT and equals 0 if MAIN-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean of the two. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT and MAIN-CONTROL. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.E.2. Alternative quantitative estimates of concentration bias: relative aggregated per-workday deviation, including controls for between-subject heterogeneity

	Dependent variable: Relative aggregated per-workday deviation of real-effort tasks from point prediction of standard discounting					
	(1)	(2)	(3)	(4)	(5)	(6)
1 if Treatment, 0 if Control	0.247*** (0.023)	0.246*** (0.024)	0.244*** (0.024)	0.246*** (0.024)	0.247*** (0.024)	0.244*** (0.025)
Avg. RT in blocks 1–8 (standardised)		0.002 (0.011)				0.000 (0.012)
Math score (standardised)			−0.009 (0.015)			−0.010 (0.017)
CRT score (standardised)				−0.004 (0.012)		−0.003 (0.013)
Raven score (standardised)					0.001 (0.011)	0.005 (0.013)
Constant	−0.024* (0.010)	−0.024* (0.011)	−0.023* (0.010)	−0.024* (0.010)	−0.024* (0.010)	−0.022* (0.011)
Age group FE	Yes	Yes	Yes	Yes	Yes	Yes
Gender FE	Yes	Yes	Yes	Yes	Yes	Yes
Session FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	200	200	200	200	200	200
Adjusted R^2	0.364	0.360	0.362	0.361	0.360	0.352

Notes: This table presents OLS estimates of the treatment difference for the relative aggregated per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if Main-Treatment and equals 0 if Main-Control. The table provides estimates for the mean and includes standardised controls for between-subject heterogeneity. Columns (2)–(5) include the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, respectively. Column (6) includes all four controls concurrently. All columns additionally include fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from Main-Treatment and Main-Control. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.E.3. Alternative quantitative estimates of concentration bias: relative aggregated per-workday deviation, accounting for noise in participants' choices

	Dependent variable: Relative aggregated per-workday deviation of real-effort tasks from point prediction of standard discounting	
	(1)	(2)
1 if Treatment, 0 if Control	0.249***	0.245***
	(0.023)	(0.025)
Initial choice row in block 9 for Main-Treatment (standardised)	−0.018	−0.017
	(0.023)	(0.023)
Initial choice row in block 9 for Main-Control (standardised)	−0.010	−0.014
	(0.008)	(0.010)
Constant	−0.024**	−0.023*
	(0.008)	(0.011)
Cognitive controls		Yes
Age group FE		Yes
Gender FE		Yes
Session FE		Yes
Observations	200	200
Adjusted R^2	0.373	0.351

Notes: This table presents OLS estimates of the treatment difference for the relative aggregated per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if Main-Treatment and equals 0 if Main-Control. The table provides estimates for the mean and controls for the noise in participants' choice by including the standardised initial choice row in decision block 9 for the respective condition. Column (2) additionally includes cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from Main-Treatment and Main-Control. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.E.4. Heterogeneity of concentration bias with an alternative quantitative measure

	Dependent variable: Relative aggregated per-workday deviation of real-effort tasks from point prediction of standard discounting							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 if Treatment, 0 if Control	0.248***	0.246***	0.248***	0.247***	0.248***	0.247***	0.248***	0.247***
	(0.021)	(0.021)	(0.023)	(0.023)	(0.023)	(0.024)	(0.023)	(0.024)
Avg. RT in block 9 relative to blocks 1–8 for Main-Treatment (standardised)	−0.094***	−0.094***						
	(0.020)	(0.021)						
Avg. RT in block 9 relative to blocks 1–8 for Main-Control (standardised)	0.013	0.013						
	(0.008)	(0.010)						
Math score for Main-Treatment (standardised)			−0.017	−0.015				
			(0.026)	(0.027)				
Math score for Main-Control (standardised)			−0.007	−0.004				
			(0.008)	(0.013)				
CRT score for Main-Treatment (standardised)					−0.017	−0.015		
					(0.021)	(0.021)		
CRT score for Main-Control (standardised)					0.010	0.007		
					(0.009)	(0.011)		
Raven score for Main-Treatment (standardised)							−0.010	0.001
							(0.020)	(0.022)
Raven score for Main-Control (standardised)							0.000	0.001
							(0.008)	(0.010)
Constant	−0.025**	−0.024*	−0.025**	−0.024*	−0.025**	−0.024*	−0.025**	−0.024*
	(0.008)	(0.010)	(0.009)	(0.010)	(0.008)	(0.010)	(0.009)	(0.010)
Age group FE		Yes		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes		Yes
Session FE		Yes		Yes		Yes		Yes
Observations	200	200	200	200	200	200	200	200
Adjusted R^2	0.481	0.471	0.372	0.360	0.373	0.360	0.369	0.357

Notes: This table presents OLS estimates of the treatment difference for the relative aggregated per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if Main-Treatment and equals 0 if Main-Control. The table provides estimates for the mean and within-condition heterogeneity of the effect. Columns (1) and (2) include the average response time in block 9 relative to the first eight decision blocks, columns (3) and (4) a measure for math ability, columns (5) and (6) a measure for the Cognitive Reflection Test, and columns (7) and (8) a measure for the Raven Progressive Matrices task, all standardised by condition. Columns (2), (4), (6) and (8) additionally include fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from Main-Treatment and Main-Control. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.E.5. Alternative quantitative estimates of concentration bias: Analysis for the MECHANISM conditions

	Dependent variable: Relative aggregated per-workday deviation of real-effort tasks from point prediction of standard discounting					
	Lower bound		Upper bound		Mean	
1 if TREATMENT, 0 if CONTROL	0.085***	0.092***	0.099***	0.107***	0.091***	0.098***
	(0.020)	(0.019)	(0.023)	(0.022)	(0.021)	(0.021)
Constant	−0.017	−0.021	−0.017	−0.021	−0.016	−0.020
	(0.010)	(0.011)	(0.010)	(0.011)	(0.010)	(0.011)
Cognitive controls		Yes		Yes		Yes
Age group FE		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes
Session FE		Yes		Yes		Yes
Observations	200	200	200	200	200	200
Adjusted R^2	0.081	0.088	0.083	0.082	0.080	0.084

Notes: This table presents OLS estimates of the treatment difference in the MECHANISM conditions for the relative aggregated per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if MECHANISM-TREATMENT and equals 0 if MECHANISM-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean of the two. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Columns (2), (4) and (6) additionally include cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MECHANISM-TREATMENT and MECHANISM-CONTROL. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.E.6. Alternative quantitative estimates of concentration bias: Difference-in-differences analysis for the conditions MAIN-TREATMENT, MAIN-CONTROL, MECHANISM-TREATMENT, and MECHANISM-CONTROL

	Dependent variable: Relative aggregated per-workday deviation of real-effort tasks from point prediction of standard discounting	
	(1)	(2)
1 if TREATMENT, 0 if CONTROL	0.091***	0.096***
	(0.021)	(0.021)
1 if MAIN, 0 if MECHANISM	−0.009	−0.051
	(0.013)	(0.061)
{1 if TREATMENT, 0 if CONTROL} × {1 if MAIN, 0 if MECHANISM}	0.157***	0.152***
	(0.031)	(0.032)
Constant	−0.016	0.004
	(0.010)	(0.031)
Cognitive controls		Yes
Age group FE		Yes
Gender FE		Yes
Session FE		Yes
Observations	400	400
Adjusted R^2	0.289	0.282

Notes: This table presents OLS estimates of the treatment difference, as well as the difference between MAIN and MECHANISM, for the relative aggregated per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if TREATMENT and equals 0 if CONTROL, a second dummy that equals 1 if MAIN and equals 0 if MECHANISM, and an interaction dummy that equals 1 if MAIN-TREATMENT and equals 0 otherwise. The table provides estimates for the mean. Column (2) additionally includes cognitive controls with the average response time for the first eight decision blocks, a measure for math ability, a measure for the Cognitive Reflection Test, and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from MAIN-TREATMENT, MAIN-CONTROL, MECHANISM-TREATMENT, and MECHANISM-CONTROL. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 1.E.7. Alternative quantitative estimates of concentration bias: Analysis for the DONATION conditions

	Dependent variable: Relative aggregated per-workday deviation of real-effort tasks from point prediction of standard discounting					
	Lower Bound		Upper Bound		Mean	
1 if TREATMENT, 0 if CONTROL	0.140***	0.139***	0.179***	0.177***	0.158***	0.157***
	(0.019)	(0.020)	(0.025)	(0.026)	(0.022)	(0.022)
Constant	−0.026**	−0.026**	−0.026**	−0.025**	−0.025**	−0.024**
	(0.008)	(0.009)	(0.008)	(0.010)	(0.008)	(0.009)
Cognitive controls		Yes		Yes		Yes
Age group FE		Yes		Yes		Yes
Gender FE		Yes		Yes		Yes
Session FE		Yes		Yes		Yes
Observations	200	200	200	200	200	200
Adjusted R^2	0.204	0.200	0.197	0.193	0.203	0.199

Notes: This table presents OLS estimates of the treatment difference in the DONATION conditions for the relative aggregated per-workday deviation of the number of real-effort tasks from the point prediction of standard discounting. The dependent variable is regressed on a condition dummy that equals 1 if DONATION-TREATMENT and equals 0 if DONATION-CONTROL. The table provides estimates for a lower and an upper bound, as well as for the mean of the two. The lower bound results from assuming that the indifference point of a subject with a corner choice is the lowest possible indifference point outside the range captured by the repeated pairwise choices. For the upper bound, we assume that the indifference point for the same subjects is the value that they state manually. Columns (2), (4) and (6) additionally includes cognitive controls with the average response time for the first eight decision blocks and a measure for the Raven Progressive Matrices task, fixed effects for age group and gender of the subjects, as well as a session fixed effect. Robust standard errors are in parentheses. The sample includes all observations from DONATION-TREATMENT and DONATION-CONTROL. *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$.

Appendix 1.F Convex Time Budgets: Details

In this section, we discuss the conditions Money-Main and Money-Mechanism of our money experiment in detail. First, we present the design of the experiment in Section 1.F.1 and derive behavioral predictions from discounted utility and from the focusing model (Kőszegi and Szeidl, 2013) in Section 1.F.2. We then report and discuss the findings for Money-Main in Section 1.F.3 and for Money-Mechanism in Section 1.F.4. We introduce a different notation in this appendix for the formal discussion of the experiment. In Section 1.6.2 of the paper, we only use the terms *balanced* and *unbalanced* where for the latter either the earlier or the later payoff is dispersed. To distinguish between these two unbalanced trade-offs, we call these now Disp-Conc or Conc-Disp. The corresponding balanced trade-offs are denoted by Conc-Conc.

1.F.1 Design of the Experiment

1.F.1.1 Intertemporal Choices

This experiment investigates intertemporal decisions that involve multiple periods. In particular, each participant repeatedly allocates monetary budgets between an earlier and a later payoff. One of the choices is randomly chosen to be payoff-relevant at the end of the experiment.

Subjects' decisions are of two different types: either both payoffs are concentrated on a single payment date ("balanced"; Conc-Conc), or one of the two payoffs is dispersed over multiple (2, 4, or 8) payment dates ("unbalanced"; Conc-Disp and Disp-Conc). Figures 1.F.1 and 1.F.2 illustrate the budget sets. Comparing how much subjects allocate to the later payoff between the two types of decisions identifies concentration bias.

Subjects decide for each budget set whether to decrease earlier payments at the benefit of increasing later payments. The sum total is the greater, the more money subjects allocate to later payment dates. Put differently, we implement an intertemporal budget constraint with a strictly positive nominal interest rate, r. Each earnings sequence included in the different budget sets specifies a series of 9 money transfers to subjects' bank accounts at given dates in the future. In doing so, we extend the "Convex Time Budget" approach introduced by Andreoni and Sprenger (2012) to settings in which individuals face more than two payment dates.

Across trials, we vary within-subject the characteristics of the intertemporal budget constraint. Thereby, we implement the decision types Conc-Conc, Conc-Disp and Disp-Conc. Irrespective of the decision, subjects receive a fixed amount of €1 at each of the 9 payment dates, to hold the number of transfers constant across condi-

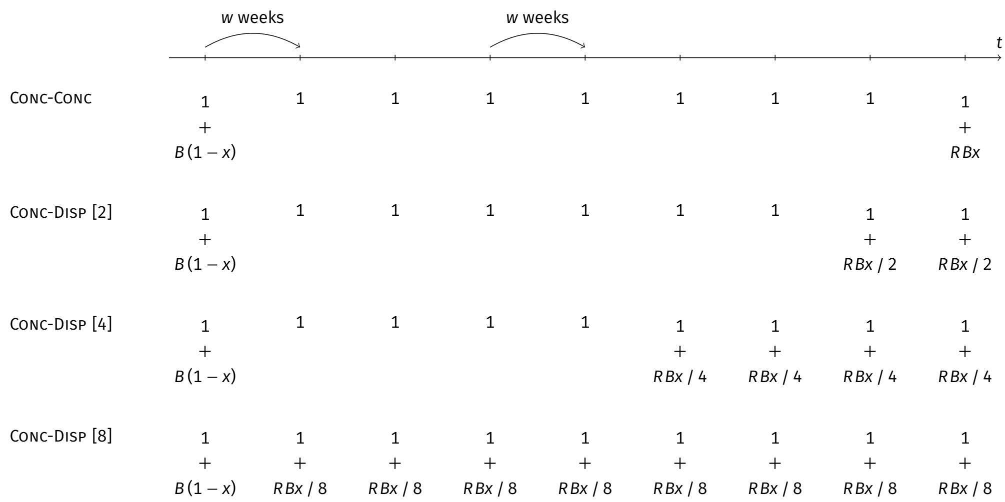


Figure 1.F.1. Budget Sets Conc-Conc and Conc-Disp

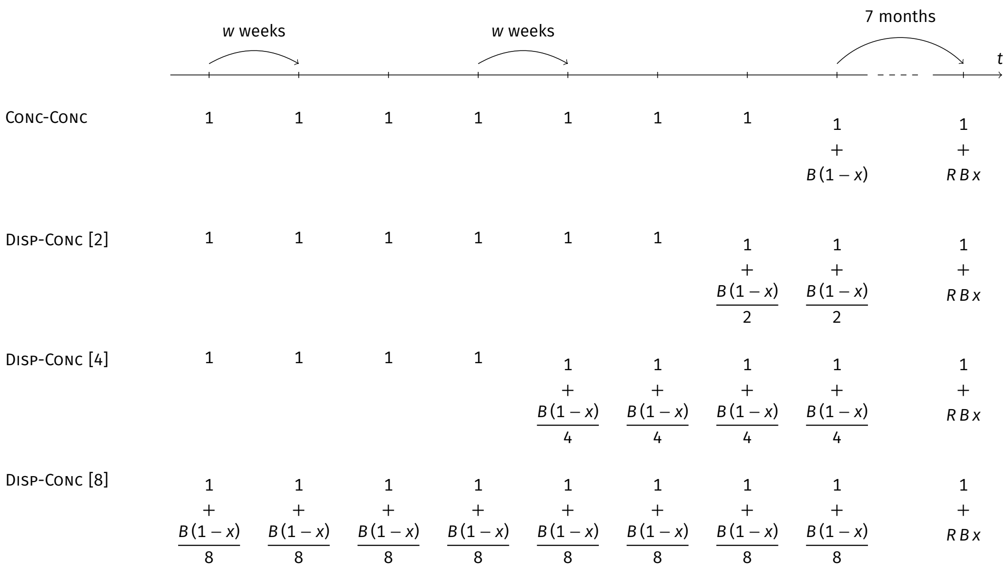


Figure 1.F.2. Budget Sets Conc-Conc and Disp-Conc

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

tions.[19] On top of this, subjects allocate a budget B between several payment dates. In Conc-Conc, the allocation is between exactly two payment dates; the intertemporal allocation thus involves payoffs that are concentrated on a single payment date each. Decreasing a payoff increases a payoff on exactly *one* other date. By contrast, in Conc-Disp and Disp-Conc, there is one payoff that is concentrated on a single date, while the other payoff is dispersed over multiple dates. Decreasing (increasing) the concentrated payoff increases (decreases) the payments on *several* (2, 4, or 8) other dates. To give an example, the earnings sequences in a balanced condition Conc-Conc are

$$\left[1+B(1-x), 1, 1, 1, 1, 1, 1, 1, 1+RBx\right],$$

while the associated earnings sequences in Conc-Disp are

$$\left[1+B(1-x), 1+\frac{RBx}{8}, 1+\frac{RBx}{8}, 1+\frac{RBx}{8}, 1+\frac{RBx}{8}, 1+\frac{RBx}{8}, 1+\frac{RBx}{8}, 1+\frac{RBx}{8}, 1+\frac{RBx}{8}\right].$$

Here, the i^{th} entry specifies the euro amount of the i^{th} payment.

We denote by x the decision maker's choice variable: the share of the budget B that she "saves," i.e., that she allocates to the later payoff, $x \in \boldsymbol{X}$ with $\boldsymbol{X} = \{0, \frac{1}{100}, \frac{2}{100}, \ldots, 1\}$.

Conc-Conc consists of two types of budget sets, each type belonging to either Conc-Disp or Disp-Conc. In the type corresponding to Conc-Disp, subjects can shift money from the earliest to the last payment date at the benefit of receiving interest. In the second one, subjects allocate money between the second-to-last and the last payment date. In every Conc-Conc decision, subjects receive B euros if they allocate their additional payment to the earlier date. If they allocate it to the later date, they receive RB euros, with $R := 1 + r > 1$. They can also choose convex combinations of payments by choosing $x \in \boldsymbol{X} = \{0, \frac{1}{100}, \frac{2}{100}, \ldots, 1\}$, which gives rise to an earlier payment of $B(1-x)$ euros and a later payment of RBx euros. While each payment date is separated by w weeks for the first type, this is true only for the first 8 payment dates for the second type, matching Disp-Conc decisions. Here, the distance between the second-to-last and last payment date is 7 months. We chose this large gap between $t = 8$ and $t = 9$ in order to minimize ceiling effects, i.e., in order to avoid a situation in which subjects exclusively choose the largest, latest payment.

For the unbalanced budget sets, Conc-Disp and Disp-Conc, either the later or earlier payoff is dispersed over $n \in \{2, 4, 8\}$ payment dates. In Conc-Disp, subjects allocate monetary amounts between the earliest payment date and the last n payment dates over which the later payoff is dispersed. More precisely, the amount of

19. This is the modern way to control for transaction costs (Balakrishnan, Haushofer, and Jakiela, 2017, p. 9; Andreoni and Sprenger, 2012, p. 3339). In addition, each subject receives two individualized e-mail messages after the experiment that include a complete listing of all payments. Before making any decisions in the experiment, the written instructions inform subjects in detail about these two messages. Hence, subjects know in advance that they will be informed when exactly they have to inspect their bank statements to check that they have received the promised amounts.

RBx euros paid at the last date in the corresponding Conc-Conc decision is dispersed over the final payment date and the previous $n-1$ dates in Conc-Disp, so that RBx/n euros are being paid at each of these dates. In turn, subjects allocate money between n earlier payments and a single later payment in Disp-Conc. That is, the amount of $B\,(1-x)$ euros paid at the second-to-last date in Conc-Conc is now dispersed over the second-to-last payment date and $n-1$ earlier dates in Disp-Conc. Thus, $B\,(1-x)/n$ euros are being paid at each of these dates. The intervals between payment dates in Conc-Disp and Disp-Conc follow the respective Conc-Conc counterparts.

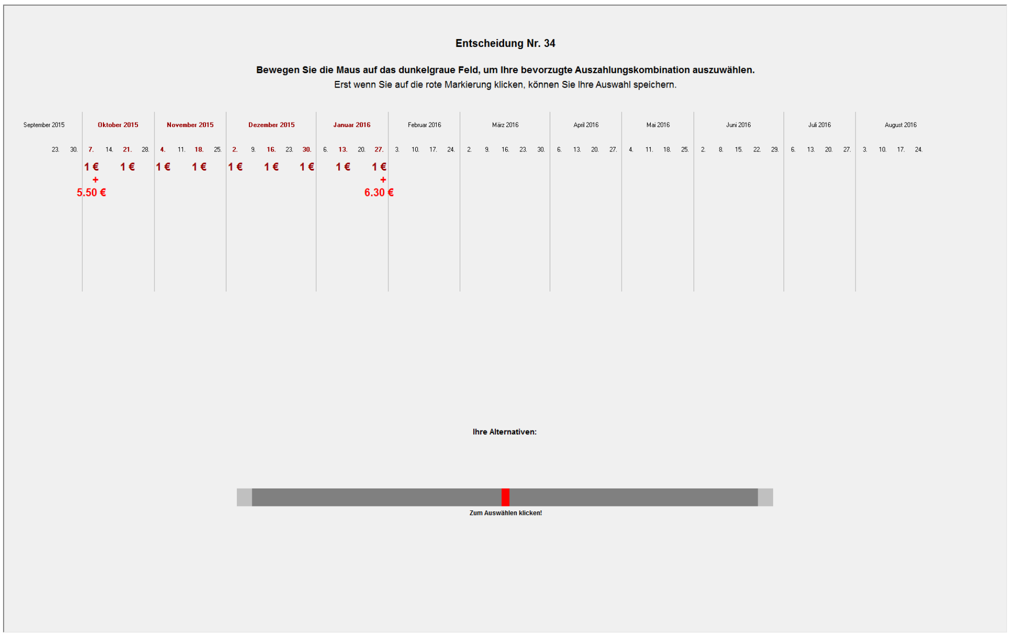
In a first step, we are interested in the comparison of chosen allocations between Conc-Conc and Conc-Disp. This comparison tests whether subjects behave differently in the case that the benefits of choosing a high savings rate x are dispersed over multiple future dates (Conc-Disp) rather than concentrated on a single future date (Conc-Conc). Concentration bias predicts that individuals underweight dispersed consequences relative to concentrated consequences. In Conc-Disp, the benefits of behaving *patiently* attract little attention, because they are dispersed in the form of comparatively small payments over several dates. By contrast, in Conc-Conc, the benefit of behaving *patiently* is concentrated in a single, comparatively large—i.e., attention-grabbing—payment. Thus, individuals are predicted to pay *less* attention to the later payoff in Conc-Disp than in Conc-Conc, which promotes a *lower* savings rate in the former condition.

Figure 1.F.3 shows an exemplary decision screen with $B = 11$ and $r \approx 15\%$ for both Conc-Conc (upper panel) and Conc-Disp with $n = 8$ (lower panel). Through a slider, subjects choose their preferred $x \in \boldsymbol{X}$.[20] The slider position in Figure 1.F.3 indicates $x = 0.5$, i.e., the earliest payment is reduced by €5.50. Since $r \approx 15\%$ in this example, this slider position amounts to €6.30 that are paid at later payment dates. While these €6.30 are paid in a single bank transfer on the latest payment date in Conc-Conc, the amount is dispersed in equal parts over the last 8 payment dates in Conc-Disp—i.e., 8 consecutive payments of €0.79.[21] Concentration bias predicts that the dispersed payoff of 8×0.79 will be underweighted relative to the concentrated payoff of €6.30.

In a second step, we are also interested in the comparison of allocation decisions between Conc-Conc and Disp-Conc. Since the benefits of being *impatient*, i.e., of choosing a small x, are dispersed in Disp-Conc, individuals tend to neglect them according to concentration bias. By contrast, the benefit of behaving *impatiently* is concentrated in a single—i.e., attention-grabbing—payment in Conc-Conc. Therefore, concentration bias predicts that individuals pay *less* attention to the earlier

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

21. We always round the second decimal place up so that the sum of the payments included in a dispersed payoff is always at least as great as the respective concentrated payoff.



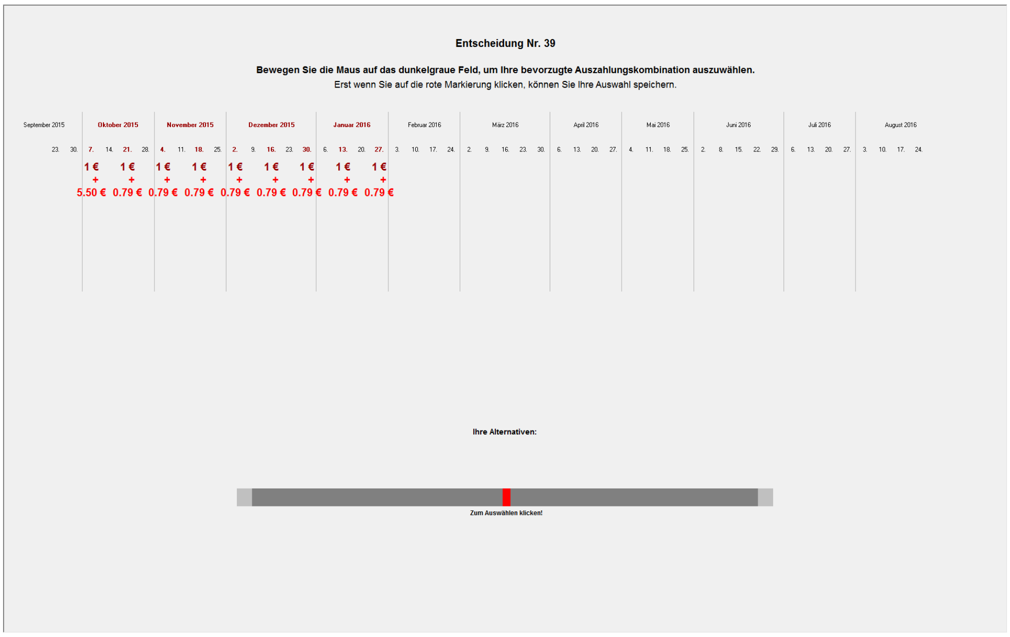


Figure 1.F.3. Screenshots of a CONC-CONC Decision (Top) and an CONC-DISP Decision (Bottom). Here, $x = 0.5$ of the budget B is shifted to the later payoff which is concentrated at the last date in the top and dispersed over $n = 8$ dates at the bottom.

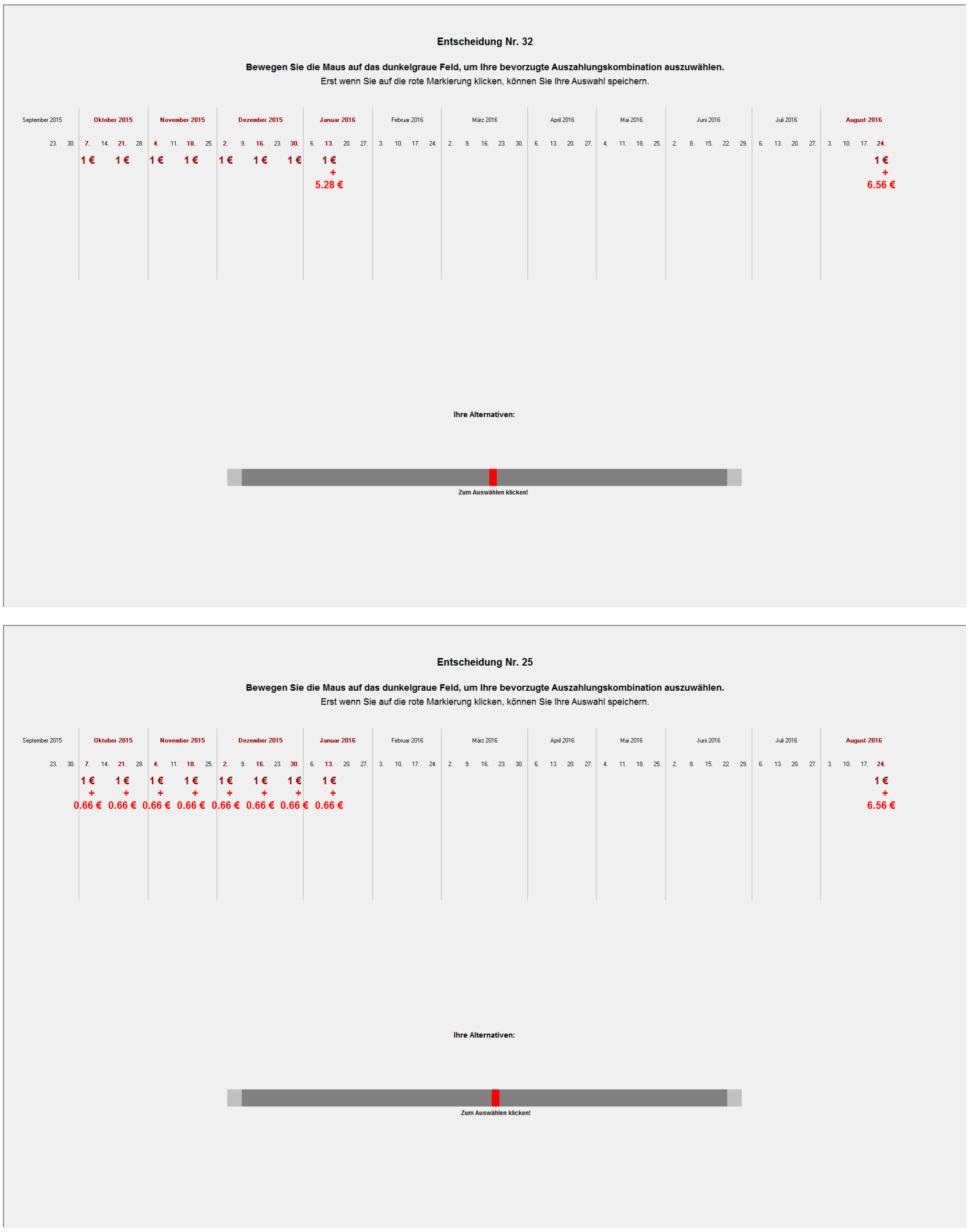


Figure 1.F.4. Screenshots of a CONC-CONC Decision (Top) and an DISP-CONC Decision (Bottom) Here, $x = 0.48$ of the budget B is shifted from the earlier payoff which is concentrated at the second-to-last date in the top and dispersed over $n = 8$ dates at the bottom.

payoff in Disp-Conc than in Conc-Conc, which promotes a *higher* savings rate in the former condition.

Figure 1.F.4 shows the decision screen of an exemplary decision with $B = 11$ and $r \approx 15\%$ for both Conc-Conc (upper panel) and Disp-Conc (lower panel). The slider position in Figure 1.F.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 bank transfer on the second-to-last payment date in Conc-Conc, the amount is dispersed in equal parts over the first 8 payment dates—i.e., 8 consecutive payments of €0.66—in Disp-Conc. Concentration bias predicts that the dispersed payoff of 8×0.66 will be underweighted relative to the concentrated payoff of €5.28.

1.F.1.2 Decision Times, Cognitive Reflection Test, and Calculation Task

Similar to Section 1.4.3, we test for heterogeneity of the concentration-bias effect. To do so, we collected the time that individuals took to make their decision, the Cognitive Reflection Test (CRT; Frederick, 2005) and a math task where subjects were asked to calculate as much as possible sums in five minutes. Each sum consisted of 4 to 9 decimal numbers and was rewarded with €0.20 if correctly solved. If a subject did not provide the correct sum within three attempts, €0.05 were deducted from their earnings. To avoid negative earnings, subjects received an initial endowment of €1.

1.F.1.3 Procedure

The experiment was conducted in two waves at the BonnEconLab.

In the first wave, each subject made 36 choices across different budget sets. One set of subjects ($N = 47$) faced 12 budget sets ($B = 8$, $r \in \{20\%, 50\%, 80\%\}$; $B = 11$, $r \in \{15\%, 36\%, 58\%\}$; $w \in \{3, 6\}$) of the type Conc-Disp for $n = 4$ or 8, and the corresponding Conc-Conc decisions$[(2 \times 3 \times 2) \times (1 + 2) = 36]$. A second set of subjects ($N = 46$) faced the same parameters for Disp-Conc, again for $n = 4$ or 8, and the corresponding Conc-Conc budget sets. That is, in the first wave, Conc-Disp and Disp-Conc decisions varied between subjects. (Note, however, that the relevant balanced versus unbalanced comparisons are all within-subject.)

In the second wave, we combined Conc-Disp and Disp-Conc decisions within-subject.[22] All subjects ($N = 92$) made 32 choices as follows: they were given four

22. In the first wave, participation in any of the two comparisons was randomized between-subjects. This is because we initially considered including an interest rate as high as $r = 80\%$ reasonable, given that in previous studies, participants had exhibited extremely strong discounting (see, e.g., Figure 2 from Dohmen, Falk, Huffman, and Sunde, 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 comparisons. This is unproblematic because all balanced–unbalanced comparisons are nevertheless within-subject comparisons. Moreover, with virtu-

budget sets ($B = 11$; $r \in \{15\%, 58\%\}$; $w \in \{2, 3\}$) of each of the two Conc-Conc and the six associated Conc-Disp and Disp-Conc types ($n \in \{2, 4, 8\}$ for each) $[(2 \times 2) \times (2 + 2 \times 3) = 32]$.

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

Subjects in both waves were also asked to choose between additional earnings sequences presented in the form of 24 (first wave) and 28 (second wave) choice lists which test for concentration bias in a slightly different manner. In this paper, we do not analyze these choice lists but refer to an earlier version working paper.

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. 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 via hroot).

1.F.2 Predictions

Similar to Section 1.3.1, we derive predictions for discounted utility as well as the focusing model.

In the following, we assume that individuals base their decisions on utility derived from receiving monetary payments c_t at various dates t. Additionally, we make the standard assumption that utility from money is increasing in its argument but not convex: $u'(c_t) \geq 0$ and $u''(c_t) \leq 0$. This assumption about the curvature of the utility function is also in line with previous findings in the literature. Andreoni and Sprenger (2012) and Augenblick, Niederle, and Sprenger (2015), for instance, estimate that utility in money is concave (albeit close to linear). When deriving our predictions, we refer to Section 1.2 for a general description of the utility function and its difference for discounted utility and focusing.

ally the same number of subjects in the two comparisons during the first wave (47 vs. 46), calculating averages across both comparisons does not suffer from unequal group sizes. Please note that the findings regarding balanced–unbalanced differences that we present below are rather conservative due to the ceiling effects.

1.F.2.1 Discounted Utility

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. We use the superscript $^{\text{DU}}$ to indicate decisions based on discounted utility.

Conc-Conc vs. Conc-Disp. We consider Conc-Conc and Conc-Disp first. In Conc-Conc, individuals decide how much to allocate to the different payment dates by choosing

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

Recall that the later payoff, which is concentrated on the latest payment date in Conc-Conc, is dispersed over the last n payment dates (i.e., partially paid earlier) in Conc-Disp (see Figure 1.F.1). In Conc-Disp, individuals therefore choose

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

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

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

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

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

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

Collectively, we have

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

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

Conc-Conc vs. Disp-Conc. We consider Conc-Conc and Disp-Conc next. In analogy to above, we denote the individual's optimal choices by $x^{\text{DU}}_{\text{C-C}}$ and $x^{\text{DU}}_{\text{D-C}}$, respectively. Recall that the second-to-last payoff of Conc-Conc is dispersed over n earlier dates in Disp-Conc (see Figure 1.F.2). Here, the marginal negative consequences of increasing x are greater in absolute terms in Disp-Conc than in Conc-Conc. This effect is, again, driven both by the (weak) concavity of the utility function u and the fact that in Disp-Conc, parts of the negative consequences occur earlier and are thus discounted less—i.e., exert a *greater* influence. This induces individuals to save at most as much in Disp-Conc as in Conc-Conc under discounted utility.

Discounted utility then predicts for the average over all n, B, and R that the difference

$$d^{\text{DU}}_{\text{D-C}} \;:=\; x^{\text{DU}}_{\text{D-C}} - x^{\text{DU}}_{\text{C-C}} \;\leq\; 0. \tag{1.F.2}$$

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

1.F.2.2 Concentration Bias

As is shown in Section 1.2, focusing extends the standard model of discounted utility by a weighting function g that causes a disproportionate focus on a single period.

Conc-Conc vs. Conc-Disp. We consider the implications of focus weighting on savings decisions in Conc-Conc and Conc-Disp first. For Conc-Conc, 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 Conc-Disp, date-1 utility also ranges from $u_1(1)$ to $u_1(1+B)$. However, date-9 utility ranges only from $u_9(1)$ to $u_9(1+RB/n)$. Thus, date-9 utility receives a lower weight in Conc-Disp than it receives in Conc-Conc, $g_{9,\text{C-C}} > g_{9,\text{C-D}}$. In fact, the larger the degree of dispersion, the smaller is the difference $\max u_9 - \min u_9$, and thus the lower is the weight, i.e., $g_{9,\text{C-D[2]}} > g_{9,\text{C-D[4]}} > g_{9,\text{C-D[8]}}$.

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

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

to be greater than

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

Expressed verbally, the benefits of being patient are underweighted in Conc-Disp relative to Conc-Conc. If this relative underweighting of dispersed benefits in Conc-Disp is sufficiently strong, focus-weighted marginal utility from being patient is greater in Conc-Conc than in Conc-Disp. In that case, the prediction of the standard model—inequality (1.F.1)—is reversed: focused thinkers save more in Conc-Conc than in Conc-Disp.

Let the superscript $^{\text{CB}}$ (for "concentration bias") indicate choices according to discounted utility in combination with focusing. With a sufficiently steep weighting function g,[23] we have

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

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

Conc-Conc vs. Disp-Conc. We now turn to the implications of focus-weighted utility on savings decisions in Conc-Conc and Disp-Conc. Recall that in Disp-Conc, the negative consequences of saving are dispersed over several payment dates, while they are concentrated at a single, thus attention-grabbing, payment date in Conc-Conc. Just as before, if the slope of g is sufficiently steep, then focus weighting reverses the prediction of the standard discounted utility—stated in formula (1.F.2)—by predicting that individuals save more in Disp-Conc than in Conc-Conc. Again considering averages over all n, B, and R used in our experiment, we have:

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

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

1.F.2.3 Hypotheses

We hypothesize that concentration bias is sufficiently strong so that it induces individuals to save more in Conc-Conc than in Conc-Disp, $d^{\text{CB}}_{\text{C-D}} > 0$, as well as to save more in Disp-Conc than in Conc-Conc, $d^{\text{CB}}_{\text{D-C}} > 0$. Both effects combined yield the prediction regarding the aggregate concentration bias of $d^{\text{CB}} > 0$, with d^{CB} being the average of $d^{\text{CB}}_{\text{C-D}}$ and $d^{\text{CB}}_{\text{D-C}}$.

Hypothesis 3. *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, $d^{\text{CB}} > 0$ (in contrast to standard discounting).*

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

Hypothesis 4. *The effect described in Hypothesis 3 is the more pronounced, the more dispersed a payoff is, i.e., $d^{\text{CB}}_8 > d^{\text{CB}}_4 > d^{\text{CB}}_2 > 0$.*

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

Table 1.F.1. Testing concentration bias, $\hat{d}$, against zero

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

Notes: This table presents an estimate of the difference in savings rates between balanced (Conc-Conc) and unbalanced (Conc-Disp and Disp-Conc) trade-offs. Standard errors are in parentheses, clustered on the subject level. The number of observations does not equal twice the number of subjects, because the subjects in the first wave participated only in one comparison, while the subjects in the second wave participated in both (see Section 1.F.1.3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

1.F.3 Results

Subjects made multiple allocation decisions in this experiment. In particular, subjects made several allocation decisions for Conc-Conc, Conc-Disp and Disp-Conc budget sets. This allows us to calculate for each individual the average difference in the savings rate between the Conc-Conc and the associated Conc-Disp and Disp-Conc budget sets. Denote by $\hat{x}$ and $\hat{d}$ the empirical counterparts of the variables introduced in Section 1.F.2, i.e., of the savings rate x^{CB} and the between-condition difference d^{CB}. With this, we can test our hypotheses.

1.F.3.1 Test of Hypothesis 3

Result 9. *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 3. Subjects allocated $\hat{d} = 6.3$ percentage points (p.p.) more money to payoffs that are concentrated rather than dispersed. This treatment effect is statistically significant, using a t-test, with standard errors corrected for potential clustering on the subject level (see Table 1.F.1).[24] This result provides evidence for concentration bias as predicted by Kőszegi and Szeidl (2013).

A closer look at the specific comparisons between Conc-Conc and Conc-Disp as well as Conc-Conc and Disp-Conc substantiates our first finding. Subjects allocated, on average, more money to later payment dates in Conc-Conc than in Conc-Disp, $\hat{d}_{\text{C-D}} = 5.7$ p.p. (= 9.12%). They also allocated, on average, more money to later payment dates in Disp-Conc than in Conc-Conc, $\hat{d}_{\text{D-C}} = 6.8$ p.p. (= 9.65%).[25] Both

24. This finding is corroborated by a signed-rank test ($p < 0.001$).

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

Table 1.F.2. Frequencies of the two measures of concentration bias, $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$, being positive, zero, or negative

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

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

$\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$ are significantly greater than zero in a t-test (both $p < 0.001$).[26] 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 1.F.2 provide further support. Table 1.F.2 shows the frequencies of individual values of $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$ being less than, greater than, or equal to zero. In both cases, the largest fraction of subjects has positive $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$ values, and there are more than twice as many subjects with positive than with negative $\hat{d}_{\text{C-D}}$ and $\hat{d}_{\text{D-C}}$ values, respectively.

At the same time, there are sizeable fractions of subjects whose $\hat{d}_{\text{C-D}}$ and/or $\hat{d}_{\text{D-C}}$ values are equal to zero. Let us investigate these subjects' behavior in greater detail. In the first group, the subjects with $\hat{d}_{\text{C-D}}$, four out of 47 subjects chose $\hat{x}_{\text{C-C}} = 0$. Hence, there was no "room" for them to save even less in the Conc-Disp condition. However, for the remaining 43 subjects who chose a savings rate of $\hat{x}_{\text{C-C}} = 1$, there was "room" to save less in the unbalanced budget sets, i.e., to choose $\hat{x}_{\text{C-D}} < \hat{x}_{\text{C-C}}$ in line with Hypothesis 3—but they did not do so. Thus, for these 43 subjects, concentration bias may not have mattered.[27] Regarding the second group, the 51 subjects with $\hat{d}_{\text{D-C}} = 0$, it turns out that 45 subjects chose $\hat{x}_{\text{C-C}} = 1$. This means that they already saved the entire budget in the Conc-Conc condition and their behavior may be confined by a ceiling effect: our task simply did not allow them to choose $\hat{x}_{\text{D-C}} > \hat{x}_{\text{C-C}}$, 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.

26. Both findings are corroborated by signed-rank tests ($p < 0.001$).

27. However, since under discounted utility, subjects with a positive discount rate are better off in Conc-Disp trials, we cannot rule out that concentration bias exactly offset the discounting-induced advantage of Conc-Disp for some subjects, moving them to $\hat{d}_{\text{C-D}} = 0$, rather than not affecting them at all.

1.F.3.2 Test of Hypothesis 4

Let us now turn to the question whether the degree of dispersion influences subjects' choices, i.e., to testing Hypothesis 4.

Result 10. *Our measure of concentration bias is the greater, the more dispersed payments in the* Conc-Disp *and* Disp-Conc *condition are, i.e.,* $\hat{d}_8 > \hat{d}_4 > \hat{d}_2 > 0$.

Our second result provides evidence in support of Hypothesis 4. We find that subjects' average degree of concentration bias depends on the degree to which the dispersed payoff is spread over time. Our measure of concentration bias is $\hat{d}_8 = 8.10$ p.p. for 8 payment dates, $\hat{d}_4 = 6.56$ p.p. for 4 payment dates, and $\hat{d}_2 = 3.67$ p.p. for 2 payment dates. All three treatment effects are significantly larger than zero according to both *t*-tests and signed-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 concentration bias for 8 payment dates is significantly larger than for 2 payment dates ($p < 0.01$) but not significantly greater than for 4 payment dates ($p = 0.237$).

1.F.3.3 Heterogeneity

Section 1.4.3 established a significant correlation of response time and the strength of the concentration-bias effect. In the money experiment, however, we do not find that the decision time correlates with concentration bias, see Column (1) of Table 1.F.3.

In addition, we test for potential influences of cognitive measures, that is we regress our measure of concentration bias, $\hat{d}$, on standardized measures of subjects' math ability and their CRT score. As evident from Table 1.F.3, we find that both these measures negatively affect concentration bias. That is, we find a stronger concentration bias for individuals who are more impulsive or who do worse in the math task. However, the correlation with impulsivity is not significant, and the correlation with math ability is only weakly significant.

1.F.4 Mechanism

By analyzing subjects' choices in Money-Main, we have provided evidence for concentration bias in intertemporal choice that is at odds with standard discounting, while it is predicted by the focusing model of Kőszegi and Szeidl (2013). As discussed in Section 1.5 it might be the case that focusing consists of two different channels, namely concentration in time and accessibility (Kahneman, 2003a,b). The intuition of the latter is that dispersed payments are cognitively more demanding to aggregate and therefore intangible and less accessible. Concentrated payments,

Table 1.F.3. Regression of the measure of concentration bias, $\hat{d}$, on decision time, a measure of mathematical ability, and CRT scores

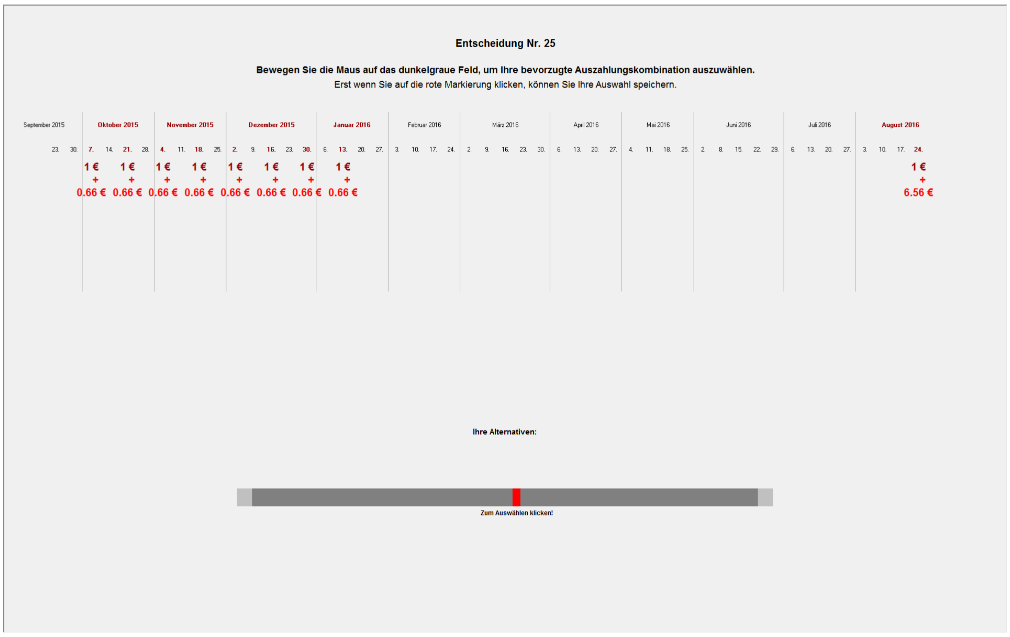
Dependent variable	(1) $\hat{d}$	(2) $\hat{d}$	(3) $\hat{d}$
$\widehat{\text{dtime}}$	0.000		
	(0.009)		
Standardized CRT score		−0.016	
		(0.011)	
Standardized Math score			−0.020*
			(0.011)
Constant	0.063***	0.063***	0.063***
	(0.011)	(0.011)	(0.011)
Observations	277	277	277
Subjects	185	185	185
R^2	0.000	0.009	0.014

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

on the contrary, are easily palpable and individuals can quickly process their underlying value. We aim to disentangle both channels of focusing with a new condition, Money-Mechanism, that is similar to Mechanism-Treatment in the consumption experiment.

In this new condition, all dispersed payoffs are “dispersed within a day” instead of being dispersed over different payment dates. More precisely, we combine the features of the unbalanced and balanced trade-offs: we make the “dispersed” payoffs equivalent to the concentrated ones, by scheduling all payments on the date of the concentrated payoff. In other words, the “dispersed within a day” payoffs are identical to the concentrated payoffs, except the difference in the display: subjects see 2, 4, or 8 relatively small monetary amounts that they have to sum to calculate the total earnings that they would receive at that date. Figure 1.F.5 displays a screenshot of the graphical representation that was shown to subjects who participated in these new conditions (lower panel) in relation to the graphical representation used in the main conditions (upper panel).

Subjects in Money-Mechanism make the same number of allocation decisions as subjects in Money-Main. Each “dispersed within a day” payoff replaces the respective “dispersed over time” payoff from Money-Main. The concentrated payoffs re-



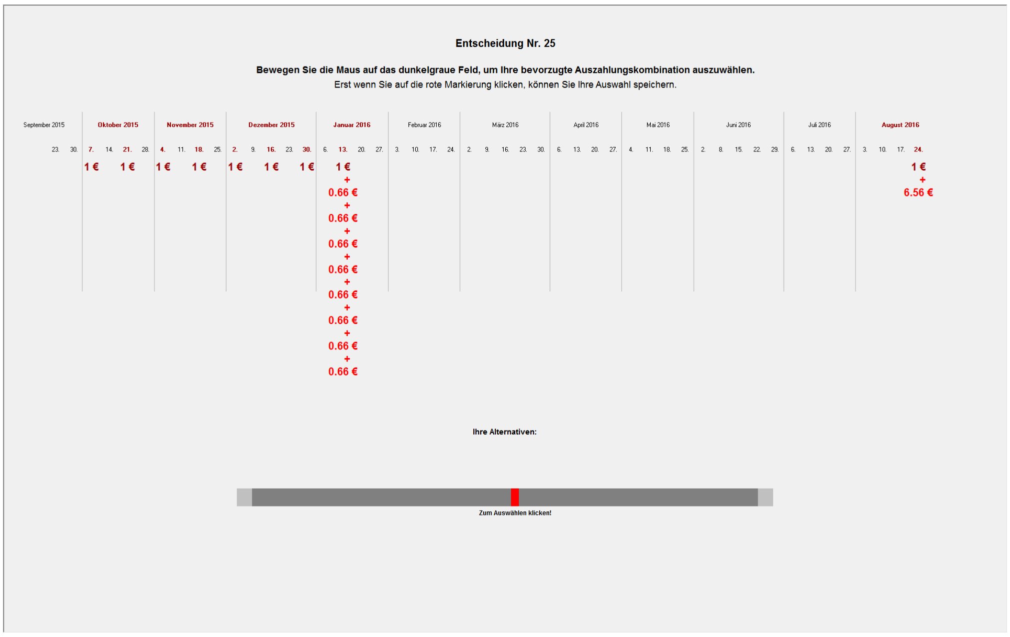


Figure 1.F.5. Screenshots of an DISP-CONC Decision in the MONEY-MAIN ("Dispersed over Time", Top) and of the Associated Decision in the MONEY-MECHANISM ("Dispersed within a Day", Bottom)

main exactly the same. Thus, we can calculate the same average difference of money allocated to concentrated payoffs between "balanced" and "unbalanced" budget sets, i.e., $\hat{d}$, for subjects as before. While $\hat{d}$ measures concentration bias in Money-Main, it measures effects resulting from accessibility in Money-Mechanism.

If our empirical measure $\hat{d}$ is statistically larger in Money-Main, it implies that the concentration bias observed in this condition cannot fully be explained by computational complexity and accessibility.

We compare $\hat{d}$ between our two conditions in an OLS regression. This comparison is between subjects and involves 374 subjects; of these, 185 participated in Money-Main and 189 participated in Money-Mechanism.[28] To compare Money-Main with Money-Mechanism, we regress $\hat{d}$ on a dummy variable that takes on the value 1 for all subjects who participated in Money-Main instead of Money-Mechanism.[29] The coefficient on the constant measures the behavioral effect of splitting up a payoff into the sum of multiple small amounts in Money-Mechanism, that is, when the payoff is "dispersed within a day." The coefficient on the Money-Main dummy measures how much larger (or smaller) the effect of splitting up the payoff is when it is dispersed over time.

As Columns (1) and (2) of Table 1.F.4 show, we find that merely presenting a payoff as the sum of multiple small payoffs, without any change in the timing of the payoffs, makes subjects choose this ("dispersed within a day") payoff less frequently than the associated concentrated payoff: the coefficient on the constant 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 Money-Mechanism. 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 accessibility.

However, we also find that our measure of concentration bias is greater in Money-Main: the coefficient on the dummy (0.036) is significantly greater than zero ($p < 0.01$). It is larger than the coefficient on the constant, suggesting that the effect in Money-Main is at least twice as strong as in Money-Mechanism. On average, subjects allocated 6.3 p.p. more of their budget to concentrated payoffs than to dispersed payoffs in Money-Main (see Table 1.F.1). Let us repeat that this is the case even though discounting works against the effect in Money-Main: discounting makes the dispersed-over-time payoffs more attractive than the dispersed-within-a-day payoffs in Money-Mechanism—which are consequentially identical to the concentrated payoffs. This provides evidence that concentration bias affects

28. Except for the first five sessions, both conditions were conducted during the same sessions, and subjects were randomly assigned within-session. During the first two sessions, only Money-Main was run; this was followed by three sessions in which only Money-Mechanism was run.

29. We have up to two values for the dependent variable per subject, depending on whether a subject participated in both comparisons or only one of the two. Consequently, we cluster standard errors on the subject level.

Table 1.F.4. Difference-in-differences analysis of concentration bias, $\hat{d}$, in Money-Main (dispersed over time) vis-à-vis Money-Mechanism (dispersed within a day)

Dependent variable	(1) $\hat{d}$	(2) $\hat{d}$
Money-Main 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 Money-Mechanism)	0.026***	0.025***
	(0.006)	(0.007)
Observations	562	562
Subjects	374	374
R^2	0.016	0.029

Notes: This table presents OLS regressions of the dependent variable, the difference between balanced and unbalanced trade-offs, on a dummy that equals 1 for Money-Main and equals 0 for Money-Mechanism. Column (2) additionally controls for decision time and standardised CRT and math scores. Standard errors are in parentheses, clustered on the subject level. The number of observations does not equal twice the number of subjects, because the subjects in the first wave participated only in one comparison, while the subjects in the second wave participated in both (see Section 1.F.1.3). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

intertemporal choice beyond what could be explained by accessibility and concentration in time is even more important, at least in this setup.

Appendix 1.G Instructions

1.G.1 Instructions for the consumption experiment

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

Screen 1—General Information

Decisions and Base Wages of all Participants

Today's experiment consists of tasks that will be explained in further details on the following pages. All participants in this study get a base wage of €10.00.

Random Selection of Three Participants

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

Random Choice of a Decision That Matters

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

Screen 2—Your Task: Translation of a Number Sequence to a Letter Sequence

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

[Example displayed]

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

[Example displayed]

The encryption key will be presented in the form of a table. It assigns a specific letter to each number. Your task is to find the corresponding letter to every number and enter it in the input field below. Please do not use spaces. The task is case sensitive. In the above example you see the number sequence “18 19 7 14 2 2”. With the code key shown in the example table you get the following letter sequence “LDXYVV”. You have to enter this sequence in the input field. The number sequence as well as the encryption key change from task to task. If you enter the letter sequence correctly, the input field turns green, and you can click on “Continue” to show the next number sequence. For practice, you will now complete 10 of such tasks.

Screen 3—Your current task

So far you have translated X number sequences. Thus, there are still Y number sequences remaining. Please enter the corresponding letter from the code table for each number (without spaces).

Screen 4—End of the Example Tasks

You have completed the example tasks successfully.

Screen 5—Selection of 8 Work Dates

Tasks per Work Date

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

Working Online

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

Choice of Work Dates

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

[list of possible dates]

Screen 6—Payment

For the full completion of all tasks on your 8 work dates there will be a pay-out consisting of two parts:

Part 1 of the Payment

Part 1 is a voucher for a restaurant visit in the Pizzeria 485°in Cologne (Kyffhäuserstraße 44, close to the university, or in Bonner Straße 34, Südstadt): On a day, chosen by yourself during the time from Mon., 19/08/2019, to Sun., 25/08/2019, you and a companion – can visit Pizzeria 485°and we will provide you with a voucher for a part of the costs of your restaurant visit. The value of the voucher will be determined during the course of today's experiment. Here is an overview of the costs of food and drinks at the pizzeria:
Appetizers: between 3.50€ and €14.50;
Pizzas (+ extra toppings): between €7.50 and €23.00;
Desserts: between €3.50 and €4.80;
Sodas: between €2.90 and €3.40;
Beer: between €3.20 and €4.70;
Regular wine per glass: between €2.90 and €8.00;
Regular wines per bottle: between €18.00 and €28.00.
There will be no additional effort for you to use thee voucher: We will communicate your name and the value of the voucher to Pizzeria 485°. You will get the voucher at the restaurant on the date that you chose for your restaurant visit. You will only have to pay a possible difference between the amount you consumed and the voucher. Here are two illustrating examples: Example 1: The voucher has a value of €20.50. But you do not exhaust its full amount: You consume food and drinks for €19.30. Then the voucher covers the full amount of €19.30. Example 2: The amount of the voucher has a value of €20.50. You and a companion consume food and drinks for €24,80. Then the voucher covers the amount of €20.50, and you have to pay the difference of €4.30 .

Part 2 of the Pay-Out

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

Choice of a Date for Your Pizzeria Visit

Please choose a date on which you want to visit Pizzeria 485°:

[list of possible dates]

Screen 7—Decisions, page 1

General Information Work Plans and Voucher

Today's experiment consists of different decisions. Each decision consists of a choice between Alternative A and Alternative B. Each alternative consists of a specific work plan and a voucher of a specific value. The work plan indicates how many tasks you have to complete on each of the 8 work days, respectively. The value of the voucher indicates how much you will get at most for your visit to Pizzeria 485°on CHOSEN DATE. In each decision, Alternative A yields a higher voucher value than Alternative B. In some decisions, Alternative A has a higher workload and in some decisions Alternative B has a higher workload.

Example 1

[Displayed example]

Alternative A consists of a work plan, according to which you have to complete 174 tasks on DATE X; on all other dates you have to complete the same number of tasks as for Alternative B. Completing all tasks yields a voucher with a value of €11.50. In this example, Alternative B consists of a work plan according to which you have to complete 157 tasks on 2DATE X; at at all other dates you have to complete the same number of tasks as in Alternative A. The completion of all tasks yields a voucher with a value of €7.50.

Randomly selected decision

At the end of the experiment, the computer randomly chooses one decision as the decision that matters. The alternative that you have chosen in this decision determines the value of the voucher and the number of tasks that you have to complete on the 8 work dates. If you complete the chosen work plan, you will get the voucher for the restaurant visit on the CHOSEN DATE and a week later €100 will be transferred to your bank account. On the next page you will learn how to make the decisions. (If you are not chosen at the end of the experiment you will not have to work on any of the 8 dates, and there will be no additional payment. In this case the experiment will be over.)

Screen 8—Decisions, page 2

Direct and Indirect Decisions

Direct Decisions

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

Indirect Decisions

In order to save time, you do not have to make all decisions directly. Instead, you can make some decisions indirectly. Your indirect decisions will always and exclusively be based on your own direct decisions. You will determine your indirect decisions according to the following principle: If you have chosen an alternative in a direct decision, this alternative will also be chosen in an indirect decision, if the chosen alternative itself has become better (rule 1) or the other alternative has become worse (rule 2). In the following, this principle will be illustrated using examples of the two rules.

Example for Rule 1

[Displayed example]

Assume you have chosen Alternative A in example 1. You no longer have to make example decision 2 explicitly since Alternative B is unchanged and alternative A has improved: On one date you have to work less. Thus in example 2 you would choose Alternative A indirectly.

Example for Rule 2

[Displayed example]

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

Information Screen

At the end of the experiment you will be shown all direct and indirect decisions.

Screen 9—Decisions, page 3

Random Selection of the Decision that Matters

Identical Probabilities

Direct and indirect decisions are selected with equal probabilities to be the decision that matters. Since the computer may pick any direct decision to be the decision that matters you should make every direct decision as if it would be implemented. Since the computer may pick any direct decision to be the decision that matters you should make every direct decision with great care. This is because, as explained on page 2, your indirect decisions will be determined completely and exclusively by your direct decisions.

Decision Blocks

The decisions are divided into 9 blocks. For each decision block, number and content of its decisions are determined at its beginning. As previously mentioned each decision of a decision block will be selected with the same probability by the computer to be the decision that matters. This holds for direct as well as indirect decisions. Thus, which alternatives you choose in the direct decisions of a decision block, has no influence on the content and number of the other decisions of the respective decision block. To repeat it again: The number and content of decisions is fixed at the beginning of the block. However, which decisions of a block you are going to make directly and which indirectly is random to some degree: The first direct decision of each block will be randomly selected by the computer among all decisions of the respective block. The order of the following direct decisions will be determined by the computer in such a way that you have to make as few direct decisions as possible–that is to save as much time as possible.

Screen 10—Decisions, page 4

Practice Decision Block

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

Screen 11—Practice Decision Block X

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

Screen 12—Practice Decision Screen

[No Instructions]

Screen 13—Control questions

Before making your decisions, please answer the following questions:

1. What amount of money do you receive for completing your work schedule?

The more money, the more tasks you complete: €0.10 per task.
Independent from your decisions you get €100.
The more money, the more tasks you complete: €0.25 per task.
Independent from your decisions you get €150.

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

Alternative A.
Alternative B.

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

7 work dates.

8 work dates.
9 work dates.

4. Which of the following statements is correct?

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

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

Screen 14—Decision Block X

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

Screen 15—Decision Screen

[No Instructions]

Screen 16—Please provide some additional information

This screen was optional and appeared whenever subjects always chose Alternative A in a decision block.

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

Screen 17—Survey

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

What is your gender?
How old are you?
What is your major?
What was your last grade in high school math class?

How high is your disposable income each month (incl. financial support by your parents, BAföG (student financial aid), unemployment insurance payments, deducting housing and health insurance expenditures)?

Screen 18—Overview of all decisions

Please click on “Continue” to get to an overview of all decisions.

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

Screen 19—Overview of all decisions

[Display of the overview]

Screen 20—Picture puzzles

In the following, you will have to solve some picture puzzles consisting of 10 separate pictures. In each picture one field is empty. Your task is to fill in this empty field, such that there is a logical progression of symbols. Please try to solve as many as possible of the 10 pictures.

A click on “Continue” starts the task. You have 5 minutes to complete all the puzzles.

Screen 21—Picture puzzle X of 10

[Display of the puzzle]

Screen 22—Picture puzzle

You have correctly solved X of 10 puzzles.

Screen 23—Addition of Numbers

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

Screen 24—Task Number X

[Display of the task]

Screen 25—Addition of Numbers

You have correctly solved X tasks and will therefore receive an additional payment of €Y.

Screen 26—Answering three questions

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

Screen 27—Question Number 1

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

Screen 28—Question Number 2

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

Screen 29—Question Number 3

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

Screen 30—Randomly selected decision

You have made decisions in 9 decision blocks. The computer randomly selected the block number X as the relevant block for your payment. From this decision block, decision number Y was randomly selected as the decision that matters. The decision was among the following two alternatives:

[Display of alternatives]

In this decision you prefer Alternative X over Alternative Y.

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

[Display of the work plan]

If you are, among the participants of the current session, randomly selected to be a person to work on all 8 work dates, the following applies:
At each work date you have to complete the tasks displayed. The completion of the work plan is compensated as follows: by a voucher with a value of €X for a restaurant visit to Pizzeria 485°on CHOSEN DATE as well as by a transfer to your bank account with an amount of €100 that is carried out one week after your restaurant visit.

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

For today's session you will receive a payment of €X (participation fee) + €Y (addition task) in cash.

Please remain seated and wait for further instructions.

1.G.2 Instructions for the money experiment

These are the instructions (translated from the German original) for both conditions of the money experiment "Convex Time Budgets". While the text of the instructions was the same for both conditions, the income sequences displayed on the respective screens were different.

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 in any case receive nine €1 payments, which will be transferred to your bank account at various dates in the future. Furthermore, you will receive one additional payment or multiple additional payments for this first part. You can decide yourself when this/these additional payment/s will be transferred. For any decision you make the following will always be hold: **If you choose a later payment, you receive, in total, more money than when choosing an earlier payment.**

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

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

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

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

Part 2

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

Screen 3

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

Screen 4—Choice Lists

Part 1a

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

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

Screen 5—Example 1

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

Screen 6—Example 2

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

Screen 7—Example 3

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

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

The transfer dates are highlighted in red.

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

This hint will be shown for the first three decisions.

Screen 8—Budget Sets

Part 1b

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

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

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

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

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

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

Screen 9—End of Part 1

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

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

Please click the "Continue" button.

Screen 10—Part 2

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

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

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

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

Screen 11

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

Screen 14—CRT 2

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

Screen 15—CRT 3

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

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Chapter 2

(Not) Everyone Can Be a Winner – The Role of Payoff Interdependence for Redistribution*

Joint with Sebastian Schaube

2.1 Introduction

Inequality and its political responses have frequently been described as one of the defining challenges of the 21st century (e.g., World Economic Forum, 2017). Facing rising levels of wealth and income inequality (Atkinson, Piketty, and Saez, 2011; Keeley, 2015), political actors and institutions have to determine commonly accepted levels of redistribution. Implementing respective policy measures that are widely accepted requires a sound understanding of which allocations people consider fair. Hence, finding the underlying determinants of preferences for redistribution is crucial for designing corresponding institutions and mechanisms, and advancing our general knowledge of social preferences.[1]

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1. Chance and individual behavior have already been identified as key determinants of preferences for redistribution. Inequality is accepted if it is backed up by performance (Abeler, Altmann, Kube, and Wibral, 2010) or individual decisions (Gantner, Güth, and Königstein, 2001; Cappelen, Hole, Sørensen, and Tungodden, 2007), but rejected if it is caused by luck (Croson and Konow, 2009; Cappelen, Konow, Sørensen, and Tungodden, 2013).

How the environment affects the realization of inequality might constitute such a determinant in itself. If it is a zero-sum game, success always comes at the expense of others and payoffs are negatively correlated. If not, success is generally attainable for everyone simultaneously and inequality might still arise but is no longer guaranteed. To shed light on this channel, this paper investigates how different degrees of payoff interdependence shape the demand for redistribution. In particular, we contrast environments where individuals' payoffs are negatively correlated with those where such a correlation is absent.

For this purpose, we conduct a series of laboratory experiments that allow isolating the causal effect of payoff interdependence on preferences for redistribution. In all conditions, two workers work on a real-effort task and can gain a prize. Afterwards, an otherwise uninvolved spectator can redistribute the prize earnings between them. The first set of treatments features randomness in the allocation process of prizes to workers. If payoffs are perfectly interdependent, workers compete for a single prize in a Tullock contest, whereby their relative performances determine their chances of receiving the prize. We remove this interdependence stepwise in two treatments. In the first step, we eliminate the interdependence in realized payoffs – one worker wins, the other loses – but keep the mutual impact of performances on others' expected payoffs identical. Prizes are now determined via two independent draws, one for each worker, whereby the two of them can both win a prize at the same time, only one might win, or even neither of them. As in the Tullock contest, relative performances determine a worker's chances of receiving a prize and hence expected payoffs are nonetheless negatively correlated. Therefore, in a second step, we implement a treatment where payoffs are entirely independent of each other: each worker still competes in a Tullock contest, but now individually against a randomly-drawn performance level, which is identical for both workers.

In a second series of experiments, we alter the allocation process to exclude the impact of randomness or luck on the workers' earnings. For the perfect interdependence of payoffs, this implies that the tournament becomes deterministic and the better-performing worker wins with certainty. Under complete independence, workers do not compete against each other but rather individually against a randomly-drawn threshold, which is the same for both workers.

In all treatments, the spectator knows the details of the procedure that generates the payoffs. After observing the outcome as well as the individual performances, she can redistribute payoffs between the two workers as she sees fit. These redistribution decisions are our main variable of interest as we interpret them as a proxy for the underlying distributional preferences. Throughout the analysis of this paper, we focus on situations with inequality in payoffs, namely where only one worker receives a prize while the other worker ends up empty-handed.

In general, we observe two main characteristics of redistribution decisions if only one worker ends up with a prize: First, across all treatments spectators redistribute sizable shares of this prize to the other worker. Second, spectators allocate more earnings to the workers who work more, meaning that they condition their decisions on performance differences. Most importantly, and this is a novel contribution of our experiment, we find that redistribution decisions are affected by the interdependence of *realized* payoffs: (1) The redistributed share is even larger if – right from the start – only one worker can potentially win a prize. Put differently, spectators redistribute less if realized payoffs are not directly linked and two prizes are a priori attainable. (2) The interdependence of realized payoffs also affects how spectators respond to performance differences. If both workers can win simultaneously, performance differences matter less for redistribution decisions. (3) For both effects, it is not relevant whether chances of winning are still influenced by the other worker's performance or completely independent from one another. Thus, redistribution decisions are only affected by the interdependence of realized payoffs, namely if in principle everyone can win simultaneously or not. Whether workers additionally influence each others' chances of winning and thereby their expected payoffs has no further impact on redistribution.

The above patterns prevail in settings in which a mix of luck and performance allocates prizes (first set of treatments), as well as in those that are deterministic (second set of treatments). However, the overall level of redistribution differs between these setting: when luck is part of the allocation process, spectators redistribute a higher share of the prize. This is independent of the interdependence of payoffs.

This observed impact of the interdependence of payoffs cannot be explained by theories on social preferences (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002) that incorporate fairness notions into the utility function, as these consider only inputs and outcomes which we keep constant across our treatments.[2]

Our findings contribute to the literature on fairness and redistribution in two distinct ways: First, to the best of our knowledge, we are the first to establish that inequality tends to be tolerated more willingly if it does not come directly at the expense of others. Second, we show that the mere presence of randomness or luck in the allocation process leads to higher demand for redistribution. Our findings therefore highlight an important channel – the outcome-generating mechanism itself – that shapes redistributive preferences.

While this paper focuses on the interdependence of payoffs and hence the degree of how people's decisions and fortunes affect others, the overwhelming majority of the literature on redistribution concentrates on the individual accountability for own payoffs (e.g., Konow, 1996; Konow, 2000). Major differences in demand for redistribution emerge if inequalities arise due to luck compared with differences in individual decisions (Cappelen, Konow, et al., 2013), investment (Cappelen, Hole,

2. Note that the workers' starting positions and chances of winning are symmetrical ex ante. Hence, they have equal opportunities to gain a prize in all treatments. Consequently, any theories considering equality of opportunity would not predict any treatment differences in our setup.

et al., 2007), and effort (Fischbacher, Kairies-Schwarz, and Stefani, 2017). Inequality is accepted when it originates from differences in performance and investment (e.g., Almås, Cappelen, and Tungodden, forthcoming; Frohlich, Oppenheimer, and Kurki, 2004) and equality is even seen as unfair (Abeler et al., 2010). By contrast, third parties tend to eliminate inequalities between others once luck is involved in the payoff mechanism (e.g., Mollerstrom, Reme, and Sørensen, 2015; Breza, Kaur, and Shamdasani, 2017; Gee, Migueis, and Parsa, 2017; Rey-Biel, Sheremeta, and Uler, 2018).[3]

In the studies investigating the role of luck in tournaments, it is unclear to the spectator whether the winner is the high-performing worker and whether her win was due to merit or luck. We eliminate any doubts about possible discrepancies by providing additional information about individual performances and hence only vary the presence of luck. Therefore we are able to causally demonstrate that a high demand for redistribution under luck is not solely a result of uncertainty about whether a high payoff was indeed merited or not. Unequal outcomes tend to be viewed as more unfair as soon as any luck is involved in their creation.

At the same time, the source of inequality becomes less important as long as people can be made (partially) responsible for their earnings, resulting in a higher acceptance of unequal incomes. Cappelen, Fest, Sørensen, and Tungodden (2016) argue that even arbitrary decisions induce a sense of responsibility for resulting inequalities. In relation to our findings, their results indicate that spectators might hold high earners responsible for the low payoffs of others if payoffs are interdependent but not if the low earner could have won simultaneously. The importance of responsibility is further stressed by Bartling, Cappelen, Ekström, Sørensen, and Tungodden (2018). Once people are given the opportunity to select into winner-take-all tournaments without randomness, the tournament's inequality is widely accepted. Our treatments without randomness support this finding. In addition, we document that the acceptance of inequality increases even further if the two parties were not competing against each other, but rather against an exogenous threshold.

While the quantitative measurements of the preferences for redistribution might be particular to the experimental parameters, we find that our qualitative results are in line with previous survey evidence. Respondents in the World Values Survey (Inglehart, Haerpfer, Moreno, Welzel, Kizilova, et al., 2014) who believe that wealth can only be accumulated at the costs of others are also more strongly in favor of

3. These papers treat the impact of performance and luck separately. We depart from this dichotomous view and allow both luck and performance to influence earnings at the same time. In a recent paper, Cappelen, Moene, Skjelbred, and Tungodden (2017) also investigate the interplay of merit and luck as determinants for redistribution. In contrast to our paper, a discernible part of individual payoffs is determined by luck and another part by performance. Spectators are unaware of the individual performances. The authors find that even if only a small part of the total inequality is based on merit, spectators redistribute similar amounts than in a case where all inequality is based on merit.

a redistribution of incomes.[4] We take this as an indication that interdependence matters for preferences for redistribution not solely under the specific knowledge of the mechanism that generates individual earnings. A perception of the general environment as either strongly interrelated or not might already influence whether inequality is accepted or not, resulting in potentially different political institutions. For instance, using the World Values Survey, we find that people in the US have a significantly stronger belief in the independence of payoffs than those in all other developed countries.[5] Alesina and Angeletos (2005) document a correlation of social spending and redistribution with beliefs about the importance of luck and effort for wealth and income across countries. Our results indicate a complementary channel through which the perception of the societal environment and context might shape institutions. Finally, as discussed by Frank and Cook (1996) and Frank (2016), technological change and the increasing prominence of bonus schemes make winner-take-all and zero-sum situations more common in everyday life. This development might contribute to a raised sense of unfairness, beyond the actual level of earnings inequality.

In this respect, our findings should not only matter in the abstract sense of beliefs about the interdependence of earnings and optimal levels of redistribution within society, but they might even be relevant for the optimal setting of wages within firms. Forced rankings by supervisors, promotion tournaments, and fixed bonus pools always imply that one employee succeeds at the cost of others. These strategies are frequently employed as they allow principals to set incentives if effort is not verifiable (Rajan and Reichelstein, 2006). On the flip side, such incentive schemes could constitute an important source of discontent and envy within the firm. In turn, this might explain the ambiguous effect of forced ranking schemes on individual performance observed by Berger, Harbring, and Sliwka (2013). In the long run, interdependent payoffs might harm employees' willingness to exert effort in the first place. By contrast, employees might be willing to accept unequal pay within a division or firm more eagerly if advanced positions are not exogenously limited and bonus pools are not fixed. In turn, this would allow firms to set steeper incentives or even reduce overall payment.

The remainder of this paper is structured as follows. Section 3.3 presents the design of the laboratory experiment, before Section 3.5 shows the results. Finally, Section 3.6 discusses and concludes.

4. The exact wording of the questions is "incomes should be made more equal" vs. "we need larger income differences as incentives for individual effort", and "people can only get rich at the expense of others" vs. "wealth can grow so there's enough for everyone". We find a correlation of $\rho = 0.13$, $p < 0.0001$ between the answers to the two questions.

5. Figure 2.A.1 in the Appendix presents the mean beliefs in the interdependence of payoffs in the western countries.

2.2 Design

In natural environments, the multitude of factors determining income and wealth as well as the ignorance about the specific relationship between performance, luck and outcomes makes it very difficult to identify the causal impact of the specific context on demand for redistribution. We therefore use the controlled environment of a lab experiment to investigate how preferences for redistribution are affected by the interdependence of payoffs.

Our design is based on a two-stage experiment – previously used, for example, by Cappelen, Konow, et al. (2013) – which features two types of subjects: workers and spectators. In the first stage of the experiment, workers have the opportunity to gain a prize. They are grouped in pairs and can work on a real-effort task to increase their likelihood of winning a prize. Subsequently, the winner(s) of the prize are determined, whereby the payoff-generating mechanism is varied between treatments. In the second stage, each spectator observes performances and earning distributions of one pair and is able to redistribute earnings between these two workers. Roles and treatments are assigned at random.

2.2.1 Workers

At the beginning of the experiment, workers are informed that they are matched with another worker and that they have the opportunity to gain a prize of €6. However, they are also told that another participant can redistribute any earnings between the two workers ex post. Workers perform a real-effort task (repositioning sliders, based on Gill and Prowse, 2012). On every screen, subjects have to adjust five sliders ranging from 0 to 100 to the mid-position (50). Each screen with five sliders counts as a single task. Workers can spend up to twelve minutes completing as many tasks as they like. This part of the experiment is conducted online.

The number of completed tasks is used to determine whether a worker is awarded a prize or not. Across treatments, we vary how earnings are realized. In principle, a higher performance increases the likelihood of earning a prize. While workers are informed about the outcome-generating process, they do not receive any information about the actual distribution of prizes at this point. They are only informed that a third subject – the spectator – observes performances and earnings and can redistribute any amount between the two workers. Only after spectators' decisions are implemented do the workers receive information about their performances and final payoffs.

2.2.2 Spectators

In the second stage, a third subject – acting as a spectator – can redistribute money within pairs of workers. For this purpose, spectators are introduced to the real-effort task and have to test it for themselves for one minute. Prior to making their deci-

Table 2.2.1. Hypothetical pairs and their occurrence in treatments

situation	winner	perf. A	perf. B	random number	R-FC	R-CC	R-NC	D-FC	D-NC
1	A	54	54	51	X	X	X	X	-
2	A	35	32	33	X	X	X	X	X
3	A	56	59	58	X	X	X	-	-
4	A	63	31	49	X	X	X	X	X
5	A	39	54	45	X	X	X	-	-
6	A	44	46	37	X	X	X	-	-
7	A	45	55	26	X	X	X	-	-
8	A	30	51	57	X	X	X	-	-
9	A	68	63	65	X	X	-	X	X
10	A	24	28	-	X	X	-	-	-
11	A	72	58	69	X	X	-	X	X
12	A	37	37	-	X	X	-	X	-
13	A&B	61	61	63	-	X	X	-	-
14	A&B	38	35	36/28	-	X	X	-	X
15	A&B	69	29	45/16	-	X	X	-	X
16	A&B	47	64	51/27	-	X	X	-	X
17	A&B	33	33	30	-	X	-	-	X
18	A&B	58	54	-	-	X	-	-	-
19	A	67	67	45	-	-	X	-	-
20	A	66	73	55	-	-	X	-	-
21	A	42	37	50	-	-	X	-	-
22	A&B	49	49	32	-	-	X	-	X
23	A&B	41	37	53	-	-	X	-	-
24	A	46	44	-	-	-	-	X	-
25	A	55	45	48	-	-	-	X	X
26	A	51	30	34	-	-	-	X	X
27	A&B	46	44	37	-	-	-	-	X

Perf. states the performance of the respective worker, “X” denotes that the situation is featured in the treatment whereas “-” indicates that it is not. Random number indicates the randomly drawn threshold for the *No Competition* treatments. For decisions 14 to 16, the first number corresponds to treatment *Randomness - No Competition*, and the second number to treatment *Deterministic - No Competition*.

sion, the spectators observe performances and current earnings of each worker.[6] Subsequently, they are asked to redistribute any amount of the prize(s) between the two workers in steps of €1. These options are presented as income distributions for workers A and B, respectively, and workers are paid according to the chosen distribution.

6. Whenever only one prize is allocated, we label worker A as the winner of that prize.

Table 2.2.2. Treatment overview

Treatment	# potential prizes	Interdependence of payoffs	Featuring randomness
Randomness - Full Competition (R-FC)	1	Yes expected & realized	Yes
Randomness - Chance Competition (R-CC)	2	Yes expected	Yes
Randomness - No Competition (R-NC)	2	No	Yes
Deterministic - Full Competition (D-FC)	1	Yes expected & realized	No
Deterministic - No Competition (D-NC)	2	No	No

Each spectator can redistribute earnings between multiple pairs of workers. We use a variant of the strategy method (as used, e.g., in Kube and Traxler, 2011) to collect choices of redistribution across multiple conditions. We present subjects with a portfolio of combinations of performances and winner(s), and inform them that only one combination represents a real pair of participants, whereas the remaining pairs are hypothetical.[7] The selected performances and the treatments in which they are used are displayed in Table 2.2.1. Subjects only learn the true pair after their decisions and are unable to identify this pair.[8] This method enables us to hold performances and earning distributions constant and exogenous across participants and treatments.

2.2.3 Treatments

As already outlined above, workers exercise a real-effort task and performance in this task influences expected and realized payoffs. The exact mapping of performance to payoffs is varied across treatments in two dimensions. The first variation involves the degree of payoff interdependence. Moreover, we vary whether randomness is involved, that is whether the worker with the lower performance has a positive chance of winning a prize. We provide an overview of all treatments in Table 2.2.2.

Our first three treatments involve randomness in the payoff allocation and vary in their degree of payoff interdependence. All of these treatments are framed as a lottery. The completion of one task produces one lottery ticket which is thrown into

7. The order of the pairs is randomized between subjects.

8. We ask subjects to guess the true pair in an incentivized ex post question and only 12 out of 200 (6%) subjects state a correct guess.

an urn. The number of urns and the composition of tickets represents the treatment variation.

In the baseline treatment *Randomness - Full Competition (R-FC)*, both workers compete for a single prize in a Tullock contest (e.g., Tullock, 2001). Accordingly, both workers put all of their tickets into a single urn, whereby one ticket is randomly drawn to determine the winner of the contest. In this setting, subjects compete in ex ante payoffs, namely in expected payoffs and chances. In addition, they compete in ex post payoffs, since exactly one prize is handed out and consequently the earnings of one worker automatically determine those of the second one.

We remove these interdependencies in two steps. First, we introduce independence of realized payoffs in treatment *Randomness - Chance Competition (R-CC)*. Before one ticket is drawn from the urn containing the tickets of both workers, the urn is duplicated. Subsequently, for each worker an independent draw is executed. If the corresponding draw of worker A produces a ticket of worker A, she gains a prize. The same is true for the draw from worker B's urn: if a ticket of worker B is drawn, B receives a prize. Consequently, it is now possible that in addition to one winner, both can win a prize simultaneously, or neither of them. Importantly, compared with *R-FC*, neither the expected payoffs nor the impact of worker A on worker B's chances (and vice versa) change. Relative performance still determines a worker's likelihood of winning a prize.

Treatment *Randomness - No Competition (R-NC)* additionally removes this interdependence of expected payoffs. Each worker has her own urn, containing only her own earned tickets. Moreover, an identical number of blanks is added to each urn. The number of blanks is randomly drawn from a predetermined set.[9] This implies that the performance of one worker no longer has any influence on the chances of success for the other worker, whereby both workers' income is determined completely independent from one another.

In addition, we conduct two further treatments that correspond to *R-FC* and *R-NC* but eliminate any randomness in the allocation of the prize. In treatment *Deterministic - Full Competition (D-FC)*, two workers compete for a single prize, which is allocated to the better-performing worker with certainty.[10] In the second condition, *Deterministic - No Competition (D-NC)*, each worker receives a prize if she exceeds a randomly-drawn threshold, so either both, one or no worker receives a prize. As before, the threshold corresponds to the performance of a third uninvolved worker and is identical for both workers.

9. In turn, this is based on previously observed performances by unrelated workers.
10. If both workers have the same performance, the winner is randomly determined.

2.2.4 Procedures

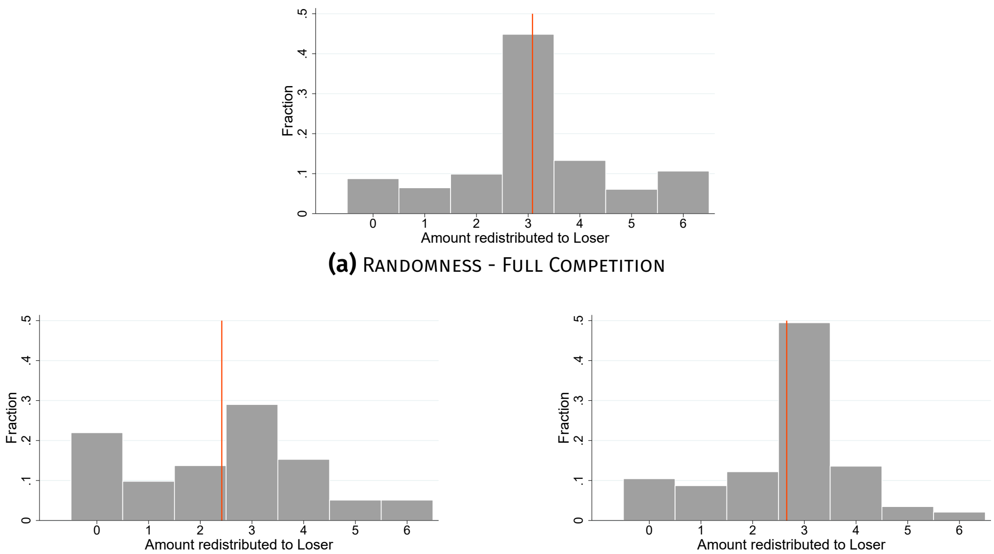
The experiment was conducted with subjects from the subject pool of the BonnEconLab between April and June 2018. Students were recruited using hroot (Bock, Baetge, and Nicklisch, 2014) and both stages were computerized via oTree (Chen, Schonger, and Wickens, 2016). The first stage of the experiment was conducted online. In addition, the subjects filled in a short questionnaire. This part of the study lasted about 20 minutes and subjects earned on average €4.25, including a show-up fee of €1. Spectators were invited to the BonnEconLab to make their redistribution decisions and received a flat fee of €8 for their participation. They could earn an additional amount of €1 by correctly guessing the non-hypothetical pair of workers once they had made all redistribution decisions. After finishing the redistribution decisions, subjects answered a questionnaire containing locus of control (Rotter, 1966), questions regarding social inequality (Scholz, Heller, and Jutz, 2011) and sociodemographics. The second stage took about 40 minutes and subjects earned on average €8.05.

Participants were assigned to treatments at random. Hence, the sizes of treatments slightly vary, whereby each treatment features between 39 and 43 spectators. For each spectator making a decision, a pair of workers was required to generate the underlying performance and earning distribution. Including all treatments, 200 spectators and 400 workers participated overall.

2.3 Results

Our experimental design allows to study the causal effect of the interdependence of payoffs on preferences for redistribution. The three main treatments, analyzed in Section 2.3.1, feature three contexts that vary the interdependence of two workers' earnings. More specifically, they correspond to a winner-take-all contest (*R-FC*), a contest that determines individual expected payoffs (*R-CC*) and two individual, independent contests against a randomly-determined performance level (*R-NC*). After presenting the results from these treatments, we support our evidence in Section 2.3.2 with a second set of treatments that remove any randomness in the prize allocation process, but still vary between full competition (*D-FC*) and no competition (*D-NC*). Finally, concluding our analysis in Section 2.3.3, we use the treatments with full and without any competition to identify the impact of randomness per se.

Throughout the entire analysis we always focus first on the redistribution behavior in those situations that are featured in all analyzed treatments (see Table 2.2.1). Accordingly, subjects observe exactly the same combinations of work performances and resulting earnings allocations. In a second step, we include those situations that are not featured in all treatments (including the actual worker pairs). Due to the differing performance levels, they are not comparable one-to-one; rather, we control for the different performance levels and study their impact for each treat-



(a) Randomness - Full Competition

(b) Randomness - Chance Competition

(c) Randomness - No Competition

The figure presents the histograms of the money transferred to the loser for the three main treatments. The vertical red lines indicate the mean level of money transferred. The figures include only transfers for those situations that were featured in all three treatments.

Figure 2.3.1. Amount redistributed to the loser for treatments with randomness

ment separately. As we are interested in responses to inequality, we focus on those situations where only one worker wins a prize and spectators face unequal earnings.[11] Throughout this section, we look at how much of the €6 prize the spectators redistribute to the loser.

2.3.1 The Effect of Payoff Interdependence on Redistribution

Figure 2.3.1 illustrates our main result, displaying histograms of redistribution decisions for those situations 1 to 8 that are featured in all three *Randomness* treatments. Under full competition (*R-FC*), spectators redistribute €3.08 to the loser on average. In slightly less than half of all situations (44.5%), spectators choose to equalize earnings between the two workers. Once the realized payoffs are no longer directly interrelated, spectators redistribute less. The average amount redistributed to the loser drops to €2.42 (*R-CC*) and €2.66 (*R-NC*), respectively. Figure 2.3.1 already reveals that the reduction has different sources. In *R-CC*, in only 29% of the decisions

11. Equal earnings cannot arise in *Full Competition*. Naturally, other tournament outcomes are possible in treatments *Chance Competition* and *No Competition*. Corresponding situations are shown to the spectators, see Table 2.2.1 for the situations with two winners. However, these situations are markedly different. If both workers win, the total sum of earnings is doubled. Redistribution decisions for these situations are analyzed in Appendix 2.C. We observe only little redistribution and no treatment differences in these situations. If no worker receives a prize, redistributing the prize money is impossible.

Table 2.3.1. Impact of payoff interdependence on redistribution for treatments with randomness

	Amount redistributed to loser		
	(1)	(2)	(3)
Chance Competition	-0.668***	-0.563**	-0.617***
	(0.231)	(0.225)	(0.231)
No Competition	-0.425**	-0.281*	-0.310*
	(0.188)	(0.168)	(0.170)
Performance Winner (cent.)		-0.002	-0.002
		(0.002)	(0.002)
Δ Performance		-0.041***	-0.054***
		(0.004)	(0.008)
Chance Competition $\times$ Δ Performance			0.024**
			(0.011)
No Competition $\times$ Δ Performance			0.016*
			(0.009)
Constant	3.084***	2.949***	2.979***
	(0.136)	(0.122)	(0.124)
N	805	1326	1326
R^2	.03	.18	.19
p-value: CC vs. NC	.29	.21	.37
Avg. redistribution	2.7	2.6	2.6

This table presents OLS regressions using the money redistributed to the loser as the dependent variable. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively. Standard errors are displayed in parentheses and clustered at the participant level. Performance is centered on the average performance of those situations that are featured in all three treatments, see Table 2.2.1. Δ Performance is the difference between the performance of the winner and the loser. CC vs. NC tests the difference of treatments *R-CC* and *R-NC* in columns (2) and (3). Column (3) shows a joint test of the treatment effect and interaction effect with the performance difference.

are earnings equalized and in 22% of all cases is nothing transferred to the loser at all. By contrast, without any competition at all, close to half of the decisions (49.5%) result in equal shares. In treatment *R-NC*, spectators less often redistribute in such a way that the loser receives more than €3.

We present the estimates from the corresponding regression analyses in Table 2.3.1. Column (1) includes only situations 1 to 8 that are featured in all three treatments. Removing the direct interdependence of realized payoffs but keeping the competition in chances (*R-CC*) lowers the transfer to the loser by €0.67. In the absence of any competition (*R-NC*), the coefficient is −0.43 and thus somewhat smaller in size, but does not significantly differ from *R-CC*. These treatment effects are robust once we include all decisions in which only one worker receives a prize, and control for the winning performance as well as the performance difference between the winner and loser (column (2)).[12] This column highlights that spectators tend not to condition their redistribution decision on the absolute performance level; rather, they strongly respond to the performance differences between the two workers. In *Full Competition*, if both workers perform equally, the predicted transfer is very close to the equal split (€2.95). *Ceteris paribus*, an additional performance difference of ten tasks leads to a reduction of €0.40 in the transfer to the loser. Symmetrically, a higher performance of ten tasks by the loser is also associated with a €0.40 increase in transfer to the loser.

Notably, the impact of performance differences changes across treatments (column (3)): if the interdependence in realized payoffs is removed, the impact of the workers' performance differences is significantly muted; spectators reaction to a ten-task change in the performance difference drops from €0.50 to €0.30.

In summary, the absence of interdependence in realized payoffs affects redistribution behavior in two separate ways. First, if high earnings for one person prohibit high earnings for another, inequality is accepted significantly less compared with a situation in which both can win simultaneously. Second, without this interdependence, redistribution decisions react less to individuals' performance differences. Hence, inequality that is not backed up by performance is more frequently accepted.

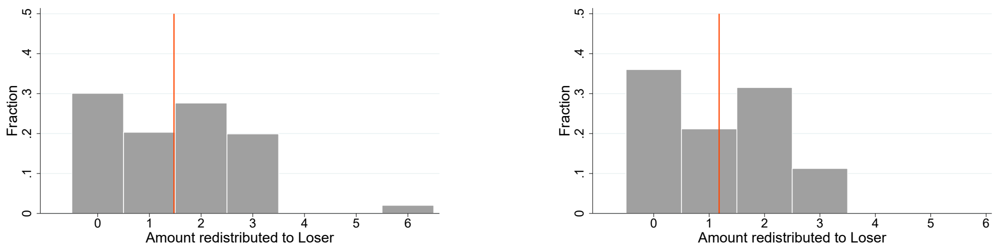
2.3.2 Payoff Interdependence in a Deterministic Setup

We support our results with a second set of treatments. Here, we again vary the degree of payoff interdependence between full competition (*D-FC*) and no competition (*D-NC*) but eliminate the impact of randomness. This means that under full competition the better-performing worker wins with certainty and in the absence of competition the workers receive a prize if their performance exceeds a randomly-drawn threshold. This also implies that the lower-performing worker can never receive the prize at the expense of the better-performing one. We therefore expect a lower baseline level of redistribution.

Figure 2.3.2 displays redistribution decisions for both treatments.[13] If the two workers are directly competing against each other, the spectators transfer €1.48 to the loser on average. This is much lower than in the previously presented treatments, which involve randomness. Unsurprisingly, almost no spectator transfers more than half of the prize to the loser; rather, 30% do not redistribute at all, while 20% choose to equalize earnings. Once the two workers no longer compete against each other

12. The performance of the winner is centered around the mean performance in situations 1 to 8. This means that the constant represents the amount redistributed in *Full Competition* if both workers have an average performance and the main effects (column (1)) are also evaluated at this mean.

13. These are situations 2, 4, 9, 11, 25, and 26 of Table 2.2.1.



(a) Deterministic - Full Competition **(b)** Deterministic - No Competition

The figure presents the histogram of the money transferred to the loser for the two deterministic treatments. The vertical red lines indicate the mean level of money transferred. The figures include only transfers for those situations that were featured in both treatments.

Figure 2.3.2. Amount redistributed to the loser for deterministic treatments

but against the same threshold, the average transfer is reduced by 20.2% to €1.18. While earnings are rarely equalized (11.2%), more than one-third choose to transfer nothing.

The corresponding estimations are presented in Table 2.3.2. Equivalent to Table 2.3.1, column (1) includes only directly comparable situations that are featured in both treatments. While this estimation indicates that under no competition redistribution is reduced by €0.30, this effect is not significant. However, once we include all situations with one winner and control for the performance level of the winner[14] and performance difference (column (2)), we find a significant effect of payoff interdependence on inequality acceptance. Again, performance difference has a significant impact and the estimated coefficients are close to those estimated for the treatments including randomness. Column (3) allows the impact of performance to differ by treatment. Reassuringly, we find the same pattern as above: the reaction to the size of the difference between the tasks the workers solve is significantly lower once the payoffs are not directly related.

In summary, the results of our second set of treatments support our findings: even without any randomness in the allocation process, the direct interdependence of payoffs causes spectators to accept inequality in payoffs less and prompts them to react more strongly to the workers' performances.

2.3.3 The Role of Randomness for Redistribution

In addition to investigating the effect of payoff interdependence, our design allows to identify the causal impact of the pure presence of randomness in a prize allocation process on the demand for redistribution. In previous studies, the involvement of luck has implied uncertainty about the performance rank of the winner

14. The performance of the winner is centered around the mean performance in situations that are featured in both treatments.

Table 2.3.2. Impact of payoff interdependence on redistribution for deterministic treatments

	Amount redistributed to loser		
	(1)	(2)	(3)
No Competition	-0.295	-0.410**	-0.612***
	(0.211)	(0.197)	(0.225)
Performance Winner (cent.)		-0.004**	-0.004*
		(0.002)	(0.002)
Δ Performance		-0.044***	-0.049***
		(0.003)	(0.004)
No Competition $\times$ Δ Performance			0.015**
			(0.006)
Constant	1.476***	2.261***	2.318***
	(0.171)	(0.147)	(0.148)
N	468	644	644
R^2	0.015	0.23	0.23
Avg. redistribution	1.3	1.5	1.5

This table presents OLS regressions using the money redistributed to the loser as the dependent variable. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively. Standard errors are displayed in parentheses and clustered at the participant level. Performance is centered on the average performance of those situations that are featured in both treatments, see Table 2.2.1. Δ Performance is the difference between the performance of the winner and the loser.

of the contest. In our design, since the spectators are fully informed about the workers' performances, they know for sure whether the high-performing subject wins the prize. In particular, we can compare tournament situations where the higher-performing worker always wins with certainty with those in which a lower-performing worker can win in principle. Here, we consider only those situations where the higher-performing worker wins. Hence, in these cases the tournaments are outcome-equivalent, whereby from an ex post perspective randomness makes no difference on the prize allocation.[15]

Table 2.3.3 displays our results. Column (1) includes only the decisions made for situations 2 and 4 from Table 2.2.1, as they are the only situations featured in all four treatments. If performances and the resulting payoffs are kept entirely constant, randomness in the allocation process increases the amount redistributed to the low-performing worker by €0.61, marking an 46% increase compared with the

15. We compare *R-FC* and *R-NC* with the two equivalent deterministic treatments. Naturally, there cannot be a counterpart for competition in chances without the presence of randomness. Hence, treatment *R-CC* is dropped from the subsequent analysis.

Table 2.3.3. Impact of randomness without uncertainty on redistribution

	Amount redistributed to loser				
	(1)	(2)	(3)	(4)	(5)
	FC & NC	FC	NC	FC & NC	FC & NC
Randomness	0.614***	0.410**	0.911***	0.588***	0.516***
	(0.161)	(0.202)	(0.177)	(0.140)	(0.151)
Performance Winner (cent.)		-0.006***	0.004	-0.003	-0.003
		(0.002)	(0.003)	(0.002)	(0.002)
Δ Performance		-0.043***	-0.037***	-0.042***	-0.048***
		(0.004)	(0.004)	(0.003)	(0.004)
No Competition				-0.228*	-0.289*
				(0.135)	(0.147)
Randomness $\times$ Δ Performance					0.007
					(0.005)
No Competition $\times$ Δ Performance					0.006
					(0.005)
Constant	1.314***	2.269***	1.684***	2.171***	2.235***
	(0.114)	(0.149)	(0.149)	(0.132)	(0.132)
N	322	677	460	1137	1137
R^2	.055	.2	.32	.24	.24
Avg. redistribution	1.6	1.9	1.7	1.8	1.8

This table presents OLS regressions using the money redistributed to the loser as the dependent variable. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively. Standard errors are displayed in parentheses and clustered at the participant level. Performance is centered on the average performance of the featured treatments. Δ Performance is the difference between the performance of the winner and the loser.

treatments without randomness. As in the previous sections, the remaining columns include further decisions that are not entirely identical between treatments but control for the performances of the workers. Columns (2) and (3) conduct the analysis separately for the treatments with full competition (*FC*) and without any competition (*NC*), respectively. Column (4) pools all treatments, controlling for the interdependence of payoffs. All specifications show a significant positive impact of the presence of randomness on the amount transferred to the loser. Finally, column (5) reveals that the impact of the workers' performance differences does not differ between the treatments that feature randomness and those that do not.

In summary, randomness influences inequality acceptance beyond making it more difficult to discern whether the inequality is based on merit or performance. Rather, the mere presence of randomness in an allocation process – allowing the low-performing individual a chance of success – makes spectators redistribute more, independent of the payoff interdependence. This suggests that in situations involving randomness, subjects not only equalize incomes out of the fear of harming the high-performing people but also act to acknowledge the unfortunate ones' ex ante chance of success.

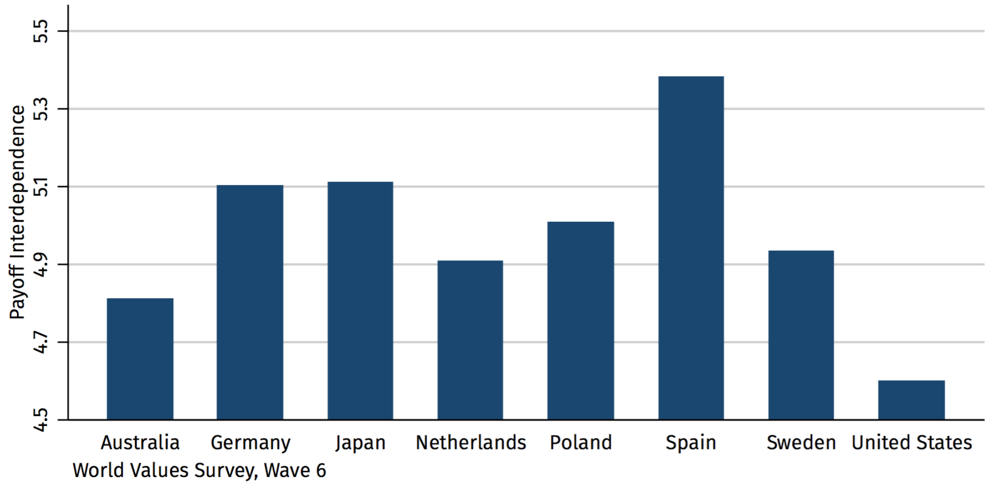
2.4 Conclusion

In this paper, we experimentally provide evidence that more inequality is accepted when payoffs are not interdependent. When two individuals compete for one high outcome, on average 25% to 50% of the prize is redistributed to the losing person ending up with the low outcome. Removing the interdependence of realized payoffs, namely allowing both persons to win simultaneously, reduces redistribution by 14% to 22%. Whether chances of winning are totally independent between persons or an interdependence in expected payoffs still exists has no additional impact on the demand for redistribution. This holds true for situations with and without randomness being present in the allocation process.

In general, we interpret our findings such that people do not solely focus on realized states; rather, they seem to take all states into account that are possible ex ante, irrespective of their actual realization. Accordingly, once payoffs are not totally interdependent, spectators seem to include the possibility that both workers could have won simultaneously in their redistribution decisions. By contrast, whenever only one worker can win at a time, this high income is perceived as being taken away from the loser. This is inconsistent with any existing theoretical model of social preferences (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002). These models solely consider inputs and outcomes but do not regard the underlying payoff-generating mechanisms and hence do not incorporate the interdependence of payoffs into the utility functions. The tendency to include unrealized states into one's future decision-making therefore hints at a broader behavioral mechanism that is currently missing in the theoretical literature on social preferences.

In summary, this paper highlights a novel source for fairness views and demand for redistribution. The perception of the state of the economy – whether growth exists or wealth is only possible at the expense of others – potentially affects political attitudes towards redistribution. Thus, informing people about the actual interdependence of payoffs can also have major consequences for these attitudes. Employers and supervisors might also be interested to include the effect of payoff interdependence on fairness perception in wage-setting decisions. If bonus pools are fixed and promotion chances are limited, this could result in a heightened sense of unfair inequality and even induce lower effort in the first place. Furthermore, independent payoffs might result in higher motivation.

Appendix 2.A World Values Survey



The figure presents the mean beliefs in the interdependence of payoffs in the western countries that are featured in wave 6 of the World Values Survey (Inglehart et al., 2014).

Figure 2.A.1. Beliefs in the interdependence of payoffs of western countries

Appendix 2.B Worker Behavior Across Treatments

The analysis of the redistribution decisions does not rely on the worker behavior elicited in the first stage of the experiment, but rather on the hypothetical pairs. Nonetheless, we can analyze the extent to which the different treatments induce variation in performance. Since the workers are not invited to the lab but rather take part via an online study, all of them have a true outside option and can spend their time freely. In addition, we elicit expectations for the average amount redistributed for each treatment.

Table 2.B.1. Summary statistics for workers

	R-FC	R-CC	R-NC	D-FC	D-NC	Total
Performance	44.10	47.21	45.15	47.23	50.18	46.70
	(18.95)	(18.50)	(18.44)	(18.72)	(18.69)	(18.68)
Expected	2.837	2.551	2.581	2.207	2.311	2.500
Redistribution	(1.012)	(1.015)	(1.260)	(0.926)	(1.404)	(1.150)

This table reports the average number of tasks solved by treatments and average amount of redistribution workers expect to be redistributed in their treatment. Standard deviations are displayed in parentheses.

Looking at Table 2.B.1, we find that the average performance slightly varies across treatments. Notably, we find that workers in treatments without any luck

Table 2.B.2. Worker behavior across treatments

	(1) Performance	(2) Expected redistribution
R-CC	2.673	-0.269
	(2.879)	(0.180)
R-NC	1.825	-0.275
	(2.811)	(0.176)
D-FC	4.094	-0.654***
	(2.846)	(0.178)
D-NC	5.737**	-0.516***
	(2.916)	(0.182)
male	9.787***	-0.257**
	(1.846)	(0.115)
Age	-0.100	-0.007
	(0.178)	(0.011)
Constant	42.078***	3.117***
	(4.701)	(0.294)
N	400	400
R^2	.078	.051

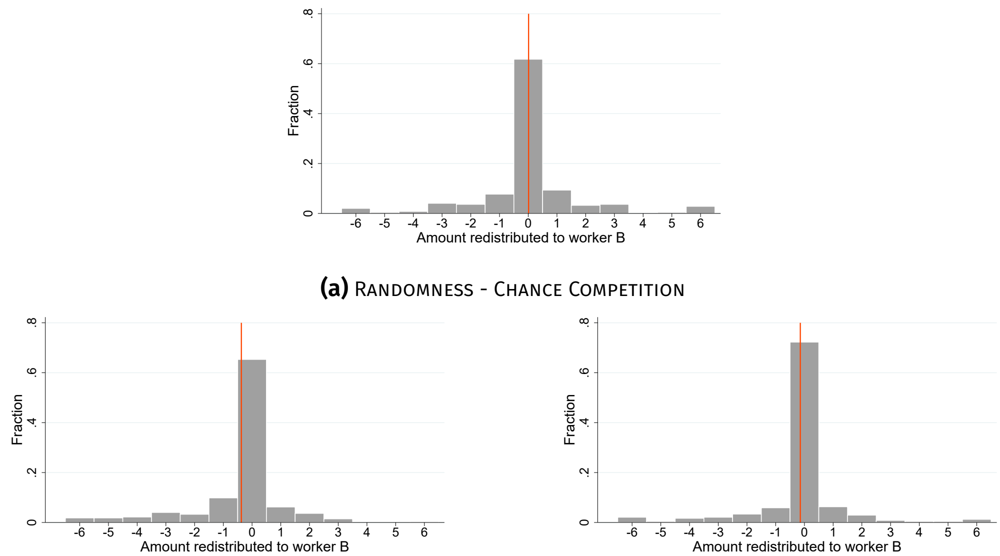
This table presents OLS regressions using the workers' performance (Column (1)) and the elicited expected redistribution (Column (2)) as outcomes. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively. Standard errors are displayed in parentheses and clustered at the participant level..

have a higher performance compared with the other treatments. The regression in Table 2.B.2 reveals that these differences are only statistically significant for the comparison of treatment *NR-NC* with *R-NC*, controlling for demographics of the subjects.

Similarly, workers expect levels of redistribution to be lower in the treatments without any randomness involved. Since we only elicit an average belief without mentioning specific performance levels, this clearly has mechanical reasons. Focusing on the beliefs for the treatment with luck, we find that workers expect a slightly, but not significantly, lower level of redistribution for the treatments without direct dependence in payoffs.

Appendix 2.C Redistribution if Both Workers Receive a Prize

In the treatments without direct dependency of payoffs (*R-CC*, *R-NC*, and *NR-NC*), both workers can receive high earnings simultaneously. In order to identify the specific impact of this interdependence, our main analysis of the paper we focuses on those situations where only one player actually wins. As explained in Section 3.3,



(a) Randomness - Chance Competition

(b) Randomness - No Competition

(c) Deterministic - No Competition

The figure presents the histogram of the money transferred to worker B when both workers receive a prize. The vertical red lines indicate the mean level of money transferred. Negative values imply that the spectator transfers money from worker B to worker A. The figures include only decisions for those situations that were featured in all three treatments.

Figure 2.C.1. Amount redistributed between workers when both win a prize

we also present the spectators with situations where two workers win. Naturally, the spectators might still want to redistribute earnings. However, in these situations equality in payoffs is the default setting. If spectators care about (monetary) equality, they will not change the allocation in these situations. Hence, we do not expect any treatment differences.

The redistribution decisions for the three treatments with two potential prizes are displayed in Figure 2.C.1. Here, the x-axis denotes the amount of money distributed to worker B. Accordingly, if spectators choose to redistribute nothing, worker B receives her prize of €6. In general, we find very little redistribution in these situations. In all treatments, spectators choose to not redistribute anything at all in more than 60% of the situations. Spectators deviate on average from the equal split by less than €1: they redistribute €0.91 in *R-CC*, €0.77 in *R-NC*, and €0.66 in *NR-NC* (either from worker A to worker B, or the other way around). In Table 2.C.1, we present the results of a corresponding regression analysis. As redistribution is not one-directional (from worker A to B) but can go both ways, we use the deviation from the equal split as the dependent variable, such that we treat workers A and B symmetrically. We do not find any significant effect of the treatments, which implies that the treatments do not influence the redistribution decisions differentially.

Table 2.C.1. Impact of treatments on redistribution when both workers win a prize

	Deviation from equal split		
	(1)	(2)	(3)
No Competition	-0.202	-0.128	-0.153
	(0.240)	(0.187)	(0.217)
No Competition	-0.225	-0.225	-0.267
	(0.257)	(0.190)	(0.219)
Δ Performance		0.040***	0.038***
		(0.005)	(0.009)
No Competition $\times$ Δ Performance			0.002
			(0.011)
No Competition $\times$ Δ Performance			0.004
			(0.013)
Constant	1.333***	0.466***	0.488***
	(0.170)	(0.150)	(0.176)
N	357	758	758
R^2	.0038	.16	.16

This table presents OLS regressions using the absolute deviation from the equal split as the dependent variable. *, ** and *** denote significance at the 10, 5 and 1 percent level, respectively. Standard errors are displayed in parentheses and clustered at the participant level.

Column (3) additionally interacts the influence of performance differences with the treatment and does not reveal any significant effect either.

Appendix 2.D Instructions

These are the instructions (translated from the German original) for the first stage (workers) and the second stage (spectators). We indicate differences between treatments within each screen.

2.D.1 Workers

Screen 1–Welcome

You are now participating in a study of the BonnEconLab.

Please read the following instructions attentively. In this study, you can earn money depending on your own choices and those made by other participants. It is therefore very important that you read the instructions carefully and understand them.

How much money you will receive at the end of this study depends on your own decisions and those made by other participants.

For this study, you will be put in a group of three participants. That is your group gets assigned another two participants.

On the next page, you will get to know your role in this group and your task.

Please click on "Next".

Screen 2–Detailed information about the procedure of the study

In this study, you and a second participant from your group have the opportunity to work on tasks for up to twelve minutes.

Randomness:

For every completely solved task you will receive one lottery ticket. At the same time, the second participant receives one lottery ticket for every task he solved.

After the task-solving-phase, a lottery will determine your income.

In the following, you will get to know how this income is being determined:

Randomness-FC:

All lottery tickets, that is yours as well as the ones from the other participant, will be placed in one urn. Out of this urn one ticket is drawn at random. This means that for every task you solved you put one ticket in the urn. At the same time, for every task solved by the second participant he puts one ticket in the urn. Subsequently, one ticket is randomly drawn from the urn and the owner of this ticket receives a prize.

The owner of the drawn ticket receives a prize of €7 and the other participant receives €1.

Example: Assume that you solved 30 tasks and the second participant solved 20 tasks. This means you are putting 30 tickets in the urn and the second participant puts 20 tickets in the urn. The possibility that one of your tickets is being drawn amounts to 30/(30+20) = 30/50 = 60%.

Randomness-CC:

Each of you has his own urn. Inside your urn are your tickets as well as a number of blanks corresponding to the number of tickets in the other participant's urn.

This means that for every task you solved you put one ticket in your urn and one blank in the other participant's urn. At the same time, for every task solved by the second participant he puts one ticket in his urn and one blank in your urn.

Subsequently, one ticket is drawn from every urn; one from your urn and one from the other participant's urn. In the case that one of your tickets is being drawn from your urn you will receive a prize. In case a blank is drawn you will not receive a prize. For the other participant, a random draw is taken from his urn as well.

If one of your tickets is being drawn you receive a prize of €7. Otherwise, you receive €1. The same applies to the other participant. This means that it is possible for either both of you to receive a prize, as well as only one or even none of you.

Example: Assume that you solved 30 tasks and the second participant solved 20 tasks. This means you are putting 30 tickets in your urn and the second participant puts 20 blanks in your urn. The possibility that one of your tickets is being drawn amounts to 30/(30+20) = 30/50 = 60%.

Randomness-NC:

Each of you has his own urn. Inside your urn are only your tickets and not those of the other participant.

This means that for every task you solved you put one ticket in your urn. In addition, both urns include as many blanks as another participant, who is not part of your group, solved tasks. This participant is not part of your group and his income is not dependent on yours. This participant and therefore the number of blanks inside your urn is chosen randomly.

Subsequently, one ticket is drawn from every urn; one from your urn and one from the other participant's urn. In the case that a ticket is being drawn from your urn you will receive an award. In case a blank is drawn you will not receive an award. For the other participant, a random draw is taken from his urn as well.

If one of your tickets is being drawn you receive a prize of €7. Otherwise, you receive €1. The same applies to the other participant. This means that it is possible for both of you to receive a prize, as well as only one or even none of you.

Example: Assume you solved 30 tasks and the randomly chosen participant solved 20. This means you are putting 30 tickets in your urn and the randomly chosen participant puts 20 blanks in your urn. The possibility that one of your tickets is being drawn amounts to 30/(30+20) = 30/50 = 60%.

Deterministic:

After the task-solving-phase, the number of completed tasks will be compared and your incomes will be determined.

The number of completed tasks will be compared as follows:

Deterministic-FC:

The one of you who solved the most tasks will receive a prize.

If you solved more tasks than the other participant you receive €7. In case you solved less, you receive €1.

Example: Assume you solved 30 tasks and the second participant solved 20 tasks. As you solved more tasks than the second participant you receive a prize.

Deterministic-NC:

The number of tasks solved by you and the second participant will be compared to the number of tasks solved by another participant for both of you. This participant is not part of your group and his income does not depend on your solved tasks. This participant and therefore the amount of solved tasks is chosen randomly.

You receive a prize in the case that you solved more tasks than this randomly chosen participant. The same applies to the other participant from your group.

If you solved more tasks than the randomly chosen participant you receive €7. Otherwise, you receive €1. The same applies to the other participant. This means that it is possible for both of you to receive a prize, as well as only one or even none of you.

Example: Assume you solved 30 tasks and the randomly chosen participant solved 20 tasks. As you solved more tasks than the randomly chosen participant you receive a prize.

Decisions of the third participant

You will not be informed about your and the other participant's income directly after the study.

First, the third participant of your group, who does not participate in this part of the study, has the chance to reallocate your incomes. He knows the task that you had to solve but did not solve any tasks himself. However, this participant will be informed about the exact course of the study and the amount of tasks solved by you and the second participant. This means that he knows your and the second participant's income as well as the number of tasks solved by each of you.

The third participant has the chance to reallocate the incomes. He can redistribute up to €6 among you. Obviously, he can also choose to not change the incomes.

After the decision of the third participant and the completion of the study, your payout will be transferred to your bank account. Please note that this might take some time and you will receive the money in two weeks. We will inform you as soon as the transfer has been commissioned. At the same time, you will receive information about the choice of the third participant.

On the next screen the task will be explained. Please click on "Next".

Screen 3–Detailed information about the task

Your task is to change the position of sliders. For each task five sliders will be presented to you on one screen. Each slider starts on the very left (position 0) and can be moved until the far right end of the scale (position 100). The current position of the slider is shown on the slider. The slider can be moved in three different ways: **by making use of the arrow keys, by moving the mouse or by clicking on the scale.**

Your task is to move the slider to the **middle position (position 50)**. The sliders can be worked on in any order. You can move a slider as many times as you want to and correct its position. Only after all five sliders have been moved to position 50, you will be able to reach the next task by clicking on the “Next” button. In total, you will have 12 minutes of time to solve as many tasks as possible. After that, this part of the study ends. Please click “Next” to start with the solving of the task.

Screen 4–Slider task

No instructions.

Screen 5–Feedback

You solved X tasks.

Your payout is dependent upon the result of the second participant as well as on the choice of the third participant in your group.

We kindly ask you to answer some further questions. After this, the study ends for you. We will inform you once your transfer has been commissioned.

2.D.2 Spectators

Screen 1–Welcome

You are now participating in a scientific study. You will receive at least €8 for your participation. We ask you to carefully read the following instructions. If you have any question, please raise your hand and we will come to you.

In this study, you have the possibility to reallocate the income of two participants. For this, we will show you the income of several pairs of participants. These participants had the opportunity to solve tasks and gain a prize of €7. The participants who did not gain a prize received €1. Whether the participants received a prize or not, did depend on the number of tasks solved by both participants. Participants had twelve minutes to solve the following tasks:

For each task, the participant faced a screen with five sliders. Each slider starts on the very left (position 0) and can be moved until the far right end of the scale (position 100). The task is to move the slider to the middle position (position 50) by making use of the arrow keys, moving the mouse or clicking on the scale.

Once all five sliders were in the correct position and the participant clicked "Next", the task was counted as solved.

In order to get a better understanding of the task, you will now be able to test the task for one minute.

Screen 2–Slider task

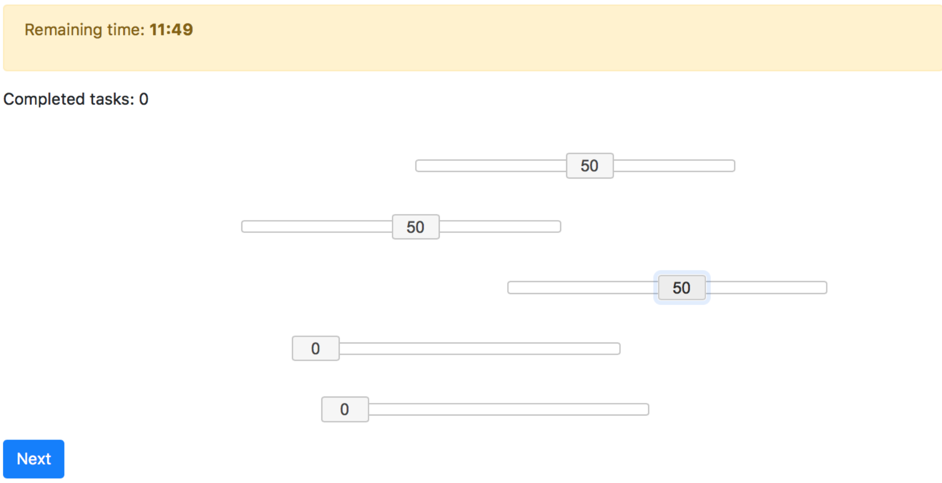


Figure 2.D.1. Screenshot of the slider task in the experiment

Screen 3–Your Result

You solved X tasks in one minute.

Screen 4–Determination of incomes

In the following we explain how incomes are determined.

Randomness

Receipt of tickets

Both participants (participant A and participant B) could gain tickets by solving the task which you just tested. For each solved task they received one ticket. This means that the more tasks one participant solved the more tickets he received. The awarding of the prize was being determined by drawing tickets.

Randomness-FC

Awarding of the prize

For this, all tickets were placed in an urn. Out of this urn, one ticket was drawn. The owner of this ticket received a prize of €7, the other participant received €1. Therefore, always exactly one of the two participants received a prize.

Example: If participant A solved 25 tasks he received 25 tickets. Assume participant B solved 15 tasks, he received 15 tickets. Therefore, a total of 40 tickets were in play. The probability of receiving the prize amounted to 25/(25+15)=62.5% for participant A and 37.5% for participant B.

Randomness-CC

For this, the tickets were placed in two urns. The tickets from participant A were placed in an urn for participant A, the tickets from participant B in an urn for participant B. However, for every ticket that was placed into the urn of participant A, one blank was placed into the urn of participant B. The same procedure was applied to the tickets of participant B and the urn of participant A. This means that the urn of participant B included as many tickets as he solved tasks and as many blanks as participant A solved tasks.

Awarding of the prizes

One draw was conducted from each urn. If a ticket of participant A was drawn from participants A's urn, he received a prize of €7. If a blank was drawn he received €1. If a ticket was drawn from participant B's urn he as well received a prize of €7. If a blank was drawn he received €1. Therefore, both participants could receive a prize, as well as only one or even none of them.

Example: If participant A solved 25 tasks, 25 tickets were placed in his urn. Assume that participant B solved 15 tasks, 15 blanks were placed in the urn of participant A. Therefore, a total of 25 lots and 15 blanks were in the urn of participant A. With this the probability of receiving a prize amounted to 25/(25+15)=25/40=62.5% for participant A. At the same time, 15 lots and 25 blanks were in the urn of participant B. With this the probability of receiving a prize amounted to 15/(15+25)=15/40=37.5% for participant B.

Randomness-NC

For this, the tickets were placed in two urns. The tickets from participant A were placed in an urn for participant A, the tickets from participant B in an urn for participant B. In addition, a random number of blanks was placed in both urns. The number of blanks corresponded to the amount of tickets another, third participant gained by solving tasks. This randomly chosen participant did not have any other connection to participants A and B.

Awarding of the prizes

One draw was conducted from each urn. If a ticket of participant A was drawn from participants A's urn, he received a prize of €7. If a blank was drawn he received €1. If a ticket was drawn from participant B's urn he as well received a prize of €7. If a blank was drawn he received €1. Therefore, both participants could receive a prize, as well as only one or even none of them.

Example: If participant A solved 25 tasks, 25 tickets were placed in his urn. Assume that participant B solved 15 tasks, 15 tickets were placed in the urn of participant B. An additional 20 blanks were placed into each urn. Therefore, a total of 25 lots and 20 blanks were in the urn of participant A. With this the probability of re-

ceiving a prize amounted to 25/(25+20)=25/45=55.5% for participant A. A total of 15 tickets and 20 blanks were in the urn of participant B. With this the probability of receiving a prize amounted to 15/(15+20)=15/35=42.8% for participant B.
Randomness

To summarize: the more tasks a participant solved, the more tickets he received and the greater were his chances of receiving a prize.
Deterministic-FC

For both participants (participant A and participant B) the number of solved tasks was counted. The participant with the higher number of solved tasks received a prize of €7. The other participant received €1. In the case that both participants had solved exactly the same number of tasks, the prize was allocated randomly. Therefore, always exactly one of the participants received a prize.

Example: Assume that participant A solved 25 tasks and participant B solved 15 tasks, then participant A received the prize.
Deterministic-NC

For both participants (participant A and participant B) the number of solved tasks was counted. At the same time, a number of tasks was chosen randomly. The number of tasks corresponded to the number of tasks another, third participant had solved. This random chosen participant did not have any other connection to participant A and B.

If participant A had solved more tasks than the randomly chosen number, he received a prize of €7. If he had solved fewer tasks, he received €1. The same was applied to participant B. Therefore, both participants could receive a prize, as well as only one or even none of them.

Example: Assume participant A had solved 25 tasks and participant B had solved 15 tasks. 20 tasks were chosen randomly. Consequently, participant A received a prize.
Deterministic

To summarize: the more tasks a participant solved, the bigger were his chances of receiving a prize.

Screen 5–Hypothetical pairs

We will present to you a total of X pairs of participants. For every pair, we will show you how many tasks participant A and participant B each solved, as well as the current income of both participants.

Out of all the pairs that we will present to you, one pair is from the BonnEconLab. All the other pairs are fictional and do not represent real pairs. When you are making your decision you do not know which one of the pairs is not fictional. Please note that each of your decisions might become relevant for two participants of the BonnEconLab. You thus determine the payout for those two participants.

Both participants have not yet been informed about their current income and will only get to know their payout as determined by you.

If you have any questions please raise your hand.

Screen 6–Control questions

Before this study starts we ask you to answer some control questions:

1.) When does a task count as solved?

a. Once the time is over.

b. Once all sliders have been moved to position 50 and the "Next" button has been clicked.

c. Once at least one slider has been moved to position 50 and the "Next" button has been clicked.

2.) How many participants can win a prize at most?

a. None.

b. One participant.

c. Two participants.

Deterministic

Assume that participant A solved 24 tasks and participant B solved 12 tasks.

Deterministic-NC

A random third participant with 6 solved tasks is chosen.

Deterministic

3.) Which income does each of the participants receive?

Randomness

Assume that participant A solved 24 tasks and participant B solved 12 tasks.

3.) How many tickets did each of the participants receive?

Randomness-NC

By random choice it is determined that 6 blanks will be added to each urn.

Randomness

4.) What is the probability to win for participant A?

Randomness-FC & CC

a. Number of tickets participant A / Number of tickets participant B.

b. Number of tickets participant B / (Number of tickets participant A + participant B).

c. Number of tickets participant A / 100.

d. Number of tickets participant A / (Number of tickets participant A + participant B).

Randomness-NC

a. Number of tickets participant A / Number of blanks.

b. Number of tickets participant B / (Number of tickets participant B + number of blanks).

c. Number of tickets participant A / (Number of tickets participant A + number of blanks).

d. Number of tickets participant A / (Number of tickets participant A + participant B).

Screen 8–Redistribution Decision

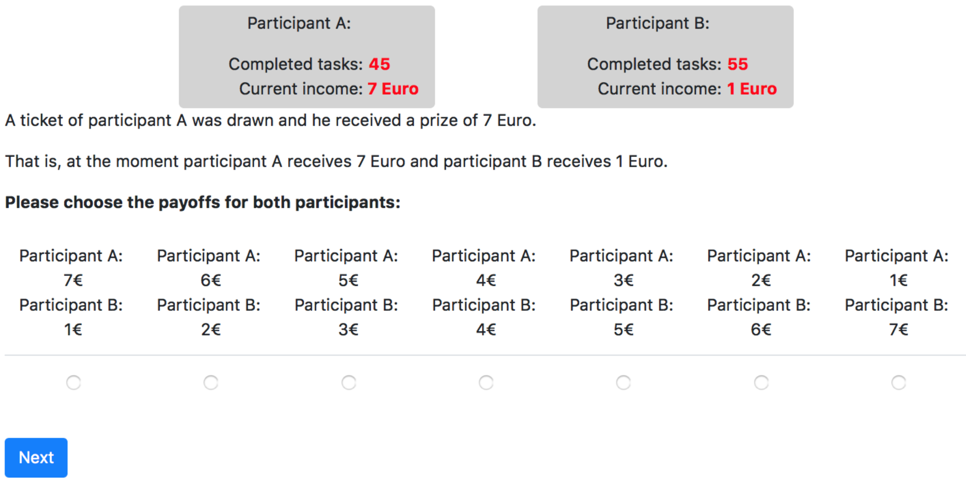


Figure 2.D.2. Screenshot of the decision screen in the experiment

Screen 9–Choice real pair

In the following, we once again show you all pairs for which you just determined the payout. As already explained, only one of those pairs is a non-fictional pair. Please indicate which of those pairs you consider to be the non-fictional one. If you choose the right pair, you will receive an additional payment of €1. If you do not choose the right pair you will not receive any additional payment.
[list of pairs]

Screen 10–Feedback real pair

You chose the right pair and will receive an additional payment of €1.
or

You did not choose the right pair.

References

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Chapter 3

Image Concerns and the Dynamics of Prosocial Behavior*

Joint with Jana Hofmeier

3.1 Introduction

Prosocial behavior is a pervasive facet of human interactions. Humans volunteer, give money to charities, donate blood, and help friends as well as strangers. All of these activities evoke personal costs but people are nonetheless willing to make sacrifices to increase social welfare (Charness and Rabin, 2002). Such behavior is often understood to reflect social preferences.[1] Ample evidence suggests that social preferences positively affect economic success (Carpenter and Seki, 2011; Becker, Deckers, Dohmen, Falk, and Kosse, 2012; Algan, Beasley, Vitaro, and Tremblay, 2014) and well-being (Dunn, Aknin, and Norton, 2008; Park, Kahnt, Dogan, Strang, Fehr, et al., 2017) in several contexts.[2] Policy makers and corporations may hence wish to foster the prevalence of social preferences to obtain its benefits. However, the current state of knowledge on the malleability and the development of social preferences allows little guidance, as our understanding of the matter is still quite limited.

We experimentally investigate how prosocial behavior, one expression of social preferences, can be fostered over time. One particular variable that can affect prosocial behavior is observability. It has repeatedly been shown that people behave differ-

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1. Important manifestations of social preferences are, for instance, altruism (Becker, 1974; Becker, 1976), inequity aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), reciprocity (Rabin, 1993; Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006), and warm glow (Andreoni, 1989).

2. Note that the correlation between social preferences and economic success could also be explained by a respective correlation of both variables with IQ (Burks, Carpenter, Goette, and Rustichini, 2009)

ently when others witness their actions (Zajonc, 1965; Guerin, 1983). In particular, being observed usually increases prosocial behavior because people want to be liked and respected by others (Ariely, Bracha, and Meier, 2009) or want to avoid resentment (DellaVigna, List, and Malmendier, 2012). These studies report, however, only the change of behavior during the observation itself. Beyond that, little is known about the sustainability of these positive observability effects and it is unclear how being observed affects the dynamics of prosocial behavior. We contribute to the existing research by investigating spillover effects of being observed during the decision over a prosocial act on subsequent prosocial behavior. We hypothesize that observability not only increases immediate prosocial behavior but has positive spillover effects on later behavior as well.

This hypothesis is motivated by a new—and at the same time ancient—approach to conceptualize the formation of altruistic attitudes. According to Aristotle's Nicomachean Ethics, virtues are formed through the practice of virtuous actions. In modern terminology, engaging in prosocial behavior becomes a habit and eventually changes the person's self-image, meaning the way they think about themselves. They henceforth keep up the prosocial behavior in order to avoid cognitive dissonance (Akerlof and Dickens, 1982). This idea is captured by the concept of *altruistic capital* that states that past altruistic behavior accumulates altruistic capital that enables individuals to internalize how actions affect others and finally increases future altruistic behavior (Ashraf and Bandiera, 2017). Being observed while doing something good should therefore increase later prosocial behavior. Being observed should, due to image concerns, increase immediate prosocial behavior compared to a situation in which one is not observed. This builds up altruistic capital, and has therefore positive spillover effects on subsequent behavior. Moreover, performing good deeds in front of others makes a given action more salient, intensifies the experience and might therefore have stronger effects on a person's self-image adjustment. Furthermore, it should also change people's beliefs about their social image; the way they presume that others think about them. These image changes lead to a stronger increase of altruistic capital. We capture these mechanisms in a theoretical framework and derive our hypotheses formally.

We conduct two laboratory experiments to test if observability of earlier prosocial actions influences later levels of prosocial behavior. The experiments differ in the currency of giving in the later period (either money or effort) and in the mode of observability (either one single observer or a multi-people audience). In Experiment A, we find that prosocial behavior, as expected, and in accordance with prior research, increases when subjects are observed. We do not find such a difference in Experiment B. Moreover, we find only small and insignificant positive effects of early observability on subsequent prosocial behavior in both experiments.

We proceed as follows: Section 3.2 reviews the relevant literature, Section 3.3 describes the two experimental designs, Section 3.4 presents a theoretical model and derives predictions, Section 3.5 presents the results, and Section 3.6 discusses the results and concludes.

3.2 Literature

In economics, social preferences are traditionally understood to be persistent traits of individuals—complementing other dimensions of their enduring personality (Becker et al., 2012). For example, they have been found to be partially transmitted from generation to generation (Nunn and Wantchekon, 2011; Dohmen, Falk, Huffman, and Sunde, 2012). However, there likewise exists evidence that social preferences can be altered, for instance when interacting and receiving attention from a socially-minded mentor during childhood(Kosse, Deckers, Pinger, Schildberg-Hörisch, and Falk, no date). Moreover, altruistic behavior is highly context-dependent (Dana, Weber, and Kuang, 2007; Grossman, 2014): Certain features may trigger people to behave less prosocially—for instance, when contexts provide individuals with cues that potentially serve as excuses for not behaving prosocially or when the responsibility for certain outcomes is diffused. At the same time, other contexts promote prosocial behavior.

People have been shown to have image concerns, meaning they behave differently when others are present and can observe their actions. This can be due to an opportunity to display a convenient and normatively desired behavior, which is or is not in line with own preferences. Regarding prosocial behavior, this implies that individuals tend to behave more prosocially when they are observed, allowing them to obtain social recognition for their actions (Alpizar, Carlsson, and Johansson-Stenman, 2008; Ariely, Bracha, and Meier, 2009; Powell, Roberts, and Nettle, 2012; Bašić, Falk, and Quercia, 2018).

We seek to contribute to these findings by testing whether positive context effects of image concerns on prosocial behavior spill over to subsequent behavior, that is, spur circles of prosociality. In a broader context, we want to find out how prosocial behavior can be increased sustainably by gradually changing social preferences.

Our project builds on theoretical and empirical literature on dynamics of prosocial and moral behavior. When deriving our theoretical model of altruistic capital, we follow Ashraf and Bandiera (2017) who argue that past altruistic behavior accumulates altruistic capital which increases future altruistic behavior. Bénabou and Tirole (2011) offer an underlying mechanism which could explain such an accumulation process. In their model, agents gain utility from high self-esteem and make inferences about their true unknown moral type by observing their own past moral or immoral actions. Moral behavior is interpreted as an investment in one's self-image. The model yields the conclusion that, under certain conditions, good actions can build up *moral capital* and lead to further good actions, whereas bad actions destroy moral capital and lock in further wrongdoing.

Empirical evidence on the development of altruistic behavior stems from psychological and recent economic research. There is evidence on people compensate early moral or immoral behavior; it is observed that early prosocial actions lead to decreased prosociality later on, whereas early selfish actions lead to an increase in prosocial behavior (see Merritt, Effron, and Monin, 2010, for a summary). Schmitz (2019) reports results from an experiment on repeated social behavior in which subjects play a donation dictator game at two points in time. The second donation is smaller and this decrease is even stronger if both decisions happen within a day instead of having an extended period of one week between the two decisions.

However, there also exists evidence on the *foot-in-the-door-effect*, which refers to the phenomenon that the acceptance of a small initial request leads to a more probable acceptance of a larger request, which is made afterwards (Freedman and Fraser, 1966; DeJong, 1979; Beaman, Cole, Preston, Klentz, and Steblay, 1983). It is argued that this effect shows due to a change in self-perception of individuals who accept the first small request, which therefore is in line with our argument.

Gneezy, Imas, Brown, Nelson, and Norton (2012) experimentally investigate another dimension that is important for subsequent altruistic behavior. They claim that the development of a prosocial self-perception is only possible if prosocial acts involve personal costs. They find that people increase prosocial behavior only when the initial prosocial behavior was costly. Costless actions, in contrast, have no effect on subsequent prosocial decisions or can even decrease them. Our design incorporates this finding since subjects always have to invest time and effort or money in order to behave altruistically.

Building on these previous works on moral dynamics, social recognition, and the malleability of social preferences, we test not only the immediate effects of observability on prosocial behavior but in particular how later prosocial behavior is affected. We conjecture that social attention directed at one's good deeds leads to an adjustment of social-image and stronger adjustments of self-image. We therefore expect subjects to increase their later prosocial behavior if they have been observed beforehand.

3.3 Experimental Design

We investigate the causal effect of observability on present and future prosocial behavior by conducting two laboratory experiments. In both experiments, subjects face two sequential prosocial decisions within one session (see Figure 3.3.1 for an overview). We vary the observability of the subjects' first decision between treatments: in the Public-Private treatment, the first prosocial decision is observed by one observer or a group of observers, while the second prosocial decision is always made in private. In contrast, both decisions are made anonymously in the Private-Private treatment. We are primarily interested in second-stage prosocial behavior to evaluate the spillover effects of being observed on subsequent non-observed prosocial behavior. We run two variants of one experiment, which differ in the way donations are made and how observability is implemented.

Both experiments were conducted at the BonnEconLab using oTree (Chen, Schonger, and Wickens, 2016) and hroot (Bock, Baetge, and Nicklisch, 2014). Experiment A was conducted in August and September 2017 and a total of 242 subjects participated (including 37 subjects who served as observers). Experiment B was conducted in December 2017 and 77 subjects participated. Section 3.A includes verbal and written instructions for participants.

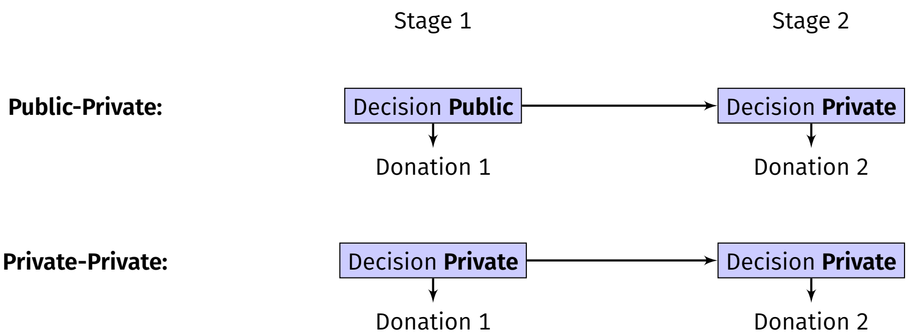


Figure 3.3.1. Treatment Overview

3.3.1 Experiment A

In Experiment A, subjects participate in one of two roles. A minority of the subjects functions as observers, that is they do not make any decisions themselves but solely monitor the behavior of other subjects. The remaining subjects, irrespective of the treatment, take the same two consecutive donation decisions.

Donation decisions are made as follows: In Stage 1, subjects can work on a real-effort task called *Counting Zeros* (first implemented by Abeler, Falk, Goette, and Huffman, 2011) to generate a donation to a project of the charity *SOS-Kinderdörfer*. In this task, subjects face 15×10 - tables, with all 150 cells each containing either the digits *0* or *1*. On each screen, containing exactly one table (see Figure 3.B.1 for a screenshot), subjects have to state the total number of zeros the table contains. Per correctly counted table,[3] the generated donation increases by a specific piece rate, which decreases in the number of completed tasks (see Table 3.3.1). To prevent subjects from simply guessing the correct number, we subtract €0.05 from the total

3. We allow for a error margin of +/- 1

donation for answers deviating by more than one from the correct number.[4] Subjects can freely choose to stop working at any time and can leave earlier when doing so. This allows for higher opportunity costs of exercising and hence more costly prosocial acts.[5] There is a maximum time of 20 minutes and a maximum number of 25 tables, resulting in a maximum donation of €2.90.

Stage 2 consists of a double-blind dictator game. In this stage, subjects open an envelope which they already receive at the beginning of the experiment. This envelope contains the subjects' compensation of €5 for participating in the experiment.[6] The envelope also contains written instructions and a smaller envelope. The instructions state that participants may leave any amount of the €5 in the small envelope to donate to a different project of the same charity as in Stage 1.[7]

We use a between-session treatment variation to prevent subjects from the Private-Private treatment being aware of any social component of the experiment. Sessions are conducted in turns, each one lasting at most 30 minutes. We now describe the exact procedure of each treatment.

Private-Private. For each Private-Private session, we invite three participants to the BonnEconLab.[8] At the beginning, they receive the aforementioned envelope, verbal instructions to open it at the end of the experiment, and information about the size of their compensation. Afterwards, they are sent to three separate rooms with one working space and one computer each. They are told to choose their respective rooms themselves to ensure a double-blind procedure and complete anonymity. Instructions for Stage 1 are already displayed on the computer screens when subjects enter the room and they immediately start with the experiment. In Stage 1, subjects work on the *Counting Zeros* task described above to generate a donation between €0 and €2.90. After subjects decide to stop working, they have solved the

Table 3.3.1. Experiment A: Piece rates for correctly solving a table of *Counting Zeros*

Tables solved	Piece rate
1 – 5	30 Cent
6 – 10	20 Cent
11 – 15	5 Cent
16 – 20	2 Cent
21 – 25	1 Cent

4. The total amount cannot become negative.
5. As mentioned before, Gneezy et al. (2012) emphasize the importance of positive costs.
6. Observers in the Public-Private condition receive a flat payment of €5 as well.
7. The €5 are provided in coins, such that all donations between €0 and €5 in steps of 10 Cent are possible.
8. In case that less than three subjects show up for a Private-Private session, the session is run with fewer subjects.

maximum number of tables, or time is up, they are informed about their generated donation and open the envelope which leads to Stage 2, which was not announced beforehand. After deciding how much money to donate in the dictator game, subjects leave without talking to or seeing the experimenter or any of the other subjects again.

Public-Private. For each Public-Private session, we invite one additional subject, resulting in a total of four subjects per session.[9] At the beginning of each session, all four subjects are seated at the same table and are asked to introduce themselves to each other by stating their first name and field of study.[10] Subsequently, one subject is randomly determined to act as an observer whose only role it is to monitor the performances of the remaining three subjects during Stage 1. After the observer is determined, he or she is separated from the other subjects and seated at a computer. On this computer, the other subjects' screens are displayed such that the observer can monitor their performances. Meanwhile, the other three subjects receive the same envelopes and the same information as subjects in the Private-Private treatment. Additionally, they are told that the observer will monitor their behavior and that each subject will have to report his or her outcomes to the observer in person. The observer is not aware of the envelopes to ensure the other subjects not feeling observed in Stage 2. From here on, the procedure of Stage 1 is identical to Public-Private. Only at the end of this stage, before moving on to Stage 2, when subjects are told about their donation, they are also asked to go to the observer and report their generated donation. Upon returning from the observer, they open the envelope which leads to Stage 2, which was not announced beforehand. The second stage proceeds in exactly the same way as in the Private-Private treatment, including complete anonymity. After deciding how much money to donate in the dictator game, subjects leave without talking to or seeing the experimenter, the observer or any of the other subjects again.

3.3.2 Experiment B

In Experiment B, all subjects, irrespective of the treatment, take the same two consecutive donation decisions. For a tighter control of the dynamics of prosocial behavior, we change, compared to Experiment A, the nature of the donation decisions. Instead of using different donation decisions in Stage 1 and Stage 2, we now use the same real-effort task in both stages. This allows detecting differences in prosocial behavior not only across treatments but also within-subjects between Stage 1 and Stage

9. In case that less than four subjects show up for a Public-Private session, we run the session with three or two subjects. If only one subject shows up, he or she is payed a show-up fee and sent home.

10. These personal interactions are used to create familiarity between subjects and have been used before. See, for instance, Gächter and Fehr (1999) and Ewers and Zimmermann (2015).

Table 3.3.2. Experiment B: Piece rates for a correctly pressed key combination of *Click for Charity*

Correct combinations	Piece rate
1 – 100	1.00 Cent
101 – 200	0.50 Cent
201 – 350	0.25 Cent
351 – 500	0.10 Cent
501 – 700	0.05 Cent
> 700	0.01 Cent

2. We now can make a statement on the prevalence of the altruistic capital effect in observed and non-observed situations.[11] Moreover, we change the observational mechanism. Subjects have to report their donation in front of all other subjects of the same session rather than reporting to a single observer to further increase the salience of observability.[12]

We first describe the task used for making donation decisions. We closely follow the design of Ariely, Bracha, and Meier (2009) using their real-effort task *Click for Charity* in both stages. The task consists of alternately pressing the keys "X" and "Y" on the computer keyboard[13] for five minutes. For each correct combination, a piece rate is donated to a project of the charity *SOS-Kinderdörfer*. Once again, the piece rate is concave and declines in the number of correct combinations (see Table 3.3.2). Figure 3.B.2 shows a screenshot of the task screen. Again, the projects differ between the two stages.

The experiment is conducted as follows: Subjects arrive at the laboratory and are randomly assigned to one of the two treatments. When receiving the instructions, subjects in the Public-Private treatment additionally learn that they will have to announce their first name and their generated donation from Stage 1 at the end of the experiment in front of all other participants of the session. Subjects in the Private-Private treatment do not receive this information and are not aware of the other treatment condition.[14] After practicing the above described *Click for Charity* task, they can work on it for five minutes to generate their Stage 1 donation. Note that none of the subjects is aware of Stage 2 during this phase. Only after finishing Stage 1, subjects receive written instructions for Stage 2, which follows the same

11. Note that one could argue that repeating the task might lead to fatigue and hence to a decline in performance of the second stage, or, contrary, learning might enhance performance. However, any time trend effects are orthogonal to our main treatment comparisons and therefore cannot explain any differences between observed and unobserved subjects.

12. On average, 22 subjects participate in one session.

13. Computer keyboards all have a German layout.

14. Subjects in both treatments only learn about the other treatment at the end of the experiment after both stages are completed and subjects of the Public-Private treatment have to announce their donations.

procedure as Stage 1. However, now all subjects are specifically informed that this stage's donation is completely anonymous.

Furthermore, we ask subjects for their level of happiness at the beginning and at the end (before the public announcement of donations) of the experiment.

Finally, participants receive a flat compensation of €6. Each session lasts at most 40 minutes.

3.4 Theory and Hypotheses

In this section, we derive a simple theoretical model and present the hypotheses that follow from it. According to Aristotle, people become virtuous by committing virtuous acts and thereby accustoming to it. We model this habitual formation with the assumption that people accumulate *altruistic capital* whenever doing something altruistic. When deriving our model of altruistic capital formation, we follow the approach of Ashraf and Bandiera (2017).

In period $t = 1, 2$, Agent i chooses an altruistic action $a_{i,t} \in [0, \bar{a}]$. The altruistic action generates social welfare $W\big(a_{i,t}\big)$, but creates a cost $c(a_{i,t}, A_{i,t})$ at the same time, where $W\big(a_{i,t}\big)$ is increasing and concave in $a_{i,t}$ and $c(a_{i,t}, A_{i,t})$ increases convex in $a_{i,t}$. The altruistic action $a_{i,t}$ does not only generate social welfare and create costs but also accumulates *altruistic capital* in the next period, denoted by $A_{i,t+1}$. Share u of the altruistic action increases social welfare in the same period, whereas share $1-u$ increases altruistic capital of the following period (this borrows from Lucas, 1988). Apart from this, altruistic capital builds up faster, the higher the parameter κ_t, which reflects a particular form of self-awareness. It reflects our understanding that higher image concerns make altruistic acts more salient and therefore enhance the internal habit formation process. Image effects are common to all agents but are situation-specific, as they depend for instance on the presence of an audience. In our experiment, we vary the effect of image in the first period between treatments, assuming that κ_t is increasing in public observability, that is $\kappa_t^{\text{Public}} > \kappa_t^{\text{Private}}$. In particular, altruistic capital in period t is $A_{i,t} = (1-u)\kappa_{t-1}a_{i,t-1} + (1-\delta)A_{i,t-1}$, where δ captures the depreciation rate of altruistic capital.

We argue that greater altruistic capital reduces the cost of acting altruistically as one accommodates to altruistic behavior. Having a prosocial identity (due to self- and/or social image adjustments) makes behaving prosocially less costly since it reduces cognitive dissonance and because the decision process becomes less difficult. We therefore assume that altruistic capital decreases the cost of acting prosocially, that is, $\partial c/(\partial a_{i,t} \partial A_{i,t}) < 0$. [15]

15. Ashraf and Bandiera (2017) assume that altruistic capital increases the marginal product of the altruistic action. Both assumptions are equivalent. We use cost reduction for the intuitive reason that habits reduce the cost of the decision process as well as of the action itself.

Finally, agent i's utility in period t is equal to $(\sigma_i + \theta_t)W\big(a_{i,t}\big) - c(a_{i,t}, A_{i,t})$. The utility increases proportionally in W for two reasons: First, the agent attaches a positive weight σ_i on W that represents her individual social preferences, such as pure altruism or warm glow. The second component, θ_t, expresses a second kind of image effects, where an agent simply wants to make a better impression while being observed (social image). We exogenously vary the parameter in our experiment, and we expect $\theta_t^{\text{Public}} > \theta_t^{\text{Private}}$. This image effect can be interpreted as the agent deriving utility from others thinking well of her (social image). The agent seeks to maximize her utility by choosing $a_{i,t}$.

Stage 1. As subjects are randomly assigned to treatments, we assume that previously accumulated altruistic capital and altruistic preferences, $A_{i,1}$ and σ_i, are equally distributed for both treatment groups. Hence, the only difference between treatments consists of the social observability. In the Public-Private treatment, we increase the social image parameter θ_1 and therefore the benefit of the generated social welfare.[16] Consequently, the agent has a higher return of her altruistic act and chooses a larger action $a_{i,1}$.

Hypothesis 5. *Subjects generate a greater donation in Stage 1 in the Public-Private treatment than in the Private-Private treatment.*

Stage 2. In the Public-Private treatment, observability occurs only in Stage 1 while subjects make their first decision. The subsequent donation decision in Stage 2 is completely private for all subjects and κ_2 and θ_2 should therefore be similar for both treatment groups. Altruistic capital $A_{i,2}$, however, is no longer equal as participants in the Public-Private treatment choose a larger action $a_{i,1}$ due to θ_1 and experience an additional increase due to a higher κ_1 (heightened self-awareness). This increases their altruistic capital stock with a higher rate, which in turn decreases the cost $c(a_{i,2}, A_{i,2})$ in period $t = 2$. A reduced cost makes it comparatively more attractive to engage in prosocial activities, which leads to our second hypothesis.

Hypothesis 6. *Subjects generate a greater donation in Stage 2 in the Public-Private treatment than in the Private-Private treatment.*

3.5 Results

3.5.1 Experiment A

In Experiment A, a total of 203 subjects participate as decision makers, 102 subjects in the Public-Private and 101 subjects in the Private-Private treatment. In Stage 1, in

16. As the existence of Stage 2 is unknown when making the decision for $a_{i,1}$, subjects are treating the optimization problem of Stage 1 in isolation from Stage 2.

which subjects can generate a donation by correctly counting zeros in tables, about 75% of all subjects solve at least five tables correctly and subjects quit, on average, after 15.9 attempts. This results in an average donation of €2.18 in the Public-Private treatment and €2.00 in the Private-Private treatment (out of a maximum of €2.90 if all 25 tables are solved correctly within 20 minutes). Subjects in the Public-Private treatment spend significantly more time working on the task (on average, around 14 minutes in the Private-Private treatment and around 17 minutes in the Public-Private treatment; the difference is significant to a level of 1%). However, almost all subjects sacrifice some time for the charitable act. In Stage 2, where subjects are no longer asked to spend time and effort but money, only 62% of subjects donate a strictly positive amount at all, albeit 12% give their complete show-up fee of €5. Nonetheless, the average donation is €1.30 in the Public-Private and €1.03 in the Private-Private treatment. Figure 3.5.1 displays donated shares of the maximum possible amount separately for the two treatment groups and for Stage 1 and Stage 2.

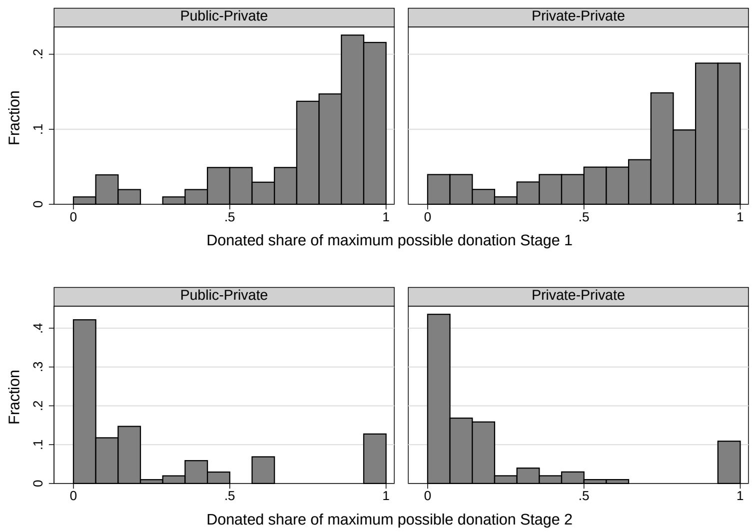


Figure 3.5.1. Donations in Experiment A: Donated share of maximum possible donation

Table 3.5.1 reports OLS estimates for Experiment A. In Column (1), Stage 1 donation is regressed on a treatment dummy, which is 1 if subjects are in the Public-Private treatment and 0 if they are in the Private-Private treatment. The coefficient is positive (subjects donate on average €0.18 more in the Public-Private treatment) and significant to a significance level of 10%. This is in line with Hypothesis 1. In Column (2), Stage 2 donation is regressed on the same treatment dummy. As stated in Hypothesis 2, the coefficient is positive (subjects donate on average €0.26 more in the Public-Private treatment), but not significant. In Column (3), Stage 2 donation is regressed on the treatment dummy, now additionally controlling for Stage 1

Table 3.5.1. Regression results: Experiment A

	(1) Donation 1	(2) Donation 2	(3) Donation 2	(4) Donation 2
Public	0.179*	0.257	0.269	0.804
	(0.105)	(0.226)	(0.230)	(0.803)
Donation 1			-0.0662	0.042
			(0.169)	(0.218)
Public x Donation 1				-0.254
				(0.342)
Constant	2.003***	1.039***	1.171***	0.954*
	(0.0796)	(0.154)	(0.389)	(0.489)
Observations	203	203	203	203
R^2	0.014	0.006	0.007	0.011

Coefficients in all columns are OLS estimates. Robust standard errors are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

donation. The coefficient of the dummy variable stays almost the same compared to Column (2). The coefficient of Stage 1 donation is close to zero, which suggests that a higher giving of Stage 1 does not per se induce higher giving in Stage 2 but observability itself induces higher giving. However, neither of the coefficients is significant. In Column (4), Stage 2 donation is regressed on the treatment dummy, Stage 1 donation, and the product of Stage 1 donation and the treatment dummy. The interaction term is negative, which could be a hint that for subjects in the Public-Private treatment Stage 1 donation has a negative effect on Stage 2 donation, speaking against a general altruistic capital effect. However, again, none of the coefficients is significant.

3.5.2 Experiment B

In Experiment B, a total of 77 subjects participate. 37 subjects are in the Public-Private and 40 subjects in the Private-Private treatment. In this experiment, subjects generate two donations by working on the real-effort task *Click for Charity* twice. Figure 3.5.2 shows the distributions of number of clicks per subject separately for each treatment group and each stage. We show graphics for performance levels instead of donations, since the concave piece rate leads to a low variation in actual donations. Therefore, performance levels give a more accurate picture of differences in behavior.

As in the previous experiment, almost all subjects engage in the task and generate a donation larger than zero. The average donation (pressed pairs) in Stage 1 is €2.12 (837.14) in the Public-Private and €2.10 (876.45) in the Private-Private treatment. We do not observe any decline in Stage 2 where the average donation (pressed pairs) is €2.13 (879.54) in the Public-Private and €2.03 (858.1) in the Private-Private treatment. Note that in Stage 1, average donations are higher in the Public-Private treatment, whereas average key combinations are lower. This is possible due to the concave piece rate which increases donations strongly in the beginning and only weakly in the end. In the Public-Private treatment, subjects press a lower total number of key combinations than in the Private-Private treatment, but the minimum number of pressed pairs is higher. This results in higher average donations.

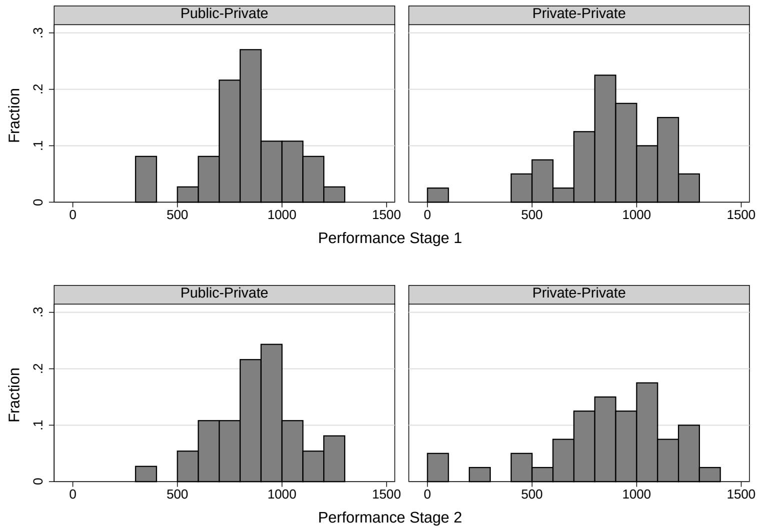


Figure 3.5.2. Performance levels (number of correctly clicked combinations) in Experiment B

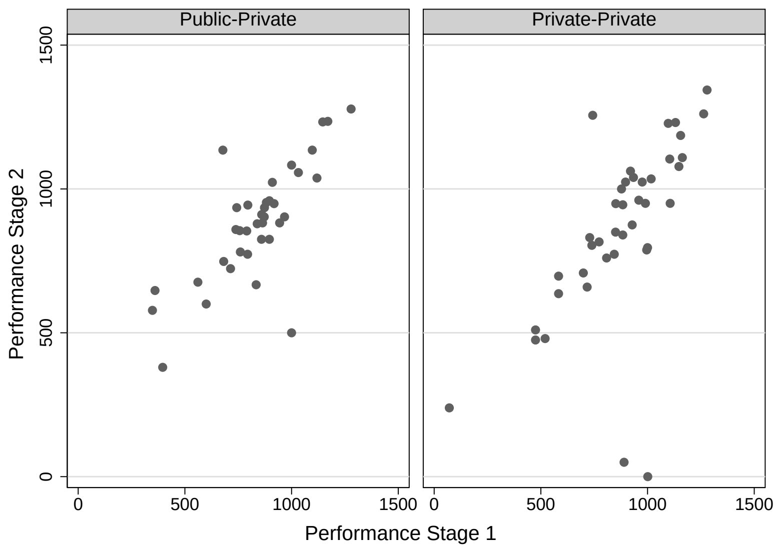


Figure 3.5.3. Relation of performance levels for Stage 1 and Stage 2 in Experiment B

As Figure 3.5.3 illustrates, we find a strong positive correlation ($\rho = 0.667$) of performance between stages. Also, the difference of performance between Stage 1 and Stage 2 is not significantly different from zero (using a t-test, $p = 0.637$), which shows that subjects do not decrease their prosocial behavior over time. We also observe that correlations between Stage 1 and Stage 2 performance in the Public-Private treatment ($\rho = 0.76$) and in the Private-Private treatment ($\rho = 0.64$) are not significantly different from each other.

Analyzing individual changes in performances between Stage 1 and Stage 2, we find that in the Public-Private treatment around 70.3% of subjects increase their performance between Stage 1 and Stage 2, whereas only 55% of subjects do so in the Private-Private treatment. This finding is visualized in Figure 3.5.4. The difference of 15 percentage points between treatments goes in the expected direction but is not significant (Wilcoxon Rank sum Test, p-value of 0.17).

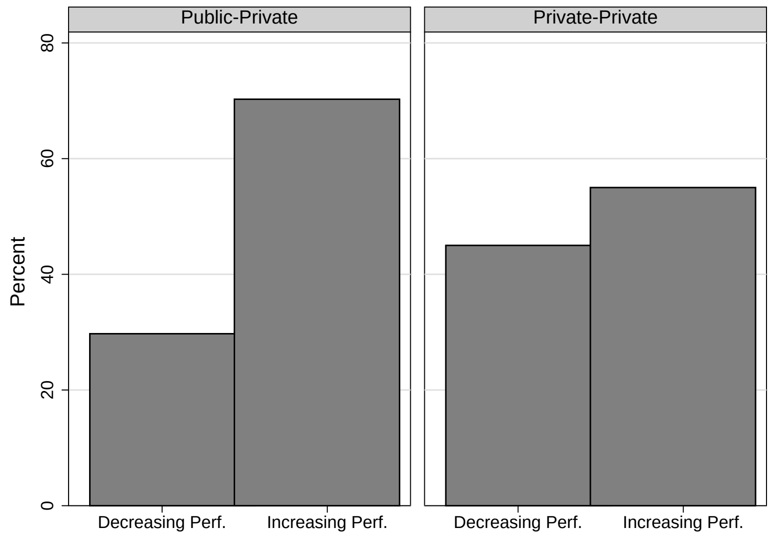


Figure 3.5.4. Percentage of subjects who increase or decrease their performance between Stage 1 and Stage 2, separately for Public-Private and Private-Private

Table 3.5.2 replicates Table 3.5.1 for Experiment B and reports OLS estimates. In Column (1), Stage 1 donation is regressed on a treatment dummy, which is 1 if subjects are in the Public-Private treatment and 0 if they are in the Private-Private treatment. In Experiment B, the coefficient is also positive, but not significant. In Column (2), Stage 2 donation is regressed on the same treatment dummy. As stated in Hypothesis 2, the coefficient is positive (subjects donate on average €0.10 more in the Public-Private treatment), but not significant. In Column (3), Stage 2 donation is regressed on the treatment dummy, additionally controlling for Stage 1 donation. The coefficient of the dummy variable stays almost the same compared to Column (2). However, the coefficient is still not significant. The coefficient of Stage 1 donation is positive and highly significant which illustrates again that Stage 1 and Stage 2 donation are strongly correlated. In Column (4), Stage 2 donation is regressed on the treatment dummy, Stage 1 donation, and the product of Stage 1 donation and the treatment dummy. Again, the coefficients of the treatment dummy and the interaction term are not significant.

Since the correlation of performance and generated donations is only piecewise, Table 3.5.3 reports regressions in the domain of performance. Again, there are no significant treatment effects and none of the hypotheses can be supported. Finally, we do not find a treatment difference in happiness.

Table 3.5.2. Regression Results: Experiment B, donations

	(1) Donation 1	(2) Donation 2	(3) Donation 2	(4) Donation 2
Public	0.0199	0.102	0.0954	-0.237
	(0.0382)	(0.0682)	(0.0688)	(0.4081)
Donation 1			0.3175***	0.3042***
			(0.0645)	(0.069)
Public x Donation 1				0.1569
				(0.1993)
Constant	2.100***	2.030***	1.364***	1.392***
	(0.0363)	(0.0677)	(0.0739)	(0.0792)
Observations	77	77	77	77
R^2	0.003	0.027	0.057	0.058

Coefficients in all columns are OLS estimates. Robust standard errors are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Even in Stage 1, being observed does not have a significant effect on donation behavior, which we find surprising since we closely follow the design of Ariely, Bracha, and Meier (2009) and thus cannot replicate their findings. In contrast to their study, subjects in Stage 1 of our Private-Private treatment actually achieve a higher performance. Both treatment groups accomplish numbers that are similar to those in the public condition of Ariely, Bracha, and Meier. Furthermore, we have enough statistical power; a treatment difference in performance similar to the one of Ariely, Bracha, and Meier (on average 822 clicks in the public condition and 548 clicks in the private condition) would be significant to a significance level of 1% with our sample size. We use the same mechanism to implement social observability, as well as the same piece rates, even though the cutoffs are different as we decrease the piece rate in steps of 100 instead of 200. The increased concavity could potentially decrease the treatment difference in donations and therefore explain why we do not find the same results. However, we observe subjects to continue the task even if one

Table 3.5.3. Regression Results: Experiment B, performances

	(1) Performance 1	(2) Performance 2	(3) Performance 2	(4) Performance 2
Public	-39.32	21.44	52.06	124.48
	(51.45)	(58.85)	(42.87)	(109.82)
Perf. 1			0.779***	0.814***
			(0.0651)	(0.0831)
Public x Perf. 1				-0.0849
				(0.1333)
Constant	876.5***	858.1***	175.5***	144.9**
	(38.15)	(48.65)	(56.23)	(64.98)
Observations	77	77	77	77
R^2	0.008	0.002	0.455	0.457

Coefficients in all columns are OLS estimates. Robust standard errors are in parentheses.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

click is worth only 0.01 Cent.[17] To summarize, despite closely following the design of Ariely, Bracha, and Meier (2009), we are not able to detect any direct effect of public observability on prosocial activities.

3.6 Conclusion

The aim of this paper is to investigate spillover effects of observability on later unobserved prosocial behavior, thereby studying the concept and prevalence of altruistic capital formation. We hypothesize that being observed during a good deed has a positive effect on subsequent prosocial behavior because people build up altruistic capital. People feel obliged to maintain their positive social and self-image, even in situations in which their actions are not observed by others, and keep on behaving prosocially. We do not find such behavior, independent of the concrete nature of the prosocial act, either requiring a donation of money or investing effort.

This lack of supporting evidence might be due to the reason that social image as a trigger of stronger prosocial behavior cannot be established in our experiments. We do find only a weakly significant positive effect in Experiment A and an insignificant effect in Experiment B. This result is not driven by a lack of prosocial behavior of subjects in the Public-Private treatment but if anything by a substantial prosocial attitude of the control group that does not face any social exposure in the first

17. We do not believe that subjects are not capable of pressing more pairs in five minutes, as Ariely, Bracha, and Meier (2009) themselves have a control condition in which subjects work for high monetary incentives and press, on average, 1290 combinations, which is the maximum level we observe.

place. People are willing to (repeatedly) sacrifice own resources for social welfare, regardless of observability. This suggests other potential drivers of repeated prosocial activity: It is possible that people already have a high altruistic capital stock and a prosocial self-perception and thus do not react to further motivation. It could therefore be interesting to run a similar study with a subject pool that has not developed high prosocial preferences (e.g. children or people who show very low altruistic preferences).

To summarize, people show substantial willingness to donate for a charitable reason. Surprisingly, image effects do not seem to make a large difference in our chosen setups, neither in the observed stages nor in the following non-observed stages. Therefore, it might be necessary to adjust the design. It seems that people are willing to spend time and effort to generate donations and keep on working even if they are not observed and even if the piece rate is extremely low. One potential solution is to make donating more costly in all stages, for example by not using real-effort tasks but dictator games instead which actually lower participants' income or by making the real-effort task more difficult and thus more costly.

Given that the effect that we hypothesized can be found in another study, it would be interesting to disentangle different channels. We elaborate in this chapter that we imagine observability to have an effect on subsequent prosocial choices via two channels. First, subsequent prosociality should increase because observability increases initial prosociality and this builds up altruistic capital. And second, because being observed fosters the altruistic capital formation directly. Disentangling these effects would be difficult but could be done, for example, by running an experiment in which, in one treatment, initial prosociality is increased via observability and, in another treatment, via a different incentive.

A consequence of no observation effects is that making conjectures on basic altruistic capital formation is more difficult. In order to test how early prosocial behavior influences subsequent prosocial behavior, one perhaps requires another exogenous manipulation of prosocial behavior, for instance a direct manipulation (such as a manipulation of the prosociality of a task's consequences) or using social reference points. The influence of the behavior of a reference person has been shown in various studies (e.g. Falk and Ichino, 2006; Mas and Moretti, 2009; Schwerter, 2016). In the area of altruistic and reciprocal behavior, information about others' donations increases own contributions to a public good (Shang and Croson, 2009), and subjects match effort provision to associates' levels (Gächter, Nosenzo, and Sefton, 2012; Gächter, Nosenzo, and Sefton, 2013; Thöni and Gächter, 2015).

In our design, subjects make decisions only within one session, which we see as a conservative test of altruistic capital formation. Nonetheless, it might be the case that the accumulation of altruistic capital requires a longer time horizon with repeatedly triggered prosocial acts to form a habit of acting prosocially or to change people's self-image. Hence, another potential extension would be to conduct a new experiment in which multiple stages take place in different weeks over a longer time horizon.

Appendix 3.A Instructions

These are the original instructions translated into English for both experiments. Instructions in italic were only for subjects in the Public-Private treatment.

3.A.1 Experiment A

Verbal Instructions to all Participants (only Public-Private)

Welcome.

Before we start, I would like to point out that your decisions in this study might not be completely anonymous. If you do not agree to these terms, you now have the possibility to end your participation in this study.

I am now kindly asking you to introduce yourselves with your first name and subject of study or occupation.

In this study, one of you has the role of an observer. All others are decision makers. All decision makers will make a decision at the computer today. The observer is also seated at a computer. This computer shows the computer screens of the decision makers. The only task of the observer is to observe the decisions made by the decision makers. At the end of the experiment, all decision makers furthermore tell the observer personally about the decisions they made.

Everyone now please draws a card. The one of you with the red card takes the role of the observer. This person please stays seated.

All others, with the black cards, please follow me to the adjoining room.

Verbal Instructions

You will receive €5 for your participation. The money is inside this envelope. Please do not open it yet, but only after the screen reads that the experiment is finished. Please sign here that you received the money. You can now go upstairs. Everyone of you may choose an office room with the numbers 1, 2 or 3. Please close the office door behind you and take a seat at your working space. You can start immediately.

Instructions Observer (only to observers in Public-Private)

Welcome to this experiment!

You will be taking the role of the observer. Your task will be the observation of all participants of the study and their choices.

For your participation in this study you will receive €5 in cash at the end.

On your screen you can see the screens of three other participants in real time. This means that you will be able to see the participants' choices throughout the course of the study. The other participants have the possibility to generate a donation for SOS-Kinderdörfer by solving tasks. At any time the participants may choose to stop solving tasks. After they have stopped working on the tasks, the participants will come to you and inform you about their office number as well as the amount of their generated donation. For this, please leave your cubicle so that the participant cannot see your screen. Please act neutral and do not speak. After all three participants have come to you, your part in the experiment is finished and you will receive your payoff. If you have any questions, please only address them to us: raise your hand and we will come to you. The violation of this rule will lead to you being excluded from this study and all its payments.

Screen 1 – Welcome and General Information

Today you have the possibility to generate a donation for the project **Fight against Hunger** by **SOS-Kinderdörfer**.

Worldwide, nearly one billion people do not have enough food. Especially children and families are suffering from this and are therefore specifically being supported by SOS-Kinderdörfer. SOS-Kinderdörfer provides a perspective to children in need and enables parents step by step to self-help: with food, seeds and through education. With their SOS Family Help they are fighting against poverty and hunger around the globe, e.g. in Bangladesh, Niger and Nicaragua.

You can generate the donation by solving as many tasks as you like out of a maximum of 25 tasks. These tasks will be explained in detail on the following page. *Your observer can see your screen. Right now, your observer can see these instructions and will also see how many tasks you solve and therefore how much you donate.* As soon as you decide not to solve any more tasks, this experiment is over. *Before you may leave, you have to let the observer know if and how much you are donating.*

You will find more details concerning the amount of the donation and the explanation of the task on the following page. After the study is concluded, we will send you a copy of the confirmation of all donations.

Screen 2 – Explanation Task

Your task is to count all zeros in a table consisting of zeros and ones.

For every correctly entered total number of zeros in a table, we donate a certain amount to the aforementioned project of SOS-Kinderdörfer. This amount varies with the number of the tables as follows:

Tables 1–5:	30 Cent
Tables 6–10:	20 Cent
Tables 11–15:	5 Cent
Tables 16–20:	2 Cent
Tables 21–25:	1 Cent

For every wrong input, 5 Cent will be subtracted from your donation, whereas the total amount cannot become negative.

You can stop the task **at any time** by clicking the button “Stop task” in the bottom right corner of the screen.

In total, you can solve up to 25 tasks. For this, you have a maximum of 20 minutes.

Your decision concerning your amount of work and the generated donation will be controlled by your observer. Additionally, after finishing the tasks, you will inform your observer in person about the total amount of your donation.

Click “Continue” to start with the tasks.

Screen 3 – Task

So far you have generated a total donation of X. For correctly solving this task your donation increases by **Y Cent**.

For a wrong answer, 5 Cent will be subtracted.

Please count all **zeros** in the following table.

Screen 4 – Results

You solved X tasks correctly and thus generated a donation of €Y. We will transact this amount for you to the project **Fight against Hunger** of SOS-Kinderdörfer.

Please go now downstairs into the laboratory to your observer and inform him or her about your office number and your generated donation.

Click “Continue” *as soon as you are back at your desk.*

Screen 5 – End

The experiment is over.

Written Instructions in Envelope

For your participation in today's experiment, you received €5. These €5 are also inside this DIN A5 envelope. Before the experiment is finished, you have another possibility to donate.

You can donate any part of your €5 to the **Project Bolivia: Children in Poverty Districts** of **SOS-Kinderdörfer**. We will transact the donation for you.

In the slums of La Paz, children and their families are living in poverty. Crime, alcoholism and hopelessness are omnipresent. People do not have another possibility than taking on irregular, precarious and merely profitable jobs. One of the main reasons for the low income per household is the low level of education of many parents: Only about 15 percent hold any degree. Most mothers and fathers therefore do not have any perspective – and the same fate threatens their children. SOS-Kinderdörfer helps by supporting the parents with 30 SOS day nurseries, educational projects, microcredits and psychological help.

You can donate any amount in between €0 to €5 in 10 Cent steps.

Your decision is completely anonymous.

Please take the money and the small white envelope out of the DIN A5 envelope and place the amount you want to donate inside the white envelope. Following this, close the small envelope and leave it at your desk.

Afterwards, the experiment is over and you can leave the laboratory.

3.A.2 Experiment B

Screen 1 – Welcome

Please wait until all participants are seated.

Screen 2 – Mood

First of all, we would like to know about your current mood.

For this purpose, please indicate your answer on a scale of 0 to 10.

0 indicates that your current mood is really bad. 10 indicates that your current mood is really good.

You can choose any integer in between 0 to 10 to express your mood.

How is your current mood?

Screen 3 – Explanation Task

Next, in the main part of the study, you will work on a task. In this task, you will have to **press the X-button and Y-button alternately.**

You now have the possibility to get to know the task by testing it for 60 seconds. By doing so, please try to press as many correct combinations as possible.

Please pay attention to pressing the buttons alternately. Otherwise it might happen that the combinations are not being counted.

Do you have any questions? If yes, please raise your hand. If this is not the case, please click "Continue" in order to test the task.

Screen 4 – Practice Task

Please press the X-button and the Y-button alternately.

So far, you have pressed X correct combinations.

Screen 5 – Result

The test phase is now over.

Within 60 seconds, you entered X correct key combinations. If you have any questions concerning the task, please raise your hand. If you do not have any questions, please click "Continue".

Screen 6 – Information Donation

For your participation in this study, you receive €6. This will be paid to you in cash at the end of the study.

In addition, you have the possibility to generate a donation for the project "Fight against Hunger" by SOS-Kinderdörfer.

Worldwide, nearly one billion people do not have enough food. Especially children and families are suffering from this and are therefore specifically being supported by SOS-Kinderdörfer. SOS-Kinderdörfer provides a perspective to children in need and enables parents step by step to self-help: with food, seeds and through education. Through their SOS Family Help they are fighting against poverty and hunger around the globe, e.g. in Bangladesh, Niger and Nicaragua.

You can generate the donation by working on the task you already got to know.

Important: Your donation is completely anonymous.

Important: At the end of the experiment we will ask you to step out of your cubicle and inform all other participants about your name and your total generated donation.

While doing so, your generated donation will be indicated on your screen, so that all other participants can read it as well.

You can find further details concerning the amount of donation on the next page.

After the end of the study, we will make a confirmation of the donation accessible to you.

Screen 7 – Information Piece Rate

For every correctly entered combination, we will donate a certain amount to the project described on the previous page. This amount per combination varies with the number of already entered combinations.

You can find the exact values from the following table:

Number of combinations	Donation per combination
1–100	1 Cent
101–200	0.5 Cent
201–350	0.25 Cent
351–500	0.1 Cent
501–700	0.05 Cent
ab 701	0.01 Cent

This means that we donate 1 Cent to the project for the first 100 combinations. For the next 100 combinations the donation amounts to 0.5 Cent etc.

If you pressed for example 160 correct combinations we would donate 100×1 Cent $+60 \times 0.5$ Cent $= 130$ Cent to the project.

To start working on the task, please click “Continue”.

Screen 8 – Task

Please press the X-button and the Y-button alternately. So far you have pressed X correct combinations and therefore generated a donation of €Y.

Screen 9 – Result

You entered X correct key combinations and thus generated a total donation of €Y.

We will transact this amount for you to the project “Fight against Hunger” of SOS-Kinderdörfer.

Please wait until all participants are finished with the task.

Screen 10 – Confirmation of Donation

As announced, you have the possibility to check the confirmation of donation for the total amount of donations generated over the course of this study. For this, we will upload a confirmation on the following website within the next couple of days, after transacting the donations:

LINK

You can now copy the website's address or take a picture of it.

Screen 11 – Information Donation

You now once again have the possibility to generate a donation. This time, we will transact the money for you to the project "Bolivia: Children in Poverty Districts" by SOS-Kinderdörfer.

In the slums of La Paz, children and their families are living in poverty. Crime, alcoholism and hopelessness are omnipresent. People do not have another possibility than taking on irregular, precarious and merely profitable jobs. One of the main reasons for the low income per household is the low level of education of many parents: Only about 15 percent hold any degree. Most mothers and fathers therefore do not have any perspective – and the same fate threatens their children. SOS-Kinderdörfer helps by supporting the parents with 30 SOS day nurseries, educational projects, microcredits and psychological help.

Important: Your donation is completely anonymous.

Important: In contrast to the first donation you will not have to inform anyone about your donation. Your second donation is completely anonymous. However, you will have to announce your first donation to all other participants of the experiment hereafter!

You can generate the donation by working on the same task as before. Also, the amount of donation per combination does not change and will be shown to you once again on the next page.

After the end of the study, we will make a confirmation of the donation accessible to you. This confirmation will also be provided on the website to which you have already received the link.

Screen 12 – Reminder Piece Rate

Here you can once again see the amount of the donation for every entered combination:

Number of combinations	Donation per combination
1–100	1 Cent
101–200	0.5 Cent
201–350	0.25 Cent
351–500	0.1 Cent
501–700	0.05 Cent
ab 701	0.01 Cent

To start working on the task, please click “Continue”.

Screen 13 – Task

Please press the X-button and Y-button alternately. So far you have pressed X correct combinations and therefore generated a donation of €Y.

Screen 14 – Result

You entered X correct key combinations and thus generated a donation of €Y.

We will transact this amount for you to the project “Bolivia: Children in Poverty Districts” of SOS-Kinderdörfer.

Please wait until all participants are finished with the task.

Screen 15 – Mood

We now once again would like to know about your current mood.

For this purpose, please indicate your answer on a scale of 0 to 10.

0 indicates that your current mood is really bad. 10 indicates that your current mood is really good.

You can choose any integer in between 0 to 10 to express your mood.

How is your current mood?

Screen 16 – Announcement (Private-Private)

In the following, some of the other participants (you are not part of them) will stand up and announce their names as well as their generated donation of part 1. These participants already knew about this while making their decision.

We ask you to stay seated inside your cubicle during this time, and to open your curtain when we ask you to do so.

After this part is finished, we will start with the payment. Please stay seated inside your cubicle until we call your cubicle number.

Screen 16 – Announcement (Public-Private)

As announced, before this study is over, you will now inform the other participants on your generated donation in part 1. For this, we will ask you to stand up and stand in front of your cubicle, whereas the curtain is open.

Some of the other participants will also step out of their cubicle at the same time. You will then sequentially state your first names and your generated donations from part 1. We ask you to be silent until your turn and to say the following sentence when requested to do so:

My name is ___ and I generated a donation of ___ Euros in part 1.

As a reminder, you will see the amount of your donation in part 1 on the next page.

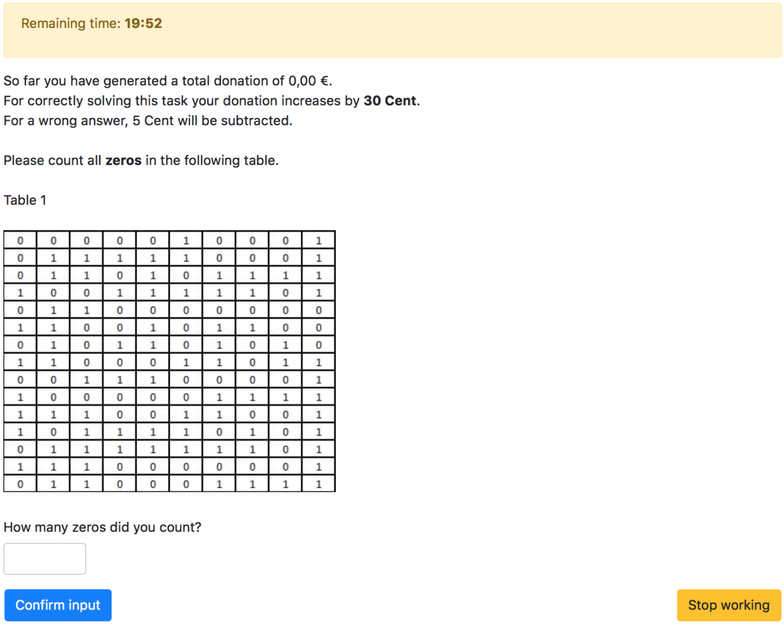
Please click "Continue", check the amount of your donation and step out of the cubicle when we ask you to do so.

Screen 17 – Announcement (Public-Private)

My name is ___ and I generated a donation of ___ Euros in part 1.

Appendix 3.B Screenshots

3.B.1 Experiment A



Remaining time: **19:52**

So far you have generated a total donation of 0,00 €.
For correctly solving this task your donation increases by **30 Cent**.
For a wrong answer, 5 Cent will be subtracted.

Please count all **zeros** in the following table.

Table 1

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

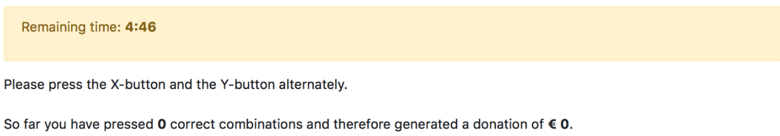
How many zeros did you count?

Confirm input

Stop working

Figure 3.B.1. Screenshot of real-effort task *Counting Zeros* for Experiment A

3.B.2 Experiment B



Remaining time: **4:46**

Please press the X-button and the Y-button alternately.

So far you have pressed **0** correct combinations and therefore generated a donation of **€ 0**.

Figure 3.B.2. Screenshot of real-effort task *Click for Charity* for Experiment B

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