Essays in Behavioral and Experimental Economics

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Introduction*

Behavioral economics has improved the understanding of economic phenomena by enriching the understanding of economic decision making with insights from psychology, sociology, and anthropology. Rigorous empirical investigations of individual behavior—which commonly involve the use of laboratory experiments—have been at the heart of behavioral economics and have lead to new theoretical accounts of decision making. The following three insights gave rise to influential branches of behavioral economics. First, the context in which individuals make decisions often unleash behavioral influences that go beyond those identified by standard economic theory. In particular, individual *preferences* commonly depend on contextual features, as has been highlighted by the literatures on reference-dependent preferences (Kahneman and Tversky, 1979), and default effects (Thaler and Sunstein, 2003). Second, individuals' perception and processing of information often does not live up to the high demands of standard economic theory. Instead, individuals seem to employ simple heuristics (Tversky and Kahneman, 1974) and attention-based decision rules (Kahneman, 2003) in complex environments. Third, while standard economic theory typically constrains individuals' motives to pure self-interest, a more comprehensive view on individuals' behavior in social interactions uncovers that individuals often care directly about the well-being of others as well as about how they are viewed and treated by others (Fehr and Falk, 2002).

   This thesis consists of three chapters that each contribute to one of these three building blocks of research in behavioral economics.

   In Chapter 2, I investigate the consequences of social reference points for decision making under risk in a series of laboratory experiments. In the main experiment, decision makers observe the predetermined earnings of peer subjects before making a risky choice. I exogenously manipulate peers' earnings and find a significant treatment effect: decision makers make riskier choices in case of

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larger peers' earnings. The treatment effect is consistent with the predictions of a model featuring social-comparison–based reference points and loss aversion. In two control experiments, I demonstrate that nonsocial—e.g., expectations-based—reference points do not explain the treatment effect.

In Chapter 3, I present novel results on individuals' intertemporal choices in joint work with Holger Gerhardt and Louis Strang. Our findings cannot be explained by exponential and hyperbolic discounting, the canonical approaches to intertemporal decision making in economics, but are consistent with an attention-based approach to intertemporal decision making that is based on concentration bias. In particular, we provide causal evidence from novel lab experiments that intertemporal choices are systematically affected by whether consequences of intertemporal choice are concentrated in few or dispersed over multiple periods: (i) Individuals are less patient in the case that the advantages of patient behavior are dispersed over many future periods than when they are concentrated in a single future period. (ii) Individuals are more patient in the case that the disadvantages of patient behavior are dispersed over multiple earlier periods than when they are concentrated in a single earlier period. Both findings demonstrate concentration bias in individuals' intertemporal choices. Our results are in line with the recent theoretical model of Kőszegi and Szeidl (2013). Despite the prevalence of dispersed payoffs and costs in everyday life, no empirical study so far has investigated whether spreading payments over time causally impacts discounting. Our results suggest that previous studies may have neglected an important channel that influences intertemporal decisions.

In Chapter 4, I study in joint work with Florian Zimmermann whether prior experience of unfair versus fair treatment affects how much individuals trust others? We provide causal evidence that trust is affected by prior personal experience of fair versus unfair treatment by an unrelated third party. We compare the willingness to trust of subjects in a lab experiment after they experienced either being paid or not being paid for a real-effort task by a peer subject. After being paid, subjects' willingness to trust is substantially higher relative to subjects who were not paid previously. Importantly, this treatment effect holds despite the fact that subjects knew the exact frequency with which subjects overall got paid or did not get paid, such that the personal experience of fair versus unfair treatment did not provide additional information regarding the subsequent interaction. Rational learning hence cannot explain the treatment effect on trust. By employing a control experiment, we show that the effect of experiencing fair versus unfair treatment on trust does also not result from income effects: when subjects were paid based on a coin toss, subjects' willingness to trust was similar to subjects who where not paid based on a coin toss.

In summary, this thesis documents effects on individual behavior that are not predicted by standard economic theory, but underscore the relevance of behavioral economics for our understanding of economic decision making.

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2

Social Reference Points and Risk Taking*

2.1 Introduction

Since Kahneman and Tversky's (1979) seminal prospect theory, the powerful insights of reference-dependent preferences have enriched the toolbox of economists in understanding individual behavior. The fundamental insight behind reference dependence is that individuals evaluate their obtained outcomes relative to a reference point. Outcomes that are superior to the reference point are perceived as gains, and inferior outcomes as losses. The essential feature of reference-dependent preference models is loss aversion: Losses have a more pronounced negative effect on utility than equal-sized gains have a positive effect.

The key question in the literature on reference-dependent preferences is: what determines the reference point? Behavioral predictions of reference-dependent preference models are highly sensitive to the specification of the reference point, which constitute what individuals perceive as losses or gains. So far, reference points based on the status-quo and expected outcomes have been stud-

ied predominantly within the literature. This improved both the understanding of reference-dependent preferences and of many economic phenomena.[1]

In this paper, I contribute to this vibrant literature by studying social-comparisons–based reference points. Surprisingly little attention has been paid to such social reference points within the literature on reference-dependent preferences.[2] However, a rich tradition of research in psychology, sociology, anthropology, and the literature on social preferences in economics suggests that social outcomes are a reasonable source of the reference point. Not only do individuals frequently engage in social comparisons (Festinger, 1954), but also individual well-being often depends on social comparisons (e.g., Veblen, 1899; R. H. Frank, 1985; Fliessbach et al., 2007; Card et al., 2012). Moreover, the perception of unfair treatment commonly arises from comparisons to what others have (e.g., Adams, 1963; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Falk and Fischbacher, 2006).

The primary research questions of this study are (i) whether social reference points influence individual behavior when individuals' decisions affect only their own outcomes and (ii) whether loss aversion around social reference points explains potential behavioral effects. By answering these questions, I contribute to the literature on social preferences. In form of inequity aversion, social–reference-point effects were studied in distributional games in which individuals were directly responsible for their peers' earnings (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). It remains unclear whether these results can be generalized to decision making when individuals affect only their own outcomes. Distributional games constitute decision-making contexts where reciprocity (e.g., Levine, 1998; Falk and Fischbacher, 2006), welfare or efficiency concerns (e.g., Charness and Rabin, 2002; Engelmann and Strobel, 2004), and prosocial image concerns (e.g., Bénabou and Tirole, 2006; Ariely et al., 2009) may obfuscate social reference point effects on behavior.[3] Importantly, reci-

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[1] These include, for instance, risk taking (e.g., Kahneman and Tversky, 1979; Rabin, 2000; Kőszegi and Rabin, 2007; Pope and Schweitzer, 2011; Sprenger, 2015), the premium equity puzzle (e.g., Benartzi and Thaler, 1995; Gneezy and Potters, 1997; Kőszegi and Rabin, 2009), the disposition effect (e.g., Shefrin and Statman, 1985; Odean, 1998; Genesove and Mayer, 2001), the endowment effect (e.g., Kahneman et al., 1990; Ericson and Fuster, 2011; Heffetz and List, 2014), labor supply (e.g., Camerer et al., 1997; Kőszegi and Rabin, 2006; Crawford and Meng, 2011), effort provision (e.g., Mas, 2006; Fehr and Goette, 2007; Abeler et al., 2011; Gill and Prowse, 2012; Gneezy et al., 2013), price competition (Heidhues and Kőszegi, 2008), contracting in principal–agent settings (Herweg et al., 2010), soccer referees (Bartling et al., 2015), job search (DellaVigna et al., 2015), tax sheltering (Rees-Jones, 2014), and marathon running (Allen et al., 2014).

[2] A notable exception is Linde and Sonnemans (2012), who tested whether prospect theory's reflection effect extends to social reference points and found no evidence for it. In contrast to Linde and Sonnemans (2012), I study social–reference-point effects based on loss aversion.

[3] For instance, giving in the dictator game and rejecting offers in the ultimatum game may result from an aversion against unequal outcomes. However, the former is also consistent with prosocial image concerns and the latter with reciprocating perceived unfair treatment.

procity, efficiency concerns, and prosocial image concerns do not predict social–reference-point effects on behavior when individuals affect only their own outcomes. By contrast, loss aversion around social reference points predicts behavioral effects.

I focus on decision making under risk to study social reference point effects. Risk taking is an important dimension of economic decision making (Dohmen et al., 2011). Understanding the determinants of risk taking is a fundamental interest of economic research. Additionally, the study of reference-dependent preferences in economics emerged around empirical investigations of risk taking.

This paper employs a novel laboratory experiment that allows to provide causal evidence on whether social reference point affect risk taking and tests loss aversion around social reference points. In doing so, I address four critical challenges. First, I induce social reference points to individuals who make a risky decision. Second, I exogenously vary the level of social reference points between two treatments, HI and LO. Third, decision-making subjects are able to avoid earning less than their peer by making different risky choices between HI and LO treatments. Fourth, I employ two control experiments that allow to test alternative explanations based on nonsocial—e.g., expectations–based—reference points.

In each session of the main experiment, a single decision making subject observed the predetermined earnings, s, of a single peer subject before making a risky choice. The risky choice allowed the decision maker to choose a binary lottery from a set of lotteries. Essentially, decision makers chose an upside payment between €3 and €16.5. The larger the decision maker chose this upside to be, the lower was the likelihood of receiving it. The downside of each lottery was no payment. Subjects could choose riskier lotteries—combining larger upsides with lower likelihoods of receiving them—or less risky lotteries—combining lower upsides with higher upside likelihoods. Ultimately, this choice involved a trade-off between the size of the upside and its likelihood.

In a between-subject design, I varied the predetermined earnings of the peer between $s_{\mathrm{HI}} = €8$ (HI treatment) and $s_{\mathrm{LO}} = €2$ (LO). Since there were only 2 subjects—one decision maker and one peer—present in the lab per experimental session, peers' earnings served as a natural comparison standard. In that sense the design allowed me to induce relevant social reference points to the risk-taking subjects. Additionally, decision makers knew that their outcomes and risky choices would never be revealed to their peers.

To derive predictions, I formalize the impact of peer earnings on risk taking in a simple model featuring social-comparisons–based reference points and loss aversion. In the case that individual behavior follows expected utility theory, no treatment effect on risk taking is expected, since decision makers face the same risky choice across treatments. In the case that individuals evaluate

lottery outcomes according to loss aversion around their peers' earnings, the treatment manipulation changes their risk-taking incentives: decision makers choose larger upsides, i.e., riskier lotteries, in HI than in LO to avoid earning less than their peer.

This is precisely what happened in the experiment: Decision makers chose an average upside of €8.25 when their peers' earnings were €8; and they chose an average upside of €7 when their peers' earnings were €2. This treatment effect on risk taking is statistically significant and provides affirmative evidence that social reference points affect risk taking.

However, an alternative explanation behind the treatment effect on risk taking may be loss aversion relative to expectations–based reference points — rather than social reference points. Before decision makers knew their risky choice, they only knew what they could have earned, if they had been assigned to the peer role. These counterfactual earnings informed their expectations regarding their own earnings, leading to potential differences in expectations–based reference points between HI-LO treatments before decision makers were informed about their risky choices.

Based on Kőszegi and Rabin (2006, 2007), expectations–based reference points can, but do not have to, account for the treatment effect on risk taking. Considering the timing of the experiment, the critical question for expectations–based reference points is whether decision makers change expectations–based reference points quickly in light of new information or slowly. If expectations–based reference points changed quickly after decision makers were introduced to their risky choices, then no treatment effect would be predicted. This follows from the fact that the risky choices are constant across the HI and the LO treatment. However, the treatment effect on risk taking is consistent with expectations–based reference points that do not (sufficiently) change after decision makers are introduced to their risky choice.

By comparing risk taking between the main experiment and two control experiments that are discussed in detail below, I investigate whether the treatment effect on risk taking is caused by social reference points (SRPs) or whether it could also result from slowly changing expectations–based reference points (ERPs). In the nonsocial control, SRPs predict no treatment effect on risk taking and ERPs make the same predictions as for the main experiment. The peer-lottery control experiment is designed to reverse these predictions. ERPs predict no treatment effect on risk taking, while SRP predict a similar treatment effect compared to the main experiment. Overall, the results are in favour of SRPs as the driver of risk taking in all experiments: There is no treatment effect on risk taking in the nonsocial control while there is a treatment effect on risk taking in the peer-lottery control. Figure 2.1 provides an overview of how the control

Main

Procedure: 2 subjects $\rightarrow$ coin toss

peer: —   receives s

decision maker: —   observes $\rightarrow$ receives risky choice $\rightarrow$ performs risky choice

Treatments: $s = €8$ in HI and $s = €2$ in LO

Conducted: in fall 2012, spring 2013, and fall 2014

Nonsocial

Procedure: 1 subject $\rightarrow$ coin toss

passive: —   receives r

active: —   observes $\rightarrow$ receives risky choice $\rightarrow$ performs risky choice

Treatments: $r = €8$ in HI and $r = €2$ in LO

Conducted: in spring 2013, summer 2014, and fall 2014

Peer-Lottery

Procedure: 2 subjects $\rightarrow$ coin toss

peer: —   lottery: €8 or €2 $\rightarrow$ delay of 5min $\rightarrow$ receives s

decision maker: —   observes $\rightarrow$ receives risky choice $\rightarrow$ performs risky choice

Treatments: $s = €8$ in HI and $s = €2$ in LO

Conducted: in summer 2014

Figure 2.1. Overview of all Risk-Taking Experiments

experiments relate to the main experiment, and Figure 2.2 presents the main results.

In the nonsocial control, I kept counterfactual earnings that inform expectations in place, while removing incentives based on SRPs. The critical modification relative to the main experiment was that only one subject participated in each session. A coin toss assigned subjects to an active or a passive role. Passive subjects received the same earnings as peer subjects did in the main experiment. Active subjects were given the same risky choice as decision makers in the main experiment. In a between-subject design, active subjects learned their counterfactual earnings ($r_{\mathrm{HI}} = s_{\mathrm{HI}}$ in HI and in $r_{\mathrm{LO}} = s_{\mathrm{LO}}$ in LO) before they received any information on their risky choice. Based on these counterfactual earnings, active subjects are equipped with potentially different ERPs between HI-LO treatments—like in the main experiment.

I find no treatment effect on risk taking in the nonsocial control. While the lottery upsides were slightly larger in HI (€7.65) than in LO (€7.62), this dif-
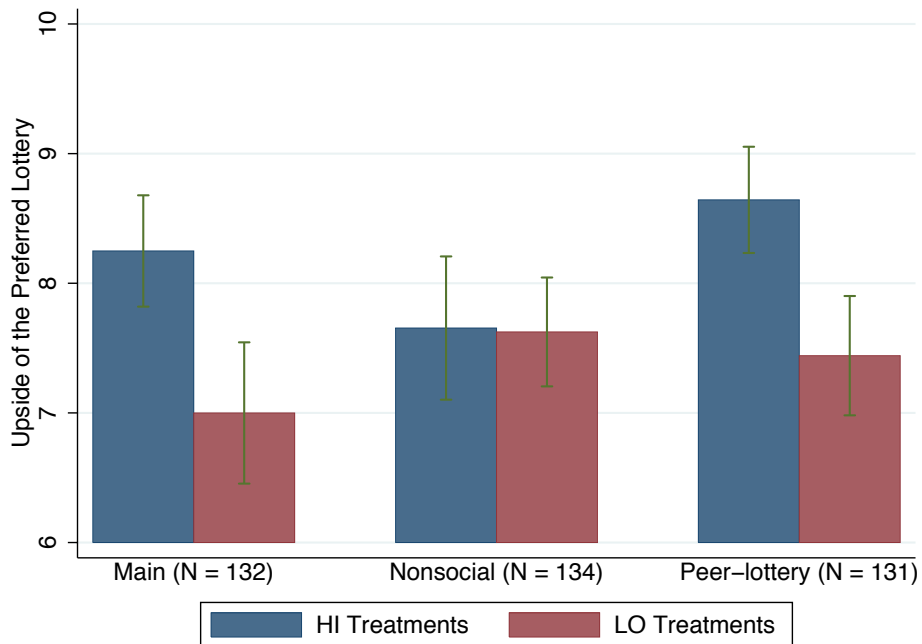
Figure 2.2. Average Risk Taking per Experiment

Note: The bars depict the average chosen upsides per experiment and treatment.

ference is not statistically significant and significantly smaller than in the main experiment. This provides evidence that the treatment effect of the main experiment is not driven by differences in ERPs that are based on counterfactual earnings. However, these results do not provide evidence against ERPs per se. The observed risk taking in the nonsocial control is consistent with subjects quickly changing reference points upon being introduced to their risky choices.[4]

In the peer-lottery control experiment, the main experiment is repeated with a critical innovation: Peers were endowed with a 50-50 lottery between €2 and €8. Decision makers learned about the lottery endowment of their peers in the beginning of the experiment. This permits introducing both €2 and €8 as counterfactual earnings. After a delay of 5 minutes, decision makers observed a coin flip that determined their peers' lottery outcomes. Decision makers observed their peers receiving €8 in HI and €2 in LO. Thereafter, decision makers were

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[4] Song (2012) shows that expectations-based reference points can change slowly as well. These different results seem to be driven by Song's (2012) emphasis on disappointment over not receiving counterfactual earnings. In his experimental setup, subjects expected a large lottery outcome before arriving at the lab. When subjects ended up not receiving the large lottery outcome, they increased their risk taking subsequently. In my experiments, decision makers' counterfactual earnings were revealed when they already knew that they would not receive them.

introduced to their risky choice and chose their preferred lottery. If counterfactual earnings affect slowly changing ERPs, ERPs do not differ between the HI and the LO treatment. Therefore, ERPs do not predict a treatment effect on risk taking in the peer-lottery control.

Decision makers chose larger average upsides in HI (€8.64) than in LO (€7.44) in the peer-lottery control. This treatment effect is statistically significant and significantly larger than in the nonsocial control. The peer-lottery control hence provides further evidence that ERP do not explain the treatment effect on risk taking in the main experiment.

This paper contributes to the literatures on reference-dependent preferences and social preferences by documenting SRP effects on risk taking. These treatment effects cannot be explained by nonsocial—e.g., expectations–based—reference points. The main treatment effect on risk taking is consistent with the qualitative prediction of the social-comparisons–based reference point model featuring loss aversion. I discuss shortcomings of the model regarding quantitative predictions that are not supported by the observed risk taking and present simple extensions to the model that account for them.

My findings also contribute to the literature on peer effects. Loss aversion around to social reference points formalizes relative concerns that are a prominent form of how peers affect individual decision making. In this paper, I show clean evidence for how relative concerns affect risk taking. In particular, I isolate relative concerns from ERP effects and from other forms of peer effects, such as imitation, social learning, and peer pressure (e.g., Sacerdote, 2001; Duflo and Saez, 2002; Falk and Ichino, 2006; Mas and Moretti, 2009). For instance, Kuhn et al. (2011) study car purchase decisions and find that (non-winning) close neighbors of winners in the Dutch Postcode Lottery are more likely to buy a car 6 months after lottery winners were announced. This may be driven by relative concerns or by imitation. Card et al. (2012) find that job satisfaction and job search intentions are higher for University of California employees that were made aware of the fact that they earned below the median. While this may reflect SRP effects, it is also in line with ERPs. Bursztyn et al. (2014) show how individuals' investment into a risky asset in a field experiment is affected by their knowledge regarding their friends' intentions to invest and their actual investment into the same risky asset. Bursztyn et al. (2014), however, cannot isolate SRP effects as the driving force behind investment choices. Individuals may draw utility from holding the same risky asset as their friends (Taylor, 2011) or expect greater returns when learning from their friends.

The documented SRP effects also contribute to the literature on status concerns (see, e.g., Ball et al., 2001; Heffetz, 2011). Status concerns include relative concerns and public recognition or approval from peers (see, e.g., Heffetz and R. Frank, 2011). For instance, Huberman et al. (2004) show in a lab experi-

ment that subjects forwent material gains in order to win a contest that was tied to a public victory announcement. My findings complement these findings by showing that relative concerns matter independently of public recognition by peers.

I proceed in Section 2.2 with providing evidence for social reference point effects. I establish that these social reference point effects cannot be explained by expectations–based reference points in Section 2.3. Section 2.4 concludes.

2.2 Evidence for Social Reference Point Effects

This section provides evidence that social reference points affect individual risk taking. In the following I present the design of the main experiment and derive behavioral predictions from a social-comparisons–based reference point model. Then I report and discuss the findings of the experiment.

2.2.1 Main Experiment

The main experiment is designed to allow for a precise measurement of risk taking after decision makers have been made aware of the earnings of their peer subjects. Between two treatments, I exogenously manipulate the predetermined peer earnings. A between-subject comparison of risk taking across treatments allows identifying the effect of social reference points.

Two subjects participated in each lab session. Upon subjects' arrival, the experimenter tossed a coin in front of their eyes to assign them to one of two roles: decision maker and peer (called participant A and B in the experiment). Thereafter, subjects received role-specific instructions in private.[5] Peers learned that they would receive a show-up fee and an additional payment of €s for completing a survey. Thereafter, they completed the survey and left the lab once they were done. Decision makers, first, learned that they would receive a show-up fee and that they could earn an additional payment for completing the same survey their peers had to complete. Before receiving any information on their own additional payment, they learned the show-up fee and additional payment of their peers. Second, decision makers were told that their own additional payment was not predetermined, but the outcome of a risky choice. Third, they learned that their peers received no information on their behalf and would leave the lab earlier than they would. Fourth, they were introduced to their risky choice. Fifth, they performed their risky choice. Finally, decision makers completed the survey and left the lab once they were done—which was by design 5–10 minutes after their peers.

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[5] Appendix 4.A provides a translation of the instructions.
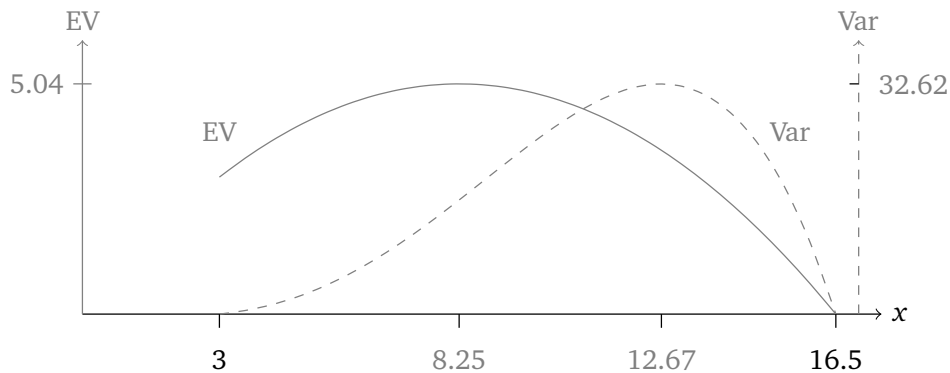
Figure 2.3. Properties of the Lotteries

Note: The solid graph shows how the expected value of all lotteries varies in the lotteries' upside, x. The dotted graph shows how the variance varies in x.

All decision makers faced the same risky choice which is based on Andreoni and Harbaugh (2010). Decision makers chose their preferred binary lottery $(x(q), q)$ from a set of lotteries. Each lottery paid an upside of $x(q) = $€16.5 − €13.5q$ with an upside likelihood of $q = i / 100$, for integers $i \in [0, 100]$, and nothing instead. Thus, the set of lotteries entailed, e.g., a certain payment of €3, (€3, 100%), an upside of €9.75 with an upside likelihood of 50%, (€9.75, 50%), and an upside of €16.23 with an upside likelihood of 2%, (€16.23, 2%). Decision makers chould choose riskier lotteries—that combined larger upsides with lower upside likelihoods—or less risky lotteries—that combined lower upsides with higher upside likelihoods. Figure 2.3 depicts what this relationship implies for the expected value and variance of these lotteries. Ultimately, decision makers faced a mean-variance trade-off for lotteries with an upside below €8.25.

I used a visual elicitation method that made it easy for subjects to understand the lottery choice. Appendix 2.B provides screenshots of the decision screens.

In the HI treatment, decision makers chose their preferred lottery after learning that their peer received $s_{\mathrm{HI}} = $€8$. The construction of the risky choice gave them a chance to earn more (or not to earn less) than their peers. In the LO treatment, decision makers' peers received $s_{\mathrm{LO}} = $€2$. The risky choice allowed decision makers to choose a lottery that combined a higher upside likelihood with a lower upside. This enabled them to avoid falling behind the peer.

The only variation between the two treatments was the level of the peer earnings. Hence, a difference in the decision makers' risk taking between the treatments allows identifying the impact of social reference points. Our design rules out the influence of other peer effects—e.g., imitation, learning, or social pressure: decision makers did not observe actions of any other subjects; deci-

sion makers did not affect peer outcomes; and decision makers knew that their choices and outcomes were not revealed to others.[6]

2.2.1.1 Private Risk Attitudes

Between one and two weeks after the main experiment, decision makers returned to the lab and received €8 as a show-up fee. In this second part, I elicited their private risk attitudes, i.e., their individual risk attitudes in the absence of any peer effects. I use this as a control variable in the analysis of the risk-taking behavior in the main experiment. Each decision maker faced 20 price-list–styled decisions. Each decision was a choice between Alternative Y, a certain amount of money, and Alternative X, a binary lottery. Alternative Y was always €3. Alternative X was a distinct lottery for each decision. Along the 20 decisions, Alternative X is getting more risky. Subjects started choosing Alternative X and switched to Alternative Y at some point. I interpret this switching point as a proxy of the decision makers' risk attitudes. I classify them as more (less) risk-averse, the earlier (later) they switched.[7] Table 2.1 lists all 20 decisions.

2.2.1.2 Procedure

The main experiment was conducted in three waves at two office rooms of the Bonn Graduate School of Economics in fall 2012 and spring 2013 and of the BonnEconLab in fall 2014. By using two rooms, both treatments, HI and LO, were conducted simultaneously. In total, 264 subjects—132 decision makers and 132 peers—participated in 132 sessions of the main experiment. No subject participated in more than one treatment (and in any other experiment conducted for this paper). I invited only male subjects to keep the sample homogenous. Each session lasted for 12 to 20 minutes. Subjects earned on average €8.5. The second part of the experiment was conducted at the BonnEconLab. All but 6 decision makers from the main experiment participated (attrition rate of 5%). Each session lasted for 10 to 40 minutes. Subjects earned on average €12.7. All experiments in this paper were computerized using the softwares z-Tree (Fischbacher, 2007) and ORSEE (Greiner, 2004).

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[6] In case pairs of decision maker and peer subjects knew each other, decision makers may have anticipated talking to their peers about the experiment afterwards. Therefore, all decision makers and peers were asked whether they had known each other prior to the experiment—which was true for 5 pairs. All results presented in this paper remain virtually unchanged when focusing only on pairs of strangers.

[7] This price-list elicitation method allows subjects to switch multiple times. For subjects who switched multiple times, the mean switching point is used to proxy their risk attitude.

Table 2.1. Choice List to Measure Private Risk Attitudes

Decision	Alternative X			Alternative Y
1	3.40 euros with 97%	or	0 euros with 3%	3 euros
2	4.08 euros with 92%	or	0 euros with 8%	3 euros
3	4.76 euros with 87%	or	0 euros with 13%	3 euros
4	5.43 euros with 82%	or	0 euros with 18%	3 euros
5	6.10 euros with 77%	or	0 euros with 23%	3 euros
6	6.78 euros with 72%	or	0 euros with 28%	3 euros
7	7.45 euros with 67%	or	0 euros with 33%	3 euros
8	8.13 euros with 62%	or	0 euros with 38%	3 euros
9	8.80 euros with 57%	or	0 euros with 43%	3 euros
10	9.48 euros with 52%	or	0 euros with 48%	3 euros
11	10.15 euros with 47%	or	0 euros with 53%	3 euros
12	10.83 euros with 42%	or	0 euros with 58%	3 euros
13	11.50 euros with 37%	or	0 euros with 63%	3 euros
14	12.18 euros with 32%	or	0 euros with 68%	3 euros
15	12.86 euros with 27%	or	0 euros with 73%	3 euros
16	13.53 euros with 22%	or	0 euros with 78%	3 euros
17	14.20 euros with 17%	or	0 euros with 83%	3 euros
18	14.88 euros with 12%	or	0 euros with 88%	3 euros
19	15.56 euros with 7%	or	0 euros with 93%	3 euros
20	16.23 euros with 2%	or	0 euros with 98%	3 euros

2.2.2 Predictions

This section examines how subjects are predicted to behave in the main experiment. I consider two cases: subjects do not or do care about their earnings relative to their peer. The risk-taking context can be summed up as follows: decision makers learned that their peer earned €s. Then, they chose a binary lottery from a set of lotteries $\{(x(q), q)\}$, with $x(q) = \overline{x} - rq$, $q = i/100$ for integers $i \in [0, 100]$, $\overline{x} = 16.5$, and $r = 13.5$. That is, subjects faced a trade-off between the seize of an upside and its likelihood. Choosing a lottery over another with a greater upside likelihood by one percentage point implies choosing a smaller upside by €$r/100$. I set $s = s_{\text{HI}}$ and $s = s_{\text{LO}}$ for the HI and LO treatment, respectively, such that

$$\frac{1}{2}\overline{x} > s_{\text{HI}} > \underline{x} = \overline{x} - r > s_{\text{LO}} > 0. \tag{2.1}$$

This implies that lotteries with a relatively low upside are above (below) the social reference point in the LO (HI) treatment. Additionally, the social reference point in the HI treatment is "reachable:" Decision makers do not need to accept extremely risky lotteries in oder to have a chance to catch up with their peer.

In the following, I discuss two approaches to deriving predictions for the risky choices of the decision makers. First, I consider the standard model of

risky decision making, expected utility. Second, I consider a social-comparisons–based reference point model that features loss aversion relative to peer earnings: decision makers have an aversion to earning less than their peers.

2.2.2.1 Expected Utility

The standard model of risky decision making assumes that individuals maximize their expected utility, $U(x, q) = qu(x)$, under the restriction of $x = \overline{x} - rq$. This decision problem is independent of s and, therefore, predicts no difference in risk taking between the treatments. In the case of linear utility in money, subjects across treatments maximize their expected payoff with

$$x^* = \overline{x}/2. \tag{2.2}$$

2.2.2.2 Social Reference Points

Second, based on the evidence that relative concerns affect the subjective well-being of individuals Clark et al. (see 2008, for an overview), I designed and conducted the main experiment under the hypothesis that social reference points affect their risk taking. In the following, I examine a simple social-comparisons–based reference point model to guide this hypothesis. The utility function of the model uses piecewise, ex post comparisons between potential outcomes and the social reference point, following Fehr and Schmidt (1999), Bolton and Ockenfels (2000), Charness and Rabin (2002), and Falk and Fischbacher (2006). I weight the ex post comparisons between outcomes and reference points by the likelihood of their occurrence, following Kőszegi and Rabin (2006, 2007). The model abstracts from other forms of reference points, since the treatment manipulation involves only social reference points.[8] Like in models of reference-dependent preferences, I assume that losses loom larger than gains and evaluations are convex in losses and concave in gains.

Decision makers are modeled as evaluating a lottery by considering both the "consumption utility" they derive from the lottery and the "social comparison utility" relative to their peer's earnings. The expected consumption utility is the expected utility of the lottery, i.e., $qu(x)$. Assuming that utility is approximately linear in x, the expected consumption utility reduces to the expected outcome of the lottery, i.e., qx. The social comparison utility, $\mu(\cdot)$, captures the two ex post earnings comparisons, $x - s$ and $0 - s$. For small arguments z, it is assumed that $\mu(z)$ is piecewise linear: $\mu(z) = \eta z$ for $z \geq 0$ and $\mu(z) = \eta \lambda z$ for $z < 0$. The parameter $\eta \geq 0$ captures the relevance of the social comparison utility for overall utility. With $\eta = 0$, the expected utility reduces to the standard model that was discussed above. In the following, I focus on the case when

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[8] Section 2.3 discusses a potential connection between social and nonsocial reference points.

social comparison utility is relevant for decisions, i.e., $\eta > 0$. The parameter λ captures how individuals evaluate having less than others. For $\lambda > 1$, individuals are loss-averse.

The expected utility of choosing a lottery with $x > s$ is

$$U(x, q(x)|s) \ = \ q(x)x + q(x)\eta(x - s) + (1 - q(x))\eta\lambda(0 - s). \quad (2.3)$$

The first term on the right-hand side is the expected consumption utility of the lottery. The second and third terms are, respectively, the expected social gain and social loss.

For lotteries with $x < s$, the expected social comparison utility collapses to losses only,

$$U(x, q(x)|s) \ = \ q(x)x + q(x)\eta\lambda(x - s) - (1 - q(x))\eta\lambda s. \quad (2.4)$$

Consider first the LO treatment. Because decision makers can only choose lotteries with an upside above their peer's earnings, i.e., $\underline{x} > s_{\mathrm{LO}}$, they maximize their expected utility of equation (2.3) under the restriction of $x = \overline{x} - rq$, yielding

$$\frac{\partial U(x|s_{\mathrm{LO}})}{\partial x} \ \overset{!}{=} \ 0 \ \iff \ x^*_{\mathrm{LO}} \ = \ x^* + \psi^* s_{\mathrm{LO}},$$

$$\text{with } \psi^* \ = \ \frac{\eta(1 - \lambda)}{2(1 + \eta)} \ < \ 0.$$

Compared to the standard model of risky choice (with $\eta = 0$), loss aversion induces decision makers to choose a less risky lottery, i.e., a lower upside. By decreasing x, decision makers increase their chances of "securing" an outcome above s_{LO}.

In the HI treatment, decision makers can choose lotteries with upsides above and below their peer's earnings, since $\overline{x} > s_{\mathrm{HI}} > \underline{x}$. Their marginal utility of taking risk is:

$$\frac{\partial U(x|s_{\mathrm{HI}})}{\partial x} \begin{cases} < \ 0 & \text{if } x \ > \ s_{\mathrm{HI}} \\ > \ 0 & \text{if } x \ < \ s_{\mathrm{HI}}. \end{cases} \quad (2.5)$$

First, assume decision makers contemplate choosing an upside that exceeds the earnings of their peer, $x > s_{\mathrm{HI}}$. Equation (2.5) states that the marginal utility of taking risk is negative: for any value of x above s_{HI}, decision makers prefer to reduce their risk taking—choose a smaller x—in order to avoid earnings less than the peer up to the point that $x = s_{\mathrm{HI}}$. In the case that decision makers consider a lottery with $x < s_{\mathrm{HI}}$, the marginal utility of taking risk is positive, equation (2.5). This reflects the following: if decision makers choose a lottery that leaves them in an unfavorable relative position, they revert to choosing the

lottery with the maximum expected value. However, the lottery with maximum expected value has an upside larger than the earnings of his peer. Therefore, loss-averse decision makers settle at setting $x_{\mathrm{HI}}^* = s_{\mathrm{HI}}$. They modify their risky behavior to match ex post earnings between their peer and themselves for the case of receiving the lottery's upside.

The model predicts that sufficiently loss-averse decision makers choose riskier lotteries in the HI than in the LO treatment, i.e., $x_{\mathrm{LO}}^* < x_{\mathrm{HI}}^*$.[9] In the LO treatment, loss aversion induces decision makers to reduce their risk taking in order to secure their favorable relative earnings and avoid falling behind their peer from too much risk taking. In the HI treatment, decision makers choose riskier lotteries to be able to "catch up" with their peer by matching their upside with their peer's earnings. Based on this argument, my main qualitative hypothesis is:

Hypothesis 1. *Loss-averse decision makers choose lotteries with larger upsides in HI than in LO, i.e., $x_{LO} < x_{HI}$.*

Based on the discussing of how loss-averse subjects in the HI treatment behave, I also make a quantitative prediction:

Hypothesis 2. *Loss-averse decision makers bunch at the lottery with an upside of €8 in HI, i.e., $x_{HI} = s_{HI}$.*

From Hypotheses 1 and 2 follows also the prediction that decision makers choose lotteries with a larger expected value in HI than in LO. Loss-averse subjects bunch around the lottery with the €8 upside in HI, which is close to the upside of the lottery with the highest expected value, and choose lower upsides in LO.

Hypothesis 3. *Loss-averse subjects choose, on average, a lottery with a higher expected value in HI than in LO.*

2.2.3 Results of the Main Experiment

The first result supports Hypothesis 1. In the LO treatment with peer earnings of €2, decision makers chose an average upside of €7. In the HI treatment with peer earnings of €8, the preferred lottery of decision makers paid an average upside of €8.25.

Comparing LO to HI, decision makers reduced their risk taking by decreasing their average upside by €1.25—a marginal effect of 18%. The mean difference in upsides between treatments is significant in an OLS regression. Column 1 of Table 2.2 shows the results of regressing the upside choices of each decision

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[9] Sufficient loss aversion is $\lambda > 1 + \mu$, with $\mu = ((\overline{x} - 2s_{\mathrm{HI}}) / s_{\mathrm{LO}})((1 + \eta) / \eta)$. For instance, if $\eta = 1$, then $\mu = 1/2$ and $\lambda > 3/2$.

Table 2.2. Treatment Effect in Risk Taking, Main Experiment

	OLS: Lottery Upsides			Logit: Lottery Upside > 8			OLS: EV of Lottery		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1 if HI treatment	1.25***	1.25***	1.30***	1.25***	1.40***	1.84***	0.25***	0.14**	0.15**
	(0.35)	(0.31)	(0.31)	(0.39)	(0.42)	(0.49)	(0.09)	(0.06)	(0.06)
Risk attitude		0.09**	0.07*		0.05	0.04		0.02*	0.02**
		(0.04)	(0.04)		(0.05)	(0.06)		(0.01)	(0.01)
Controls for days, time, and wave	No	No	Yes	No	No	Yes	No	No	Yes
Constant	7.00***	6.00***	5.25***	−1.31***	−2.01***	−5.07**	4.57***	4.50***	4.10***
	(0.27)	(0.42)	(1.14)	(0.30)	(0.65)	(2.14)	(0.08)	(0.12)	(0.32)
Observations	132	126	126	132	126	122	132	126	126
Adj./Pseudo R^2	0.08	0.14	0.13	0.06	0.09	0.20	0.05	0.07	0.11

Notes: Columns 1–3 show regressions of risk taking, measured by the chosen lottery upside, on a treatment dummy (equal to 1 for the HI treatment). I added controls for risk attitudes to this regression in Column 2 and additional controls for the days of the week, time of the day, and the wave of the experiment in Column 3. Columns 4–6 present the results of a logistic regression to test whether choosing a lottery upside larger than €8 is more likely in the HI treatment than in the LO treatment. Columns 5 and 6 use the same controls as Columns 2 and 3, respectively. Columns 7–9 show the analyses of Columns 1–3 with the expected value of the chosen lottery as the dependent variable. In parentheses are (heteroskedasticity-robust) standard errors. Significant at the 1 (5) [10] percent level: *** (**) [*].

maker on a treatment dummy, which is equal to one for decision makers in the HI treatment.

Result 1. *On average, riskier lotteries were chosen in HI than in LO.*

This coefficient remains significant when controlling for risk attitudes of the decision makers (Column 2) and, additionally, for the day of the week, time of the day, and the wave of the experiment (Column 3). This indicates that the implementation of the experiment—most importantly, the randomization into treatments—worked well. The risk attitude control variable is significant (measured 1–2 weeks after the main experiment took place, see Section 2.2.1.1 for details). The positive coefficient indicates that subjects who are less (more) risk-averse chose relatively (less) riskier lotteries.

Figure 2.4 shows a histogram of the chosen upsides per treatment—larger values imply a higher willingness to take risk. Consistent with Result 1, the distribution of upsides in the LO treatment is statistically smaller than in the HI: Subjects are more likely to choose less risky lotteries in LO than in HI. A Mann–Whitney U-test (Kolmogorov–Smirnov test) yields a p-value of 0.0001 (0.001).[10]

Reflecting the average upside choices across treatments, I find that decision makers chose on average lotteries with a higher expected value in HI than in LO.

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[10] P-values in this study refer to two-sided tests.
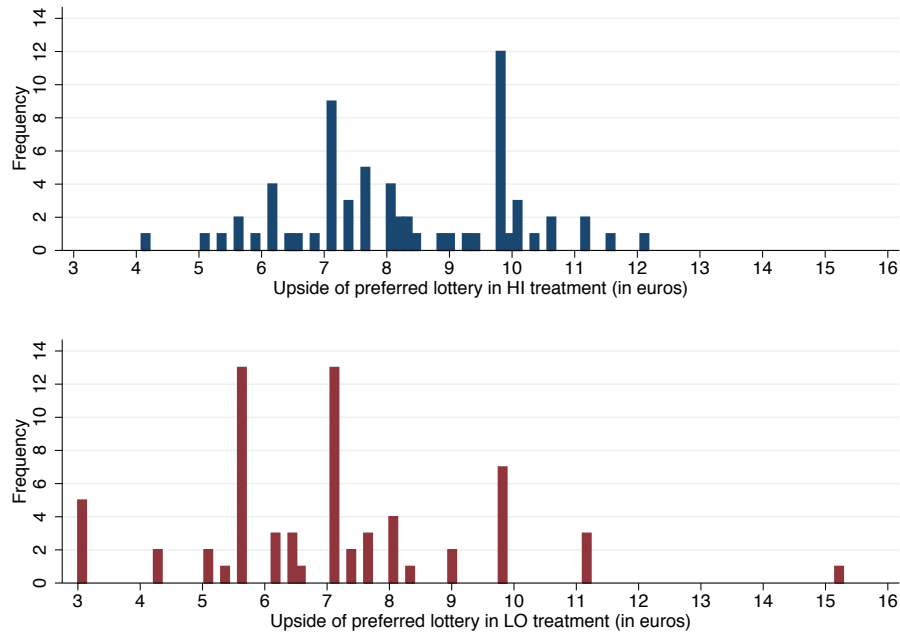
Figure 2.4. Frequencies of Risk Taking in the Main Experiment per Treatment

The last three columns in Table 2.2 show that this treatment effect is significant and robust to controls.

Result 2. *Decision makers chose on average lotteries with a higher expected value in HI than in LO.*

As shown in the previous section and articulated in Hypothesis 2, the social-comparisons–based, reference-dependent preference model predicts that loss-averse subjects bunch at the €8 upside in HI. However, there is no evidence for "bunching." Only 4 decision makers chose this lottery in HI—matching exactly the respective frequency in LO. While Figure 2.4 seems to indicate that more decision makers in HI chose lotteries with upsides around €8 than in LO, this is not statistically significant.[11]

Result 3. *Decision makers do not bunch around the €8 lottery in HI.*

Figure 2.4 also illustrates that more decision makers chose a lottery with an upside larger than €8 more often in HI than in LO. I test the statistical sig-

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[11] I test this by constructing a binary outcome variable which equals 1 in the case that $\hat{x} \in [7.59, 8.4]$ and zero otherwise. A logit (probit) regression of this binary outcome variable on the treatment dummy yields insignificant treatment differences. This remains to be the case when I change the interval to $[7.84, 8.14]$ and $[8, 8.14]$.

nificance of this difference in lottery choices across treatments by constructing a binary outcome variable which equals 1 in the case of $\hat{x} > 8$ and zero otherwise. Column 4 in Table 2.2 reports the results of a logistic regression of this binary outcome variable on the treatment dummy. The estimation yields a significantly positive coefficient of the treatment dummy: being assigned to the HI treatment increases the likelihood of choosing a lottery with $\hat{x} > 8$ by 27%. Columns 5 and 6 show that this result is robust to controlling for risk attitudes, the day of week, time of the day, and the wave of the experiment.

Result 4. *Lotteries with $\hat{x} > 8$ were chosen more often in HI than in LO.*

2.2.4 Discussion of the Main Experiment

The findings of the main experiment suggest that decision makers consider different lotteries as desirable between LO-HI treatments. The desirability of lotteries is driven by social reference points, i.e., peer earnings, as shown in Results 1, 2, 3, and 4. Lotteries with larger upsides, in particular upsides larger than €8, are more desirable to the decision makers in the HI treatment relative to the LO treatment.

The average upside and expected value choices (Results 1 and 2) are in line with loss aversion around peer earnings. However, instead of bunching at the €8 upside, decision makers chose upsides substantially larger than €8 in HI. Importantly, upside choices above €8 are significantly more frequent in HI than LO (Result 4) and they are implying risk proclivity as they are often above €8.25. These findings are not in line with loss aversion around peer earnings, which predicted bunching at €8. In the following, I discuss extensions to the model that can account for upside choices larger than €8 in HI (and are equally consistent with the observed lottery choices in LO).

Upside choices larger than €8 in HI suggest that decision makers aspire to earn more than their peers. Such aspirations could reflect a direct preference for earning more than their peers or a preference to receive the same overall utility as peer subjects—who stay in the lab for a shorter time than decision makers, see Section 2.2.1—with overall utility including the difference between the utility of experimental earnings and the disutility of the time spent at the experiment. Both aspiration accounts could be conceptualized by reference points of decision makers that would not be €8 but a greater amount, i.e., $€8 + \gamma$ with $\gamma > 0$. While loss aversion around reference points $\in (8, 8.25]$ would predict upside choices of $x \in (8, 8.25]$, this does not hold for reference points above €8.25. This results from the fact that loss aversion cannot predict risk-seeking lottery choices. Additional assumptions would have to be made for these lottery choices. Plausible candidates would be (i) that decision makers anticipate a utility jump at the reference point and (ii) that the social comparison utility exhibits dimin-

ishing sensitivity around such reference points with convex social comparison utility below the reference point and concave social comparison utility above the reference point.

While these findings document social reference point effects on risk taking, it remains to be shown that the observed risk-taking behavior does not reflect nonsocial reference point effects. Decision makers may expect to earn what they could have earned if they had been assigned the peer role. This may lead to a difference in expectations–based reference points between HI-LO treatments, which would be capable—together with loss aversion around these expectations–based reference points—of explaining the treatment effect on risk taking in the main experiment. In the next section, I investigate this alternative explanation by means of two control experiments. Both control experiment provide evidence against the alternative explanation. Thus, the next section establishes further evidence that the treatment effect on risk taking observed in the main experiment results from social reference points.

2.3   Social vs. Nonsocial Reference Points

The previous section showed that decision makers responded to social reference points by taking more risk in the case of larger peers' earnings. This behavior is consistent with the predictions of a model featuring the social-comparisons–based reference points and loss aversion, as presented above. This section investigates an alternative explanation for the observed behavior: expectations–based reference points. Before decision makers knew what they would be able to earn in the experiment, they knew what they could have earned, had they been assigned to the peer role. These counterfactual earnings potentially inform subjects' expectations, which in turn influence their (expectations–based) reference points. Upon being introduced to their risky choices, these reference points may adapt to the new information arising from the risky choice—or remain unchanged. In case that expectations–based reference points do not change, decision makers would have made their lottery choice with different nonsocial reference points between HI-LO treatments. Together with loss aversion relative to such potential expectations–based reference points, this could account for the observed differences in risk taking.

I designed two control experiments to test whether slow-changing, expectations-based reference points could serve as a valid alternative explanation for the decision makers' risk taking in the main experiment. The next two sections describe these control experiments and discuss its results in turn.

2.3.1 Nonsocial Control Experiment

In the nonsocial control experiment, I removed behavioral motives based on so-cial reference points from risk taking, while keeping the potential for differences in expectations-based reference points in place. Thus, expectations-based refer-ence points make the same predictions regarding risk taking—based on coun-terfactual earnings—as for the main experiment. If the same difference in risk taking between HI-LO treatments in the nonsocial control were to appear, this would suggest that expectations-based reference points explain the treatment effect reported in the main experiment. If, on the contrary, there is no differ-ence in risk taking in the nonsocial control, this would provide support that it is indeed social reference points which affect risk taking in the main experiment.

2.3.1.1 Design of the Nonsocial Control Experiment

The nonsocial control basically replicates the main experiment with an impor-tant innovation: Only one subject participated in each session. The experimenter tossed a coin in front of the subject to randomly assign one of two roles: active or passive. Passive subjects received €2 in LO and €8 in HI. Active subjects received the outcome of a lottery they chose from a set of lotteries. The risky choice is exactly the same as in the main experiment. Before active subjects received any information regarding their risky choice, they learned what they would have earned if they had been assigned the passive role: active subjects chose their preferred lottery while knowing that they could have earned €8 in the HI or €2 in the LO treatment.

The crucial feature is that both decision makers (in the main experiment) and active subjects (in the nonsocial control) chose their preferred lottery in light of the same counterfactual earnings. However, only for decision makers did this constitute a social comparison in earnings. Active subjects were not accompanied by peers to compare earnings with. Therefore, motives based on social reference point were removed from their risk taking. However, potential differences in expectations-based reference points were kept constant between treatments. Active subjects knew what they could have earned, if they had been assigned the passive role just as much as decision makers knew what they could have earned, if they had obtained the peer role. This difference in designs allows to investigate whether social comparison based motives explain the findings in the main experiment rather than expectations-based reference points.

The nonsocial control was conducted in three waves at two office rooms of the Bonn Graduate School of Economics in fall 2013 and in the BonnEcon-Lab in summer and fall 2014. In total, 262 subjects—134 active and 128 pas-sive—participated in 262 sessions. No subject participated in more than one treatment (and in any other experiment conducted for this paper). Only male

subjects were invited to make the results comparable to the main experiment. Each session had a duration for 12 to 20 minutes. Subjects earned on average €8.50. Active subjects were also re-invited to participate in a second part of the experiment which measured their risk attitudes (see Section 2.2.1.1). The second part was conducted at the BonnEconLab. All but 15 active subjects from the nonsocial control participated (attrition rate of 11%). Each session lasted for at most 40 minutes, and subjects earned on average €12.

2.3.1.2 Nonsocial Control Results

Figure 2.5 shows that active subjects in the nonsocial control experiment chose fairly similar lotteries across treatments. This is also reflected in the fact that the average chosen upsides were almost identical in HI and LO. In the LO treatment, active subjects chose an average upside of €7.62. In the HI treatment, the average preferred upside of was €7.65. Additionally, across both treatments, same frequency of lottery choices with upsides larger than €8 was roughly the same.

In the following, I present the main results of the paper: the treatment effects on risk taking in the main experiment are significantly larger than in the nonsocial control. This allows to identify social reference points on risk taking. I replicate Results 1, 3, and 4 in a difference (between main experiment and nonsocial control) in differences (between HI-LO treatments) analysis.

I regress the upside choices of the decision makers and active subjects, respectively, on a treatment dummy (= 1 if HI), on an experiment dummy (= 1 if nonsocial control), and an interaction term of the two. The coefficient of the interaction term estimates the difference in differences treatment effect on risk taking. Table 2.3 reports the results of such difference-in-differences estimations. It turns out that the coefficients on the interaction term are significantly smaller than zero. Thus, the differences on risk-taking behavior reported in Results 1, 3, and 4 are significantly smaller in the nonsocial control compared to the main experiment.

Result 5. *The difference in risk taking reported in Result 1 is significantly larger in the main experiment than in the nonsocial control. The difference in expected values reported in Result 3 is significantly larger in the main experiment than in the nonsocial control. The difference of lottery choices with an upside larger than €8 reported in Result 4 is significantly larger in the main treatment than in the nonsocial control.*

Result 5 provides evidence that the treatment effects on risk taking of the main experiment identify social reference point effects rather than nonsocial— e.g., expectations-based—reference points. This evidence rests on the assumption that potential differences in expectations–based reference points are constant between the main experiment and the nonsocial control. This assumption
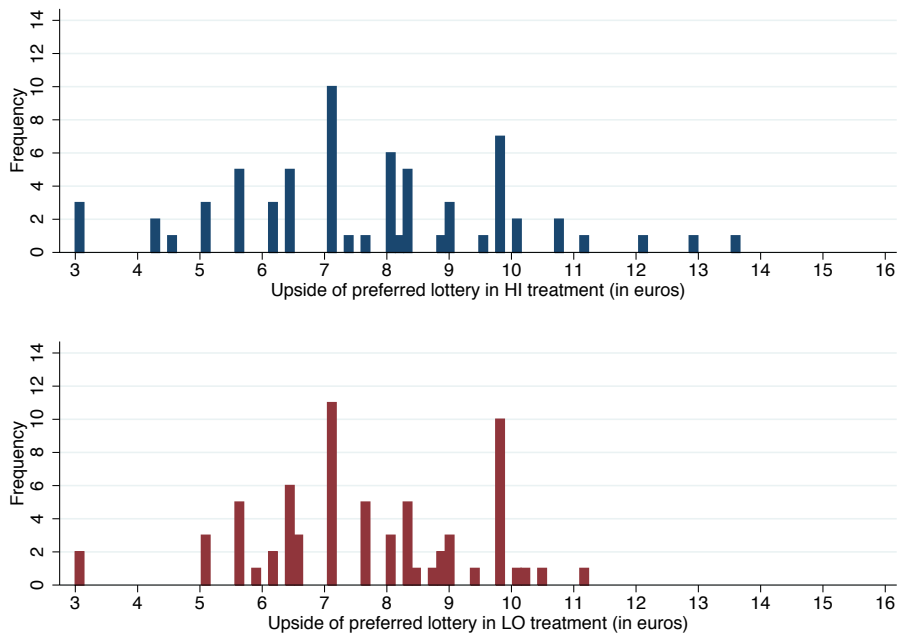
Figure 2.5. Frequencies of Risk Taking in the Nonsocial Control Experiment per Treatment

appears reasonable, since in both experiments subjects learn about the same counterfactual earnings. This holds, however, only if counterfactual earnings that are actually earned by peers (like in the main experiment) are not more informative for the expectation formation of individuals than purely counterfactual wages (like in the nonsocial control). In the next section, I present further support on social reference points effects on risk taking that is independent of the discussed assumption.

2.3.2 Peer-Lottery Control Experiment

I designed the peer-lottery control experiment such that social reference points predict a similar treatment effect on risk taking as in the main experiment, while expectations—and thus expectations–based reference points—are kept constant across HI-LO treatments. Thus, finding a similar treatment effect on risk taking in the peer-lottery control would corroborate that social reference points affect risk taking.

Table 2.3. Comparing Treatment Effects between the Main Experiment and the Nonsocial Control

	OLS: Lottery Upsides			Logit: Lottery Upsides > 8			OLS: EV of Lottery		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1 if HI treatment	1.25***	1.21***	1.26***	1.25***	1.40***	1.65***	0.25***	0.14**	0.15**
	(0.35)	(0.30)	(0.31)	(0.39)	(0.43)	(0.47)	(0.08)	(0.07)	(0.06)
1 if nonsocial control exp.	0.62*	0.49	0.21	0.89**	0.93**	0.68	0.23	0.09	0.02
	(0.34)	(0.32)	(0.38)	(0.39)	(0.43)	(0.59)	(0.08)	(0.08)	(0.07)
1 if nonsocial and HI treatment	−1.22**	−1.17**	−1.36***	−1.26**	−1.37**	−1.76***	−0.40***	−0.26***	−0.29***
	(0.49)	(0.46)	(0.46)	(0.53)	(0.57)	(0.63)	(0.06)	(0.11)	(0.10)
Risk attitude		0.14***	0.14***		0.13***	0.12***		0.02**	0.02***
		(0.03)	(0.03)		(0.04)	(0.04)		(0.01)	(0.01)
Controls for day, time and wave	No	No	Yes	No	No	Yes	No	No	Yes
Constant	7.00***	5.45***	5.26***	−1.31***	−2.87***	−4.28***	4.57***	4.44***	4.48***
	(0.27)	(0.36)	(1.02)	(0.30)	(0.53)	(1.24)	(0.06)	(0.08)	(0.20)
Observations	266	245	245	266	245	242			245
Adj./Pseudo R^2	0.04	0.14	0.17	0.03	0.08	0.13	0.03	0.07	0.07

Notes: Columns 1 to 3 show regressions of risk taking, measured by the lottery upside, on a treatment dummy (= 1 for the HI treatment), an experiment dummy (= 1 for the nonsocial control experiment) and an interaction variable between the treatment and experiment dummies. I added controls for risk attitudes in Column 2 and additional controls for the days of the week, time of the day, and the wave of the experiment in Column 3. In Columns 4–6 present the results of a logistic regression on a different dependent variable—whether the lottery upside was larger than €8 (= 1) or not (= 0)—which repeats the analyses of Columns 1–3. In Columns 7–9 show the analyses of Columns 1–3 with the expected value of the chosen lottery as the dependent variable. In parantheses are (heteroskedasticity-robust) standard errors. Significant at the 1 (5) [10] percent level: *** (**) [*].

2.3.2.1 Design of the Peer-Lottery Control Experiment

The peer-lottery control augmented the main experiment with a critical innovation: Peer subjects received a 50-50 lottery between €2 or €8 instead of a fixed €2 or €8 payment. The first information decision makers received was the lottery that their peers were endowed with. After a delay of 5 minutes, the experimenter flipped a coin in front of the decision makers to determine their peers' earnings. After learning their peers' earnings, decision makers received information regarding their risky choice—the same risky choice used in the other risk-taking experiments in this paper—and then chose their preferred lottery. The random outcome of the peers' lottery determined the treatment assignment of decision makers. If the coin flip determined peers to earn €2, decision makers were in the LO treatment, while they were in the HI treatment if the coin flip determined their peers to earn €8. See Figure 2.1 for a comparison between the main experiment and the peer-lottery control.
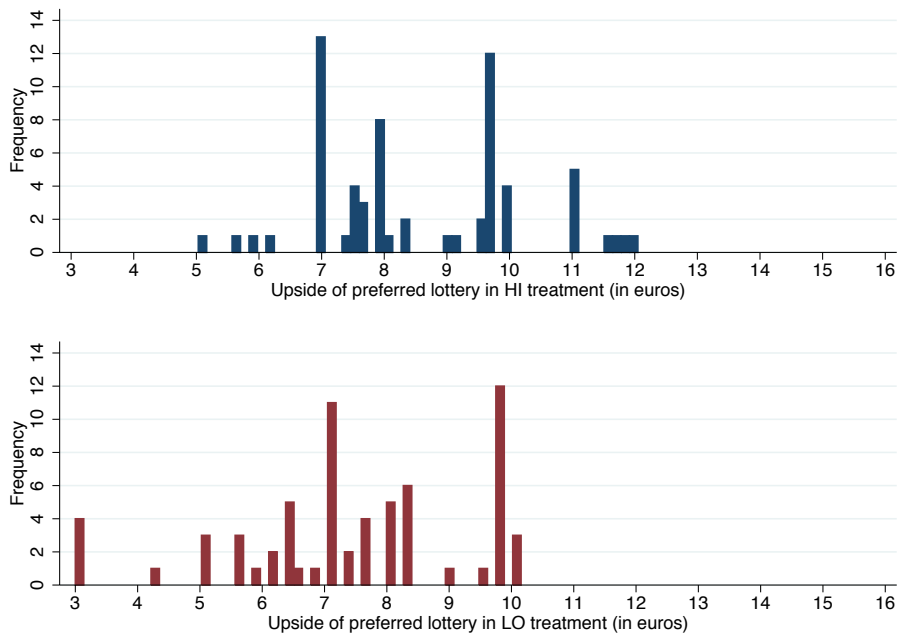
Figure 2.6. Frequencies of Risk Taking in the Peer-Lottery Control per Treatment

The design of the peer-lottery control allows testing for social reference point effects on risk taking while holding decision makers' initial expectations—based on counterfactual earnings—constant across HI-LO treatments. In both treatments decision makers knew that their peers would be paid with equal chances either €2 or €8. In that sense expectations based on counterfactual earnings cannot vary between HI-LO treatments. However, peers still earn either €8 and €2. Therefore, loss aversion with respect to peer earnings predicts the same treatment effect on risk taking as in to the main experiment.

The peer-lottery control was conducted at two office rooms of the BonnEcon-Lab in summer 2014. In total, 262 subjects—131 decision makers and 131 peers—participated in 131 sessions of the peer-lottery control. No subject participated in more than one treatment (and in none of the other experiments conducted for this paper). I invited only male subjects to keep the results comparable. Subjects earned on average €8.35. Decision makers were also invited to a second part in which their risk attitudes were elicited. All but 9 decision makers returned (attrition rate of 7%). Each session of the second part had a duration of at most for 20 minutes. Subjects earned on average €12.5.

Table 2.4. Comparing Treatment Effects between the Peer-Lottery Control and the Nonsocial Control

	OLS: Lottery Upsides			Logit: Lottery Upsides > 8			OLS: EV of Lottery		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1 if HI treatment	1.20***	0.97***	1.04***	0.59*	0.44	0.71*	0.09	0.08	0.09
	(0.30)	(0.32)	(0.34)	(0.36)	(0.39)	(0.41)	(0.07)	(0.07)	(0.07)
1 if nonsocial control exp.	0.18	0.04	0.39	0.21	0.14	0.23	0.06	0.2	0.13
	(0.31)	(0.31)	(0.43)	(0.36)	(0.37)	(0.55)	(0.08)	(0.07)	(0.08)
1 if nonsocial and HI treatment	−1.17**	−0.95**	−1.01**	−0.61	−0.43	−0.75	−0.24**	−0.21*	−0.21*
	(0.46)	(0.47)	(0.48)	(0.50)	(0.54)	(0.56)	(0.11)	(0.11)	(0.11)
Risk attitude		0.11***	0.11***		0.10***	0.11***			0.16**
		(0.03)	(0.03)		(0.03)	(0.04)			(0.01)
Controls for day, time and wave	No	No	Yes	No	No	Yes	No	No	Yes
Constant	7.44***	6.31***	5.48***	−0.63**	−1.69***	−4.03***	4.74***	4.57***	4.42***
	(0.23)	(0.42)	(0.94)	(0.26)	(0.46)	(1.46)	(0.06)	(0.12)	(0.22)
Observations	265	241	241	265	241	240	265	241	241
Adj./Pseudo R^2	0.05	0.10	0.08	0.01	0.03	0.10	0.01	0.04	0.03

Notes: In Columns 1 to 3 show regressions of risk taking, measured by the chosen lottery upside, on a treatment dummy (= 1 for the HI treatment), an experiment dummy (= 1 for the nonsocial control experiment) and an interaction between the later two. I added controls for risk attitudes to this regression In Column 2 and additional controls for the days of the week, time of the day, and the wave of the experiment in Column 3. Columns 4–6 present the results of a logistic regression of a different dependent variable—whether the chosen lottery upside was larger than €8 (= 1) or not (= 0)—which repeats the analyses of Columns 1–3. Columns 7–9 show the analyses of Columns 1–3 with the expected value of the chosen lottery as the dependent variable. I state (heteroskedasticity-robust) standard errors in parentheses. Significant at the 1 (5) [10] percent level: *** (**) [*].

2.3.2.2 Peer-Lottery Control Results

The results of the peer-lottery control provide further support for social reference point effects on risk taking. On average, decision makers chose an average upside of €8.64 in HI and of €7.44 in LO. This difference in risk taking is significant in an OLS regression and significantly larger than in the nonsocial control, see Columns 1 to 3 of Table 2.4. Additionally, the average difference in expected values between HI-LO treatments is larger in the peer-lottery control relative to the main experiment, see Columns 7 to 9. While the likelihood that decision makers chose upsides larger than €8 is larger in the HI treatment than in the LO treatment, see Row 1 and Columns 4 to 6, this difference is not significantly larger than in the nonsocial control, see Row 3 and Columns 4 to 6.

Result 6. *Result 5 can be replicated in difference-in-difference analyses on risk taking between HI-LO treatments in the peer-lottery and nonsocial control experiments. The treatment effect on upsides, expected values, and frequency of upside*

choices above €8 is larger in the peer-lottery control than in the nonsocial control. The former two are significant, while the latter is not.

In sum, these findings indicate that the treatment effect on risk taking observed in the main experiment is caused by social reference points rather than nonsocial reference points. When keeping expectations constant across treatments and only varying social reference points, decision makers behaved similarly relative to the main experiment.

2.4   Conclusion

Using a simple laboratory experiment, I provide causal evidence for social reference point effects on risk taking. Decision makers increase their risk taking in light of relatively larger peer earnings—in the absence of any other social motives and forms of peer effects. The observed risk taking is consistent with an aversion against earning less than others. These findings provide clean evidence that relative concerns affect human behavior.

The interpretation of the main experiment is substantiated by means of two control experiments. Most importantly, when removing motives based on social reference point but maintaining the potential for alternative explanations based on nonsocial—e.g., expectations-based—reference points constant, risk taking is not affected. Difference-in-difference analyses between the main and nonsocial control experiments support the interpretation that the treatment effect on risk taking in the main experiment identifies social reference points effects.

The results of this study are applicable to the recent literature on the use of relative concerns at the workplace (Moldovanu et al., 2007) Performance rankings may incentivize workers to increase their effort in order to improve relative performance—independent of additional pecuniary incentives. This study suggests, that apart from effort, the willingness to take risk of workers may also be affected by such social comparisons. Any principal that may want to make use of social incentives should, therefore, take the potential effect on risk taking into account as well. My results also suggest that even in the case that wages are not made transparent through performance rankings, but individuals receive private signals of their relative outcomes, behavioral consequences should be anticipated by principles.

The findings of this study imply that salient peer outcomes affect the reference point of individuals. In my study, subjects were presented with one peer outcome only, but in many applications, individuals observe multiple peer outcomes (Falk and Knell, 2004). An interesting avenue for future research, therefore, is to attain a better understanding, both theoretically and empirically, of which peer outcomes individuals will find most salient or choose from as a comparison when confronted with multiple sources for social reference points.

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Appendix 2.A   Instructions

Main experiment, decision maker, HI [LO] treatment

In this experiment, your task is to complete a survey. Participant B completes the same survey.

The both of you receive the show-up fee for your participation in this experiment. Participant B receives an additional payment of €8 [€2]. You can also receive an additional payment. Your additional payment is not determined yet. Your additional payment depends on your decision-making before you start completing the survey.

Notice, participant B does not learn your additional payment and leaves the lab before you do.

[next screen]

Your additional payment depends on your choice between different options.

One option is the certain payment of €3.

All other options are binary lotteries. Among all options, one outcome is 0. You can choose the other outcome freely between a minimum and maximum outcome. The higher you choose this outcome, the lower is the likelihood that you receive it.

We use urns to display lotteries graphically in this experiment. If you choose a lottery, then the computer randomly chooses which outcome you receive as your additional payment. This happens at the end of the experiment, after you completed the survey and are paid in cash.

Nonsocial control, active subject, HI [LO] treatment

In this experiment, your task is to complete a survey. If you would have been participant B, you would have to complete the same survey.

In both roles you receive the show-up fee for your participation in this experiment. If you would have been participant B, you would receive an additional payment of €8 [€2]. As participant A, you can also receive an additional payment. Your additional payment is not determined yet. Your additional payment depends on your decision-making before you start completing the survey.

[next screen]

Your additional payment depends on your choice between different options.

One option is the certain payment of €3.

All other options are binary lotteries. Among all options, one outcome is 0. You can choose the other outcome freely between a minimum and maximum outcome. The higher you choose this outcome, the lower is the likelihood that you receive it.

We use urns to display lotteries graphically in this experiment. If you choose a lottery, then the computer randomly chooses which outcome you receive as your additional payment. This happens at the end of the experiment, after you completed the survey and are paid in cash.

Peer-lottery control, decision maker, HI [LO] treatment

In this experiment, your task is to complete a survey. Participant B completes the same survey.

The both of you receive the show-up fee for your participation in this experiment. Participant B receives an additional payment of either €8 or €2. It is random whether participant B receives €8 or €2. A coin toss will be perfomed in front of you to determine the additional payment of participant B. With heads, participant B earns €2 and with tails €8. Please wait until the experimenter will come to you to perform the coin toss.

[next screen]

The coin toss determined that participant B receives €8 [€2] additionally. You can also receive an additional payment. Your additional payment is not determined yet. Your additional payment depends on your decision-making before you start completing the survey.

Notice, participant B does not learn your additional payment and leaves the lab before you do.

[next screen]

Your additional payment depends on your choice between different options.

One option is the certain payment of €3.

All other options are binary lotteries. Among all options, one outcome is 0. You can choose the other outcome freely between a minimum and maximum outcome. The higher you choose this outcome, the lower is the likelihood that you receive it.

We use urns to display lotteries graphically in this experiment. If you choose a lottery, then the computer randomly chooses which outcome you receive as your additional payment. This happens at the end of the experiment, after you completed the survey and are paid in cash.

Appendix 2.B   Screenshots
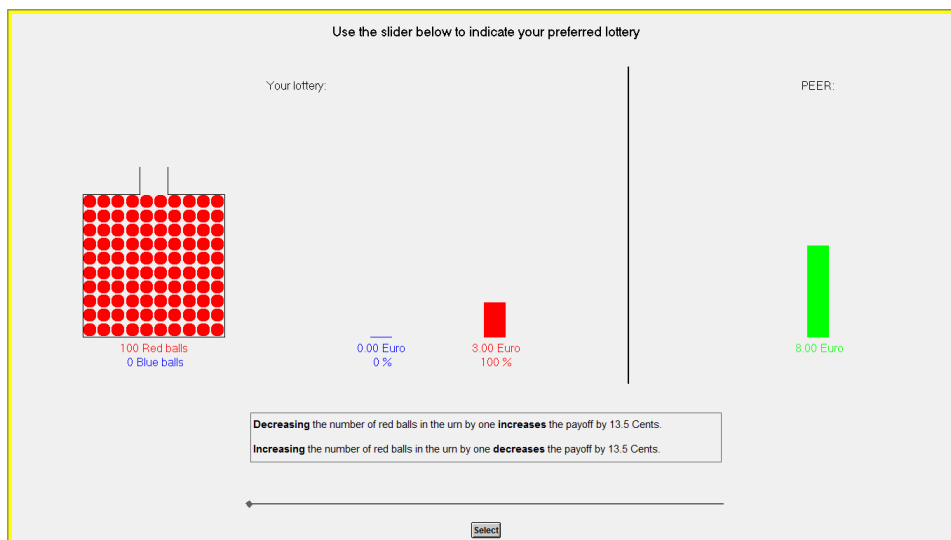
Figure 2.B.1. Decision Screen of the Main Experiment and Peer-Lottery Control, HI Treatment (Slider Position 1)

Note: The position of the slider indicates a preferred certain payment of €3.
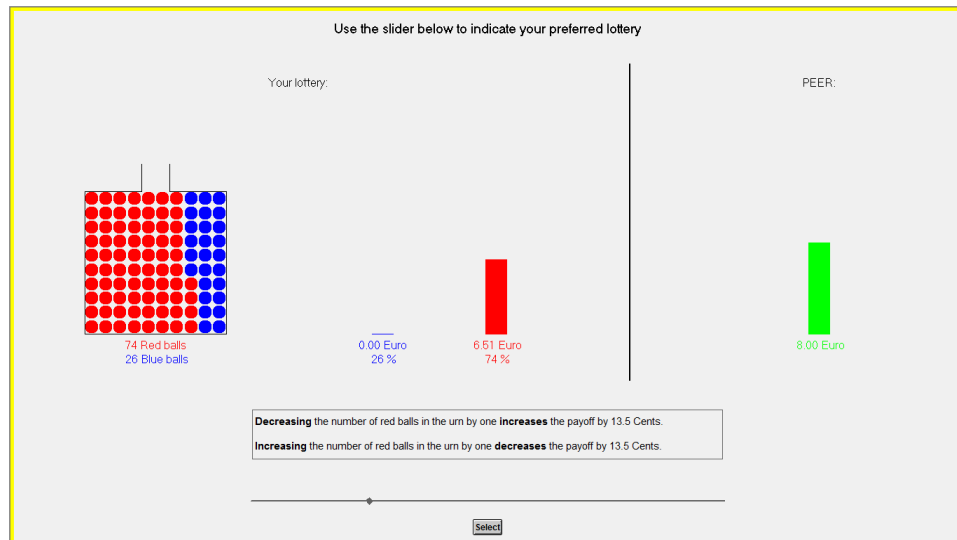
Figure 2.B.2. Decision Screen of the Main Experiment and Peer-Lottery Control, HI Treatment (Slider Position 2)

Note: The position of the slider indicates a preferred lottery that pays €6.51 with 74%.
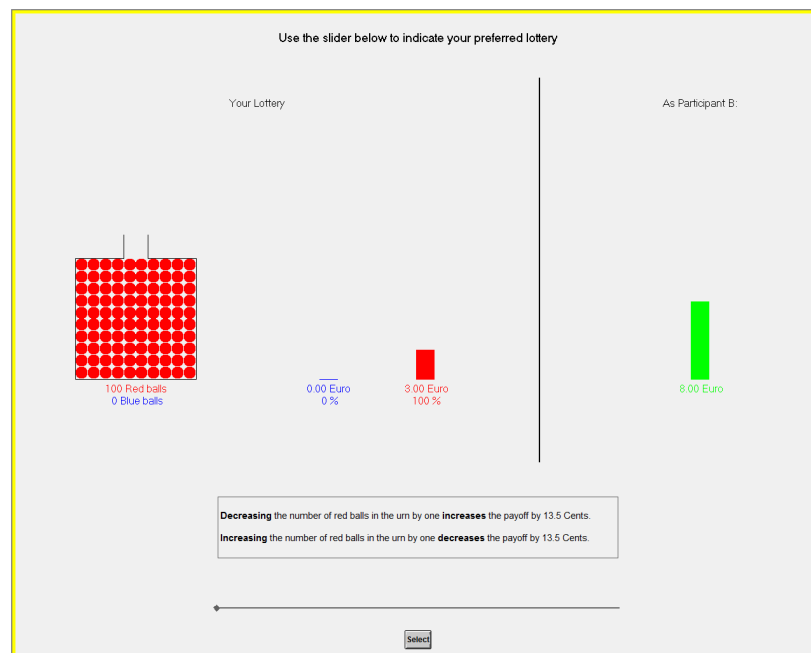
Figure 2.B.3. Decision Screen of the Nonsocial Control, HI Treatment

Note: The position of the slider indicates a preferred certain payment of €3.

3

Concentration Bias in Intertemporal Choice*

Joint with Holger Gerhardt and Louis Strang

3.1 Introduction

Any decision that we make has intertemporal consequences. These consequences often stretch over numerous days, months, or even years. For instance, missing out on exercising at the gym today deteriorates physical well-being each following day; or aspiring to a bonus payment at the end of the year may animate overtime work each day until the end of the year. According to exponential and hyperbolic discounting—the canonical approaches to intertemporal decision making in economics—individuals evaluate a decision by aggregating all its consequences into a weighted sum, with the weights reflecting their time preferences. Individuals then choose the option that yields the highest sum. Whether constant and hyperbolic (present-biased) discounting are sufficient to capture all important aspects of intertemporal decision making, however, remains an open question.

In this paper, we contribute to the understanding of intertemporal decision making by studying an attention-based approach to how individuals (mis-)aggregate intertemporal consequences. The potential of this approach stems from a pervasive characteristic of intertemporal decisions: positive consequences are often concentrated in a single, attention-grabbing period, while negative consequences are dispersed in non-tangible doses over numerous periods. For instance, avoiding the hassle of exercising at the gym today marginally deteriorates physical well-being each following day; or the prospect of receiving a large

bonus payment at the end of the year may come at the cost of working half an hour overtime each day until then. In light of the evidence from cognitive psychology that human perception is an important driver of individual decision making (see, e.g., Kahneman, 2003), this asymmetry may give rise to attention-based effects regarding how individuals aggregate intertemporal consequences and, thus, how they make intertemporal decisions.

The attention-based approach that we build on generates an overweighting of concentrated consequences relative to dispersed consequences. Thus, weighting of intertemporal consequences no longer solely reflects time preferences à la constant or present-biased discounting, but entails an asymmetric overweighting of particular consequences. Kőszegi and Szeidl (2013) recently introduced a model of economic decision making that formalizes such overweighting. Their model is based on the assumption that individuals weight the consequences in a period the more, the greater the difference between the maximum and minimum consequences in that period. Take, for instance, the example of getting a bonus payment at the end of the year at the expense of some overtime work each day until then or not getting any bonus payment at the benefit of no overtime work. The day the bonus payment is received is characterized by a large difference between consequences: the bonus payment is paid out or not. The days prior to the bonus payment are characterized by smaller differences between the consequences: little overtime work or none. Similarly, materializing or avoiding the hassle of going to the gym entails a greater distance in consequences today than for any later period, because in these later periods, each gym attendance generates only marginal changes in physical well-being. In general, consequences that are more concentrated—i.e., that are spread over fewer periods—than equal-sized dispersed consequences imply a greater per-period difference and are thus overweighted, according to the model.

Such concentration bias—when added to constant and present-biased discounting—yields two testable predictions on intertemporal decision making. First, individuals may behave overly impatient—relative to constant and present-biased discounting as the benchmark—when concentrated positive consequences of behaving impatiently precede its dispersed negative consequences. For instance, suffering from a little back pain each day in the future may be disregarded when this allows avoiding the attention-grabbing hassle of going to the gym now. Second, when concentrated positive consequences of behaving patiently succeed dispersed negative consequences, individuals may behave overly patient—relative to both exponential and hyperbolic discounting. For instance, little overtime work each day may be neglected in exchange for receiving an attention-grabbing bonus payment in the future. While concentration bias seems intuitively compelling, there exists—to our knowledge—no empirical investigation of it yet.

We designed a novel laboratory experiment to fill this gap. In particular, the design of our experiment allows us to investigate whether individuals systematically overweight concentrated intertemporal consequences relative to dispersed consequences. In doing so, we test both directions of concentration bias: Do subjects behave more impatiently when the negative consequences of impatient behavior are dispersed over multiple *later* periods? Do they behave more patiently when the negative consequences of patient behavior are dispersed over multiple *earlier* periods?

In our experiment, subjects were endowed with multiple earnings sequences. Each earnings sequence specified a series of 9 money transfers to subjects' bank accounts at given dates in the future. Subjects decided whether to decrease earlier payments at the benefit of increasing later payments. The sum total was the greater, the more money subjects allocated to later periods. The larger the amount of money that subjects decided to receive at later payment dates—i.e., the more they were willing to wait in order to receive an overall larger sum of money—the more patient we consider them (see Andreoni and Sprenger (2012)).

To test whether concentration bias affects intertemporal choices, we varied within-subject whether the intertemporal allocation was exclusively between concentrated consequences (CONC) or whether there were both concentrated and dispersed consequences (DISP). Put differently, between conditions, we changed the shape of the intertemporal budget constraint (the "intertemporal allocation technology"). We test whether subjects' decisions differed significantly between conditions.

In order to investigate both directions of a potential concentration bias, we compare intertemporal decisions between earnings sequences in $CONC_a$ and $DISP_a$ as well as between earnings sequences in $CONC_b$ and $DISP_b$, as described in the following. In condition $CONC_a$, subjects received a sequence of 9 dated money transfers that were separated by several weeks each. We can express this sequence as the vector

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

with the ith entry specifying the euro amount of the ith payment. $R \equiv 1 + r$ is an interest factor (with r being the nominal interest rate), and B is the endowment in the first period. That is, R and B denote parameters of a given income sequence (e.g., $B = €11$, $r = 15\%$), while x is the subject's choice variable. By choosing $x \in \{0, {}^1/_{100}, {}^2/_{100}, \ldots, 1\}$, subjects decided what fraction of their first payment they would forego in exchange for receiving the remaining fraction plus interest at the last payment date. (For instance, with $B = €11$ and $r = 15\%$, subjects could receive $€1 + €12.60$ as the last payment).

In condition $DISP_a$, subjects received similar earnings sequences as in $CONC_a$. The only difference between $DISP_a$ and $CONC_a$ was that RBx was not paid at the last payment date, but was dispersed across the last payment date and multiple earlier payment dates. In $DISP_{a8}$, for instance, c was dispersed over the last payment date and 7 earlier payment dates. That is, the earnings sequence given to subjects in $DISP_{a8}$ can be represented as

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

This means that in $DISP_a$, the negative consequences of impatient behavior—i.e., of choosing a low x—were dispersed. In $DISP_{a8}$, they took on the form of small payments over the last 8 periods. In $DISP_{a4}$ and $DISP_{a2}$, we dispersed payments over the last 4 and 2 periods, respectively. By contrast, in $CONC_a$, the negative consequence of allocating the entire amount to the soonest payment date was concentrated, and thus attention-grabbing, at the last payment date.

Concentration bias, therefore, predicts that individuals allocate less money to later payments in $DISP_a$ than in $CONC_a$. Importantly, the present value of $DISP_a$ is higher than that of $CONC_a$. Thus, in contrast to concentration bias, both exponential and hyperbolic discounting predict that the amount allocated to later periods in $DISP_a$ is at least as large as in $CONC_a$.

In condition $CONC_b$, subjects received similar sequences as in $CONC_a$. That is, also $CONC_b$ involved intertemporal allocation of money between two concentrated payments. $CONC_b$ differs from $CONC_a$ in that subjects received $B\left(1 - x\right)$ on the second-to-last payment date (instead of the first date):

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

In the associated dispersed condition, $DISP_b$, subjects were endowed with similar earnings sequences as in $CONC_b$. The only difference between $DISP_B$ and $CONC_B$ was that $B\left(1 - x\right)$ was not paid at the second-to-last date, but was dispersed over the second-to-last date and multiple earlier payment dates. In $DISP_{b8}$, for instance, $B\left(1 - x\right)$ was dispersed over the second-to-last date and 7 earlier dates. That is, the earnings sequence was of the type

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

Consequently, in DISP_{b8}, the negative consequences of being patient—i.e., choosing a high x—were dispersed, small reductions in the payments over the first 8 periods. In DISP_{b4} and DISP_{b2}, $B\,(1-x)$ was dispersed over the second-to-last payment date and 4 and 2 earlier dates, respectively.

For a comparison between CONC_b and DISP_b, concentration bias thus predicts that individuals allocate more money to the last date in DISP_b than in CONC_b. By contrast, both exponential and hyperbolic discounting predict the opposite effect: since the present value of the first eight payments is higher in DISP_b than in CONC_b, the allocation to the last date in DISP_b should be at most as large as in CONC_b.

The results of the experiment provide support for both predictions of concentration bias. Subjects allocated, on average, 5.5% more money to later payments in CONC_a than in DISP_a decisions and 6.5% more in DISP_b than in CONC_b. Both of these effects are statistically significant in parametric and non-parametric tests and correspond to an increase in "patience" by 9%, respectively. The effects are robust to controlling for alternative explanations based on a mere calculation inability or small-numbers aversion.

Our results provide novel evidence in support of the focusing model by Kőszegi and Szeidl (2013). While the focusing model is in line with many observations from the field (e.g. Warner and Pleeter, 2001; Davidoff et al., 2005; DellaVigna and Malmendier, 2006), it is hard or even impossible to discriminate between this model and competing explanations based on the particular field data alone, since the *ceteris paribus* assumption is violated. By enabling the manipulation of a single characteristic of the earnings sequences in a controlled environment, our lab experiment allows for establishing a *causal* effect of the dispersion of consequences on discounting. We thereby contribute to the literature in three important ways:

First, our results indicate that taking concentration bias into account enriches our understanding of intertemporal decision making beyond exponential and hyperbolic discounting.

Second, it helps explain a discrepancy between recent experimental findings and results from the analysis of field data: the observed degree of present bias and the incidence of time-inconsistent behavior are fairly low in experimental settings according to recent studies (Andreoni and Sprenger, 2012; Augenblick et al., 2015), while they have been found to be much more severe in the analysis of field data (e.g., low gym attendance, DellaVigna and Malmendier (2006); resistance to the annuitization of pension plans, Warner and Pleeter (2001) and Davidoff et al. (2005)). Our study resolves this discrepancy by identifying the lack of dispersed payments in previous lab experiments as a plausible source of this discrepancy.

Third, we contribute to the mostly theoretical and very recent literature in economics that introduces endogenous perception effects to economic decision making (e.g., Bordalo et al., 2013; Bushong et al., 2015). One particular implication is that one needs to be careful when interpreting the results of experimental studies that rely on multiple payments, like the novel method of "measuring discounting without measuring utility" proposed by Attema et al. (2015). We discuss this point in greater detail in Section 3.4.

We proceed in Section 3.2 with providing evidence for concentration bias in intertemporal decision making. We provide robustness analyses in Section 3.3. Section 3.4 concludes.

3.2 Evidence for Concentration Bias

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

3.2.1 Design

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

3.2.1.1 Intertemporal Choices

In our experiment, subjects were endowed with multiple earnings sequences of which only one was randomly chosen to be payoff relevant at the end of the experiment (random incentive mechanism). Each earnings sequence specified a series of 9 money transfers to subjects' bank accounts at given dates in the future. The earliest payment date was 5–7 days in the future. We describe the precise structure of these earnings sequences in the following paragraphs. Figures 3.1 and 3.2 visualize the earnings sequences.

Subjects decided for each earnings sequence whether to decrease earlier payments at the benefit of increasing later payments. The sum total was the greatest, the more money subjects allocated to later periods. Put differently, we implemented an intertemporal budget constraint with a positive nominal interest rate, r. The more money individuals allocated to later periods, the more
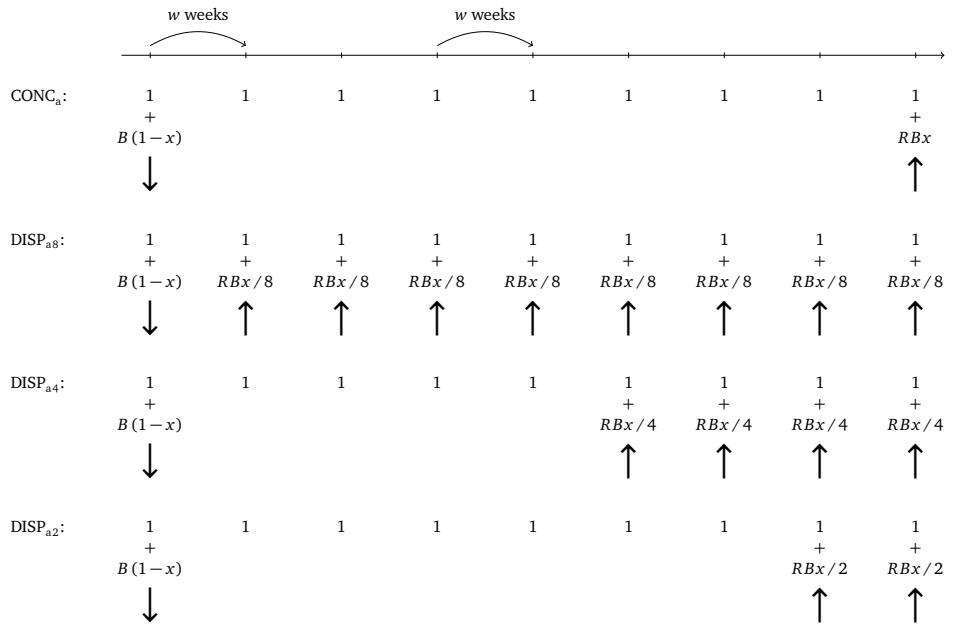
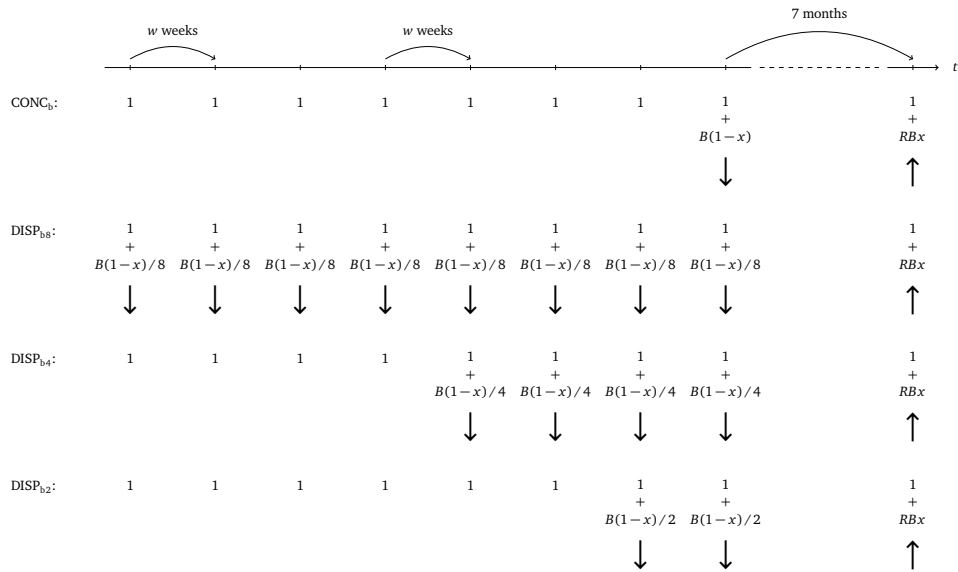**Figure 3.1.** Budget Sets: CONC$_a$ and DISP$_a$ Conditions

Note: For the values of B, R, and w that we used, see Section 3.2.1.3.

patient we consider them. In doing so, we extend the "convex budget set" approach to intertemporal decision making introduced by Andreoni and Sprenger (2012) to settings in which individuals face more than two payment periods.

We varied within-subject the characteristics of the intertemporal budget constraint between two conditions, CONC and DISP. In both conditions, subjects received a fixed amount of €1 at each of the 9 payment dates to hold the number of transfers constant across conditions. Subjects allocated an additional amount of money between payment dates. In CONC, the allocation was between exactly two payment dates. In CONC, the intertemporal allocation thus involved consequences that were concentrated in single periods each. Decreasing (increasing) a payment increased (decreased) a payment in exactly *one* other period. By contrast, in DISP, subjects allocated between multiple payment dates. More precisely, there was one consequence that was concentrated in a single period, while the other consequence was dispersed over multiple periods. Decreasing (increasing) the concentrated consequence increased (decreased) the payments in *several* (2, 4, or 8) other periods.

CONC consists of two types of earnings sequences, CONC$_a$ and CONC$_b$. In CONC$_a$, subjects could shift money from the earliest to the last payment date at the benefit of receiving interest. In CONC$_b$, subjects allocated between the second-to-last and the last payment date. In both CONC$_a$ and CONC$_b$, subjects received B euros if they allocated their additional payment to the earlier date.

Figure 3.2. Budget Sets: $CONC_b$ and $DISP_b$ Conditions

Note: For the values of B, R, and w that we used, see Section 3.2.1.3.

If they allocated it to the later date, they received RB euros, with $R \equiv 1 + r > 1$. They could also choose convex combinations of payments by choosing $x \in \{0, 1/100, 2/100, \dots, 1\}$ that determined an earlier payment of $B(1-x)$ euros and a later payment of RBx euros. While each payment date is separated by w weeks in $CONC_a$, this is true only for the first 8 payments dates in $CONC_b$. The distance between the second-to-last and last payment date was 7 months in $CONC_b$. 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 people exclusively choose the largest, latest payment.

DISP consists of two types of earnings sequences, $DISP_a$ and $DISP_b$, that are related to $CONC_a$ and $CONC_b$, respectively. In $DISP_a$, subjects allocated monetary amounts between the earliest payment date and multiple (2, 4, or 8) later payments. Thus, instead of receiving RBx euros at the last payment date like in $CONC_a$, the amount of RBx euros is dispersed over the last period and multiple (1, 3, or 7) earlier periods. In $DISP_b$, subjects allocate between multiple (2, 4, or 8) earlier payments and a single later payment. Instead of receiving $B(1-x)$ euros at the second-to-last date like in $CONC_b$, the amount of $B(1-x)$ euros is dispersed over the second-to-last payment date and multiple (1, 3, 7) earlier dates. The interval between payment dates follows the respective CONC counterpart for each DISP condition.

In the following, we are interested in the comparison of chosen allocations between $CONC_a$ and $DISP_a$. This comparison tests whether subjects behave dif-

ferently in the case that the negative consequences of behaving impatiently—
i.e., choosing a smaller x—are dispersed over multiple future periods (DISP_a)
rather than concentrated in a single future period (CONC_a). Concentration bias
predicts that individuals underweight dispersed consequences relative to con-
centrated consequences.[1] In DISP_a, the negative consequences of behaving im-
patiently are less tangible, as they are dispersed in small payments over many
periods. In CONC_a, the negative consequences of behaving impatiently are con-
centrated in a single—i.e., attention-grabbing—payment. Thus, individuals are
predicted to pay more attention to the negative consequences in CONC_a than in
DISP_a, which promotes impatient behavior in the latter condition.

Figure 3.3 shows the decision screen of an exemplary decision with $B = 11$
and $r \approx 15\%$ for both CONC_a (upper panel) and DISP_{a8} (lower panel). Through
a slider, subjects chose their preferred $x \in \{0, 1/100, 2/100, \ldots, 1\}$. The slider posi-
tion in Figure 3.3 indicates $x = 0.5$, i.e., the earliest payment is reduced by €5.50.
Since $r \approx 15\%$, this amounts to €6.30 that are paid at later payment dates. While
these €6.30 are paid in a single sum on the latest payment date in CONC_a, they
are dispersed into equal parts over the last 8 payment dates—i.e., 8 consecutive
payments of €0.79—in DISP_{a8}.[2] Concentration bias predicts that the dispersed
payment of €6.30 will be underweighted relative to the concentrated payment
of €6.30.

We are also interested in the comparison of allocation decisions between
CONC_b and DISP_b. This comparison tests whether subjects behave more pa-
tiently in DISP_b than in CONC_b. To reiterate, concentration bias predicts that
individuals underweight dispersed consequences relative to concentrated conse-
quences. Since the negative consequences of behaving patiently are dispersed in
DISP_b, individuals tend to neglect them according to concentration bias. On the
contrary in CONC_b, the negative consequences of behaving patiently are con-
centrated in a single—i.e., attention-grabbing—payment. Therefore, individu-
als are predicted to pay more attention to the negative consequences in CONC_b
than in DISP_b, which promotes patient behavior in the latter condition.

Figure 3.4 shows the decision screen of an exemplary decision with $B = €11$
and $r \approx 15\%$ for both CONC_b (upper panel) and DISP_{b8} (lower panel). The slider
position in Figure 3.4 indicates $x = 0.48$, which implies an additional €6.56 paid
at the latest payment date. While the remaining €5.28 ($B(1-x)$) are paid as
a single sum on the second-to-last payment date in CONC_b, they are dispersed
into equal parts over the first 8 payment dates—i.e., 8 consecutive payments of
€0.66—in DISP_{b8}.

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[1] This will be discussed in more detail below.

[2] We always rounded the second decimal place up so that the sum of the dispersed payments
was always at least as great as the respective concentrated payment.
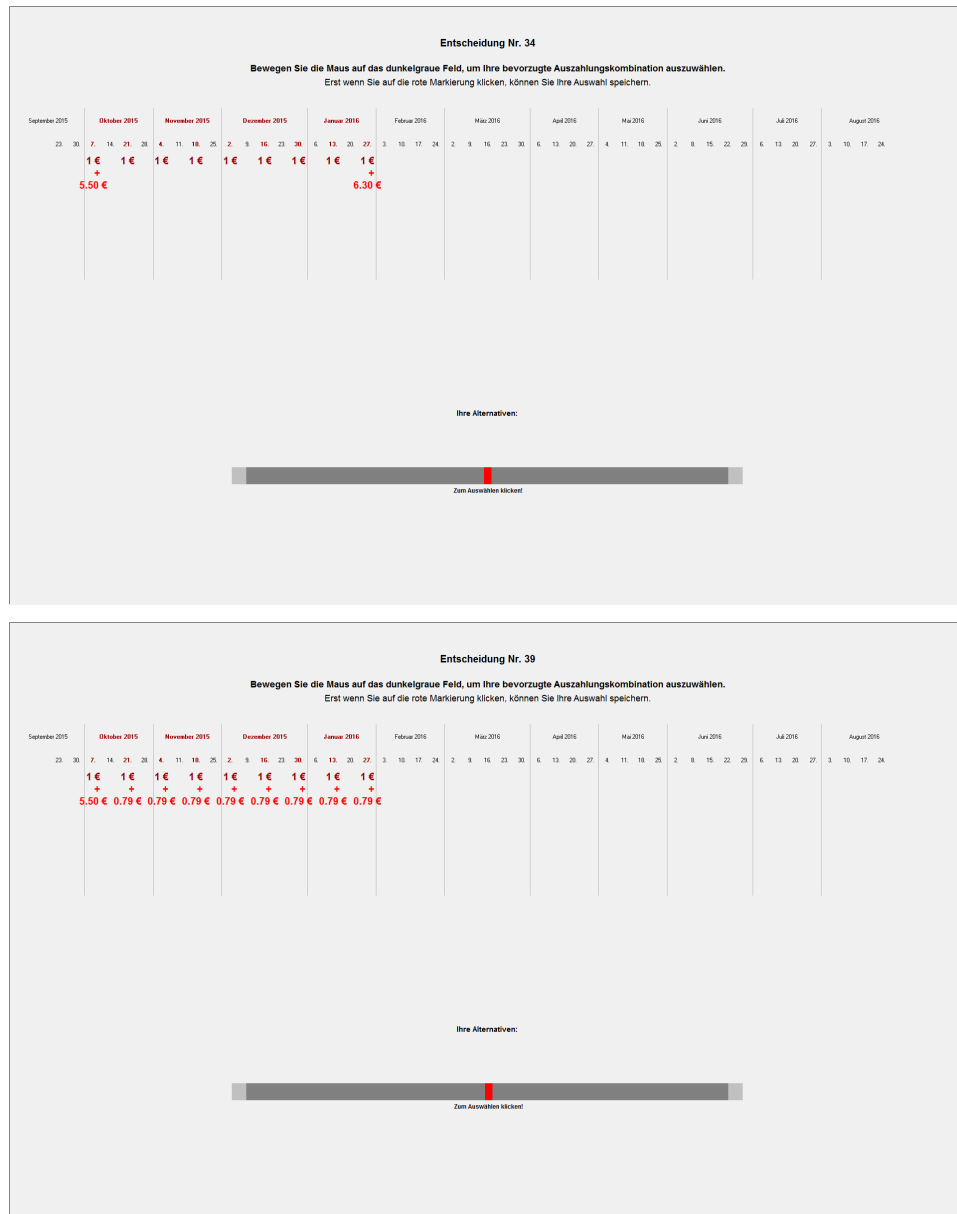
Figure 3.3. Screenshots of a $CONC_a$ and a $DISP_{a8}$ Decision

3.2.1.2 Cognitive Reflection Test and Calculation Task

Concentration bias should be understood as a heuristic-like decision-making tool that differs from deliberate contemplation over the advantages and disadvantages—or benefits and costs—of an action. This suggests the potential for heterogeneity in the degree to which individuals are affected by concentration bias. First, individuals that are less able to control their impulsivity might be more
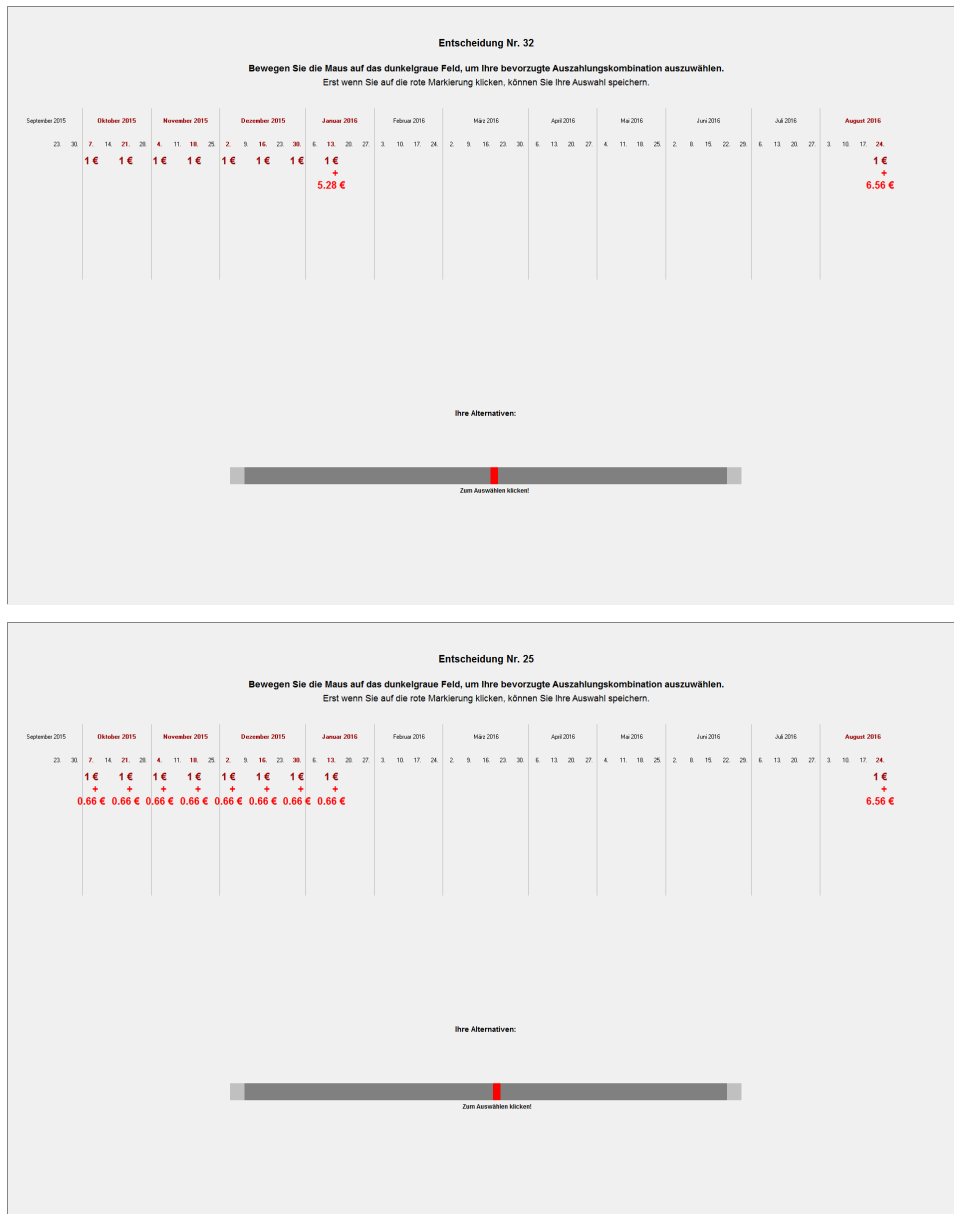
Figure 3.4. Budget Sets: Screenshots of a $CONC_b$ and $DISP_{b8}$ Decision

prone to concentration bias. Second, individuals that are less capable of calculating sums of payoffs might exhibit more pronounced concentration bias. We test for these two sources of potential heterogeneity by letting subjects complete the Cognitive Reflection Test (CRT; Frederick, 2005) and a calculation (mental-arithmetic) task at the end of the experiment.

The CRT measures the degree to which individuals are prone to let their decision making be governed by their impulses rather than deliberate contemplation. We did not incentivize the CRT.

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

3.2.1.3 Procedure

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

In the first wave, each subject made 36 choices across different earnings sequences. One set of subjects ($N = 47$) faced 12 earnings sequences ($w \in \{3, 6\}$; $B = 8$, $r \in \{20\%, 50\%, 80\%\}$; and $B = 11$, $r \in \{15\%, 36\%, 58\%\}$) of each of the types $CONC_a$, $DISP_{a8}$, and $DISP_{a4}$ [$2 \times 2 \times 3 \times 3 = 36$]. A second set of subjects ($N = 46$) faced the same parameters for $CONC_b$, $DISP_{b8}$, and $DISP_{b4}$ earnings sequences. In the second wave, each subject made 32 choices across different earnings sequences: all subjects ($N = 92$) received four earnings sequences ($w \in \{2, 3\}$, $B = 11$, $r \in \{15\%, 58\%\}$) of each of the two CONC and the three respective DISP types [$2 \times 2 \times (2 + 3 + 3) = 32$].

During the first wave, experimental sessions took place on Friday. During the second wave, sessions took place on Wednesday, Thursday, or Fridays. The earliest bank transfer for any earnings sequence was always next week's Wednesday. Thus, subjects' earnings sequences always started 5, 6, or 7 days in the future. Recall that we are interested in the within-subject difference of intertemporal choices between CONC and DISP earnings sequences. Since CONC and DISP earnings sequences always start at the same point in time per subject, the temporal distance between the experiment and the first payment date is irrelevant.

Overall, subjects in both waves were also endowed with 24 (first wave) and 28 (second wave) additional earnings sequences. In this paper, we do not analyze these earnings sequences. The remaining earnings sequences also test concentration bias, but in a different manner. While these remaining earnings sequences yield similarly supportive evidence for concentration bias, they do not

Table 3.1. Set of Earnings Sequences for Each Allocation Technology

$$C_{\text{CONC}_a} = \left[1 + B(1-x), 1, 1, 1, 1, 1, 1, 1, 1 + RBx \right]$$

$$C_{\text{DISP}_{a8}} = \left[1 + B(1-x), 1 + \frac{RBx}{8}, 1 + \frac{RBx}{8}, 1 + \frac{RBx}{8}, 1 + \frac{RBx}{8}, \right.$$
$$\left. 1 + \frac{RBx}{8}, 1 + \frac{RBx}{8}, 1 + \frac{RBx}{8}, 1 + \frac{RBx}{8} \right]$$

$$C_{\text{DISP}_{a4}} = \left[1 + B(1-x), 1, 1, 1, 1, 1 + \frac{RBx}{4}, 1 + \frac{RBx}{4}, 1 + \frac{RBx}{4}, 1 + \frac{RBx}{4} \right]$$

$$C_{\text{DISP}_{a2}} = \left[1 + B(1-x), 1, 1, 1, 1, 1, 1, 1 + \frac{RBx}{2}, 1 + \frac{RBx}{2} \right]$$

$$C_{\text{CONC}_b} = \left[1, 1, 1, 1, 1, 1, 1, 1 + B(1-x), \qquad\qquad 1 + RBx \right]$$

$$C_{\text{DISP}_{b8}} = \left[1 + \frac{B(1-x)}{8}, 1 + \frac{B(1-x)}{8}, 1 + \frac{B(1-x)}{8}, 1 + \frac{B(1-x)}{8}, \right.$$
$$\left. 1 + \frac{B(1-x)}{8}, 1 + \frac{B(1-x)}{8}, 1 + \frac{B(1-x)}{8}, 1 + \frac{B(1-x)}{8}, \qquad 1 + RBx \right]$$

$$C_{\text{DISP}_{b4}} = \left[1, 1, 1, 1, 1 + \frac{B(1-x)}{4}, 1 + \frac{B(1-x)}{4}, 1 + \frac{B(1-x)}{4}, 1 + \frac{B(1-x)}{4}, 1 + RBx \right]$$

$$C_{\text{DISP}_{b2}} = \left[1, 1, 1, 1, 1, 1, 1 + \frac{B(1-x)}{2}, 1 + \frac{B(1-x)}{2}, \qquad\qquad 1 + RBx \right]$$

allow for the robustness check that we report in Section 3.3. We therefore include a more detailed description and our analysis of behavior for these earnings sequences in Section 3.B.

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 et al., 2014) for inviting subjects from the BonnEconLab's subject pool and recording their participation. Prior to their participation, subjects gave informed consent and agreed to providing us with their bank details (this prerequisite was already mentioned in the invitation messages sent out to prospective participants via hroot).

3.2.2 Predictions

In this section, we examine predictions regarding individuals' behavior in the main experiment. We consider two distinct cases: subjects base their allocation decisions on standard time preferences—discounted utility—or subjects are, in addition, affected by concentration bias in the way specified by Kőszegi and Szeidl (2013). Importantly, by discounted utility, we refer to any lifetime utility that is time-separable. In many articles, discounted utility and exponential discounting are treated as synonymous, while other authors use discounted utility

as the generic concept and regard particular types of discounting, such as exponential, hyperbolic, or quasi-hyperbolic, as instances of discounted utility. We use the latter terminology. Importantly, the predictions derived below hold for all three mentioned frequently used types of discounting and not only exponential discounting.

Subjects were endowed with multiple earnings sequences. Each earnings sequence C comprised 9 payments. For each sequence, subjects chose a share x of the early payment(s) to allocate to later payment dates. Between the CONC and DISP conditions, we varied the type of the intertemporal budget constraint (one could also call this the "allocation technology"). Table 3.1 lists C for the different types of intertemporal budget constraints that we implemented. Prior to subjects' allocation decision $x \in \{0, 1/100, 2/100, \ldots, 1\}$, each earnings sequence C is, essentially, a set of 101 earnings sequences. Out of this set, an allocation decision x determines a unique instance c that specifies payments c_t for the payment dates $t = 1, \ldots, 9$. For example, in CONC_a, a choice of $x = 0.5$ implemented the earnings sequence $c = [c_1, \ldots, c_9] = [1 + B/2, 1, 1, 1, 1, 1, 1, 1, 1 + R(B/2)]$.

In the following, we assume that individuals anticipate to consume the payments they receive within the same period. This is an assumption that is frequently made in experiments on intertemporal decision making (see Halevy (2014), for a discussion). Given that the maximum payment was below €20 and that any two periods were separated by at least two weeks, this assumption seems reasonable. Additionally, we make the standard assumption that utility from consumption is increasing in its argument but not convex, i.e., $u'(c_t) \geq 0$ and $u''(c_t) \leq 0$.

3.2.2.1 Discounted Utility

Individuals make their allocation decisions by comparing the aggregated consumption utility of each earnings sequence $c \in C$. Discounted utility assumes that the utility of each period enters overall utility additively. That is, utility derived from the payment to be received at future date t can be expressed as $u_t(c_t) \equiv D(t) u(c_t)$. Here, $D(t)$ denotes the individual's discount function for conversion of future utility into present utility. The discount function satisfies $0 \leq D(t)$ and $D'(t) \leq 0$, such that a payment further in the future is valued at most as much as an equal-sized payment closer in the future.[3]

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

$$U(c) = \sum_{t=1}^{T} u_t(c_t) = \sum_{t=1}^{T} D(t) u(c_t). \tag{3.1}$$

---

[3] Normalization such that $D(t) \leq 1$ is not necessary in our case. Examples of $D(t)$ are $D(t) \equiv \delta^t$ for exponential and $D(t) \equiv (1 + \delta t)^{-\gamma/\delta}$ for hyperbolic discounting.

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

We consider CONC_a and DISP_{a8} first. In CONC_a, individuals decide how much to allocate to the different periods by choosing

$$x^\star_{\text{CONC}_a}(B,R) \equiv$$

$$\arg\max_x D(1)\,u(1 + B(1 - x)) + \sum_{t=2}^{8} D(t)\,u(1) + D(9)\,u(1 + RBx),$$

$$(3.2)$$

and in DISP_{a8} by choosing

$$x^\star_{\text{DISP}_{a8}}(B,R) \equiv \arg\max_x D(1)\,u(1 + B(1 - x)) + \sum_{t=2}^{9} D(t)\,u(1 + RBx\,/\,8).$$

$$(3.3)$$

Since $D'(t) \leq 0$ and $u''(\cdot) \leq 0$—as well as $D(t) \geq 0$, $0 \leq x \leq 1$, $b \geq 0$, $R \geq 1$, and $u'(\cdot) > 0$—the following holds. While the marginal negative consequences of being patient, i.e., of increasing x, are the same across CONC_a and DISP_a, $D(1)\,u'(1 + B(1 - x)) \times (-B)$, the marginal positive consequences are weakly smaller in CONC_a than in DISP_a,

$$D(9)\,u'(1 + RBx) \times Rb \;\leq\; \sum_{t=2}^{9} D(t)\,u'(1 + RBx\,/\,8) \times RB\,/\,8.$$

This effect is driven both by the (weak) concavity of the utility function u and the fact that in DISP_a, parts of the positive consequences occur earlier and are, thus, discounted less. Therefore, individuals allocate to later periods at least as much in DISP_{a8} as in CONC_a. This reasoning applies analogously to comparisons between CONC_a and DISP_{a4} as well as CONC_a and DISP_{a2}. Hence, collectively, we have

$$d^\star_{a,8}(B,R) \;\equiv\; x^\star_{\text{CONC}_a}(B,R) - x^\star_{\text{DISP}_{a8}}(B,R) \;\leq\; 0,$$
$$d^\star_{a,4}(B,R) \;\equiv\; x^\star_{\text{CONC}_a}(B,R) - x^\star_{\text{DISP}_{a4}}(B,R) \;\leq\; 0,$$
$$d^\star_{a,2}(B,R) \;\equiv\; x^\star_{\text{CONC}_a}(B,R) - x^\star_{\text{DISP}_{a2}}(B,R) \;\leq\; 0,$$

with $d^\star_{a,8}(B,R) \leq d^\star_{a,4}(B,R) \leq d^\star_{a,2}(B,R)$.

In the following, let $d^\star_i$ denote the mean of all $d^\star_{i,j}(B,R)$ and x_{DISP_i} the mean of all $x_{\text{DISP}_{ij}}$ for $i \in \{a, b\}$ and $j \in \{2, 4, 8\}$. Thus, discounted utility predicts that on average (across parameters B and R as well as DISP_{a8}, DISP_{a4} and DISP_{a2}

conditions), subjects are at least as patient in DISP_a as in CONC_a, i.e.,

$$d_a^\star \equiv x_{\text{CONC}_a}^\star - x_{\text{DISP}_a}^\star \ \leq \ 0. \tag{3.4}$$

The latest payment that is concentrated in CONC_a is dispersed over 8, 4, and 2 periods in DISP_{a8}, DISP_{a4}, and DISP_{a2}, respectively. Importantly, the latest of the dispersed payments is paid when the latest payment in CONC_a is paid. All other dispersed payments are paid earlier, as illustrated in Figure 3.1. Thus, a large part of the later payment is discounted to a lesser degree than in CONC_a. Therefore, subjects are weakly better off in DISP_a than in CONC_a, if the same amount of money is allocated to later periods. Consequently, subjects allocate at least as much to later periods in DISP_a as in CONC_a.

We consider CONC_b and DISP_{b8} next. In CONC_b, individuals decide how much to save by choosing

$$x_{\text{CONC}_b}^\star \equiv \arg\max_x \sum_{t=1}^{7} D(t)\,u(1) + D(8)\,u(1 + B(1-x)) + D(9)\,u(1 + RBx),$$

and in DISP_{b8}, by choosing

$$x_{\text{DISP}_{b8}}^\star \equiv \arg\max_x \sum_{t=1}^{8} D(t)\,u(1 + (B(1-x))/8) + D(9)\,u(1 + RBx).$$

Here, the following holds. While the marginal positive consequences of postponing, i.e., increasing x, are identical across CONC_b and DISP_b, $D(9)\,u'(1 + RBx) \times b$, the marginal negative consequences are weakly smaller (i.e., greater in absolute terms) in DISP_{b8} than in CONC_b,

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

This effect is, again, driven both by the (weak) concavity of the utility function u and the fact that in DISP_b, parts of the negative consequences occur earlier and are, thus, discounted less. Therefore, individuals save at most as much in DISP_{b8} as in CONC_b. The same holds, analogously, for DISP_{b4} and DISP_{b2}. This implies that

$$d_b^\star \equiv x_{\text{DISP}_b}^\star - x_{\text{CONC}_b}^\star \ \leq \ 0. \tag{3.5}$$

Since dispersion is greatest in DISP_{b8} and least pronounced in DISP_{b2}, we have $d_{b,8}^\star \leq d_{b,4}^\star \leq d_{b,2}^\star \leq 0$.

The second-to-last payment of CONC_b is dispersed over 8, 4, or 2 earlier periods in DISP_{b8}, DISP_{b4}, and DISP_{b2}, respectively, as is illustrated in Figure 3.2.

Thus, a large share of earlier payments is discounted over fewer periods in DISP_b than in CONC_b. This induces subjects to save at most as much in DISP_b as in CONC_b.

3.2.2.2 Concentration Bias

In this section, we extend the model of standard discounted utility by incorporating concentration bias. Concentration bias captures the intuition that concentrated consequences are more attention-grabbing than dispersed consequences. Kőszegi and Szeidl (2013) proposed a particular weighting function g that captures such concentration bias. Period-t weights g_t scale period-t consumption utility u_t. Individuals are modeled to have focus-weighted utility as follows:

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

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

$$g_t \equiv g[\Delta_t(C)] \quad \text{with} \quad \Delta_t(C) \equiv \max_{c' \in C} u_t(c'_t) - \min_{c' \in C} u_t(c'_t).$$

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

We consider the implications of focus weighting on savings decisions in CONC_a and DISP_a first. We will see that the following intuition is captured by including g in the aggregation of consequences: In CONC_a, the positive consequences of being patient are concentrated in the last period and are, therefore, attention-grabbing. By contrast, in DISP_a, the positive consequences of saving are less tangible, as they are dispersed over several periods.

For CONC_a, period-1 utility ranges from $u_1(1)$ to $u_1(1+B)$ ($x = B$ or $x = 0$, respectively), while period-9 utility ranges from $u_9(1)$ to $u_9(1+RB)$. For DISP_a, period-1 utility also ranges from $u_1(1)$ to $u_1(1+B)$. However, period-9 utility now ranges only from $u_9(1)$ to $u_9(1+RB/8)$ in DISP_a8, to $u_9(1+RB/4)$ in DISP_a4, and to $u_9(1+RB/2)$ in DISP_a2. Thus, period-9 utility receives a lower weight in DISP_a than it receives in CONC_a, $g_9^{\text{CONC}_\text{a}} > g_9^{\text{DISP}_\text{a}}$. 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., $g_9^{\text{DISP}_\text{b2}} > g_9^{\text{DISP}_\text{b4}} > g_9^{\text{DISP}_\text{b8}}$. In exchange for this downweighting of u_9, the preceding periods t' ($t' = 2, \ldots, 8$ in DISP_a8, $t' = 6, 7, 8$ in

$\mathrm{DISP}_{\mathrm{a}4}$, and $t' = 8$ in $\mathrm{DISP}_{\mathrm{a}2}$) receive a larger weight $g_{t'}$ in $\mathrm{DISP}_{\mathrm{a}}$ than in $\mathrm{CONC}_{\mathrm{a}}$. This is because for those periods, $\max u_{t'} - \min u_{t'} = 0$ in $\mathrm{CONC}_{\mathrm{a}}$, while it is positive in $\mathrm{DISP}_{\mathrm{a}}$. Importantly, g is strictly increasing. If g is sufficiently steep, then the relatively large weight g_9 in $\mathrm{CONC}_{\mathrm{a}}$ is greater than the sum of the multiple smaller weights $g_{t'}$, including g_9, in $\mathrm{DISP}_{\mathrm{a}}$.

Expressed verbally, the positive consequences of being patient are underweighted in $\mathrm{DISP}_{\mathrm{a}}$ relative to $\mathrm{CONC}_{\mathrm{a}}$. If this relative underweighting of dispersed payoffs in $\mathrm{DISP}_{\mathrm{a}}$ is sufficiently strong, focus-weighted utility predicts larger marginal utility from being patient in $\mathrm{CONC}_{\mathrm{a}}$ than in $\mathrm{DISP}_{\mathrm{a}}$. In that case, the prediction of the standard model—as specified in equation (3.4)—is reversed: focus-weighting individuals may want to save more in $\mathrm{CONC}_{\mathrm{a}}$ than in $\mathrm{DISP}_{\mathrm{a}}$. Let ** indicate optimal choices according to discounted utility in combination with focusing (in analogue to * indicating optimal choices under discounted utility without focus weighting). Then we have, with a sufficiently positive slope of the weighting function g,[4]

$$d_{\mathrm{a}}^{\star\star} \equiv x_{\mathrm{CONC}_{\mathrm{a}}}^{\star\star} - x_{\mathrm{DISP}_{\mathrm{a}}}^{\star\star} > 0 \tag{3.6}$$

as well as $d_{\mathrm{a},8}^{\star\star} \geq d_{\mathrm{a},4}^{\star\star} \geq d_{\mathrm{a},2}^{\star\star} > 0$.

We now turn to the implications of focus-weighted utility on savings decisions in $\mathrm{CONC}_{\mathrm{b}}$ and $\mathrm{DISP}_{\mathrm{b}}$. Recall that in $\mathrm{DISP}_{\mathrm{b}}$, the negative consequences of saving are dispersed over several payment dates, while they are concentrated at a single, thus attention-grabbing, payment date ($t = 8$) in $\mathrm{CONC}_{\mathrm{b}}$. The strictly increasing weighting function g captures this potential neglect of the dispersed payoffs in $\mathrm{DISP}_{\mathrm{b}}$. Period-8 utility ranges from $u_8(1)$ to $u_8(1+B)$ (for $x = B$ and $x = 0$, respectively) in $\mathrm{CONC}_{\mathrm{b}}$. By contrast, it ranges only from $u_8(1)$ to $u_8(1+B/8)$ in $\mathrm{DISP}_{\mathrm{b}8}$, to $u_8(1+B/4)$ in $\mathrm{DISP}_{\mathrm{b}4}$, and to $u_8(1+B/2)$ in $\mathrm{DISP}_{\mathrm{b}2}$. Hence, focus-weighted utility assigns a lower weight to period-8 utility in $\mathrm{DISP}_{\mathrm{b}}$ than in $\mathrm{CONC}_{\mathrm{b}}$. In exchange for this downweighting of u_8, the preceding periods t' ($t' = 1, \ldots, 7$ in $\mathrm{DISP}_{\mathrm{b}8}$, $t' = 5, 6, 7$ in $\mathrm{DISP}_{\mathrm{b}4}$, and $t' = 7$ in $\mathrm{DISP}_{\mathrm{b}2}$) receive a larger weight $g_{t'}$ in $\mathrm{DISP}_{\mathrm{b}}$ than in $\mathrm{CONC}_{\mathrm{b}}$. This is because for those periods, $\max u_{t'} - \min u_{t'} = 0$ in $\mathrm{CONC}_{\mathrm{b}}$, while it is positive in $\mathrm{DISP}_{\mathrm{b}}$. Just as before, if the slope of g is sufficiently positive, then the relatively large weight g_8 in $\mathrm{CONC}_{\mathrm{b}}$ is greater than the sum of the multiple smaller weights $g_{t'}$, including g_8, in $\mathrm{DISP}_{\mathrm{b}}$.

If such underweighting of the utility generated by early payments (up to payment date no. 8) is sufficiently strong in $\mathrm{DISP}_{\mathrm{b}}$, then focus-weighting reverses the prediction of the standard model—as stated in equation (3.5)—by predict-

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

ing that individuals save more in $DISP_b$ than in $CONC_b$:

$$d_b^{\star\star} \equiv x_{DISP_b}^{\star\star} - x_{CONC_b}^{\star\star} > 0, \tag{3.7}$$

and $d_{b,8}^{\star\star} \geq d_{b,4}^{\star\star} \geq d_{b,2}^{\star\star} > 0$.

In the following, we compare savings decisions between $CONC_a$ and $DISP_a$ as well as between $CONC_b$ and $DISP_b$. We hypothesize that concentration bias is sufficiently strong and leads individuals to save more in $CONC_a$ than in $DISP_a$, $d_a^{\star\star} > 0$, as well as more in $DISP_b$ than in $CONC_b$, $d_b^{\star\star} > 0$. Both effects taken together yield the prediction regarding the aggregated concentration bias of $d^{\star\star} > 0$, with $d^{\star\star}$ being the average of $d_a^{\star\star}$ and $d_b^{\star\star}$.

Hypothesis 4. *Subjects allocate more money to payments that are concentrated at a single date than to equal-sized payments that are dispersed over multiple earlier dates, $d^{\star\star} > 0$ (in contrast to standard discounting).*

Define variables $d_j^{\star\star}$ to capture the differences in savings averaged across the different degrees of dispersion: $d_j^{\star\star} \equiv (d_{a,j}^{\star\star} + d_{b,j}^{\star\star})/2$ for $j = 2, 4, 8$.

Hypothesis 5. *The effect described in Hypothesis 4 is the more pronounced, the more dispersed a payment is, i.e., $d_8^{\star\star} > d_4^{\star\star} > d_2^{\star\star} > 0$.*

3.2.3 Results

Subjects made multiple allocation decisions in our experiment. In particular, subjects made several allocation decisions for CONC and DISP earnings sequences. This allows us to calculate for each individual the average difference of money allocated to later periods between CONC and DISP earnings sequences. Denote by $\hat{x}$, $\hat{d}$, etc. the empirical counterparts of the variables introduced in Section 3.2.2 (i.e., of $x^{\star/\star\star}$, $d^{\star/\star\star}$, etc.). That is, $\hat{d}$ is the individual average of $\hat{d}_a$ ($= \hat{x}_{CONC_a} - \hat{x}_{DISP_a}$) and $\hat{d}_b$ ($= \hat{x}_{DISP_b} - \hat{x}_{CONC_b}$).

3.2.3.1 Test of Hypothesis 4

With this, we can report our first result.

Result 7. *On average, subjects allocated more money to payments 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 4. Subjects allocated $\hat{d} = 6.3$ percentage points (p.p.) more money into payments that were concentrated rather than dispersed. This treatment effect is statistically significant, suing a t-test, with standard errors corrected for potential clustering on the subject level (see Ta-

Table 3.2. Testing Concentration Bias, $\hat{d}$, against Zero

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

Notes: Standard errors in parentheses, clustered on the subject level.
$^\star p < 0.10$, $^{\star\star} p < 0.05$, $^{\star\star\star} p < 0.01$.

ble 3.2).[5] This result provides evidence for concentration bias as predicted by Kőszegi and Szeidl (2013).

A closer look at the specific comparisons between CONC_a and DISP_a as well as CONC_b and DISP_b substantiates our first finding. Subjects allocated on average more money into the future in CONC_a than in DISP_a, $\hat{d}_\text{a} = 5.7$ p.p. ($= 9.12\%$), as well as in DISP_b than in CONC_b, $\hat{d}_\text{b} = 6.8$ p.p. ($= 9.65\%$).[6] Both $\hat{d}_\text{a}$ and $\hat{d}_\text{b}$ are significantly larger than zero in a t-test (p-value<0.01).

The results reported in Table 3.3 provide further support. Table 3.3 shows the frequencies of individual values of $\hat{d}_\text{a}$ and $\hat{d}_\text{b}$ being smaller, larger, or equal to zero. A sign-rank test shows that the values of both $\hat{d}_\text{a}$ and $\hat{d}_\text{b}$ are not distributed symmetrically around zero. In both cases, the largest fraction of subjects has positive $\hat{d}_\text{a}$ and $\hat{d}_\text{b}$ values, and there are more than twice as many subjects with positive than with negative $\hat{d}_\text{a}$ and $\hat{d}_\text{b}$ values, respectively.

At the same time, there are seizable fractions of subjects whose $\hat{d}_\text{a}$ and/or $\hat{d}_\text{b}$ values are equal to zero. Let us investigate these subjects' behavior in greater detail. Out of 47 subjects with $\hat{d}_\text{a} = 0$, four subjects chose $\hat{x}_{\text{CONC}_\text{a}} = 0$ so that there was no "room" for them to save even less in the DISP condition, as our Hypothesis 4 predicts. However, for the remaining 43 subjects, there was "room" to save less in the dispersed earnings sequences, i.e., to choose $\hat{x}_{\text{DISP}_\text{a}} < \hat{x}_{\text{CONC}_\text{a}}$ in line with Hypothesis 4—but they did not do so. Thus, for these 43 subjects, concentration bias does not seem to have mattered.

Regarding the second group, the 51 subjects with $\hat{d}_\text{b} = 0$, it turns out that 45 subjects chose $\hat{x}_{\text{CONC}_\text{b}} = 1$. This means that they were already so patient in the CONC_b condition that their behavior may be confined by a ceiling effect: our task simply did not allow them to choose $\hat{x}_{\text{DISP}_\text{b}} > \hat{x}_{\text{CONC}_\text{b}}$, 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.

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[5] This finding is substantiated with a sign-rank test ($p < 0.001$).
[6] $\hat{x}_{\text{CONC}_\text{a}} = 68.3\%$, $\hat{x}_{\text{DISP}_\text{a}} = 62.5\%$, $\hat{x}_{\text{CONC}_\text{b}} = 77.3\%$, and $\hat{x}_{\text{DISP}_\text{b}} = 70.5\%$.

Table 3.3. Frequencies of the Two Measures of Concentration Bias, $\hat{d}_a$ and $\hat{d}_b$, Being Positive, Zero, or Negative

Difference	$\hat{d}_a$	$\hat{d}_b$
Positive	63 (45%)	59 (43%)
Zero	47 (34%)	51 (37%)
Negative	29 (21%)	28 (20%)
N	139	138

3.2.3.2 Test of Hypothesis 5

Let us now turn to the question whether the degree of dispersion influences subjects' choices, i.e., to testing Hypothesis 5.

Result 8. *Our measure of concentration bias is the greater, the more dispersed payments in the DISP condition are, i.e., $\hat{d}_8 > \hat{d}_4 > \hat{d}_2 > 0$.*

Our second result provides evidence in support of Hypothesis 5. We find that the degree of concentration bias that subjects exhibit depends on the degree to which the dispersed payment is actually dispersed. Our measure of concentration bias is $\hat{d}_8 = 8.10$ p.p. for 8 payment dates; it is $\hat{d}_4 = 6.56$ p.p. for 4 payment dates and $\hat{d}_2 = 3.67$ p.p. for 2 payment dates. All three treatment effects are significantly larger than zero according to both t-tests and sign-rank tests ($p < 0.001$ for $\hat{d}_8$ and $\hat{d}_4$; $p < 0.05$ for $\hat{d}_2$ in both tests). Moreover, concentration bias in the case that payments were dispersed over 4 or 8 payment dates is significantly greater than when payments were dispersed over on 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 larger than for 4 payment dates ($p = 0.237$).

3.2.3.3 Heterogeneity

The model of focusing can be considered as a formalization of a rule of thumb which people use because, for some reason, they cannot evaluate the whole situation correctly. One possible reason for applying a heuristic way of thinking is that people are not cognitively able to evaluate all involved consequences and therefore focus on some attention-grabbing concentrated payoffs. Alternatively, they simply do not have the arithmetic skills to aggregate the monetary payoffs correctly.

Table 3.3 shows that there is substantial heterogeneity in the degree of concentration bias between subjects. It is conceivable that this heterogeneity is related to heterogeneity in cognitive abilities and/or impulsivity. We therefore try

Table 3.4. Regression of the Measure of Concentration Bias, $\hat{d}$, on a Measure of Mathematical Ability and CRT Scores

	(1)	(2)
Dependent variable	$\hat{d}$	$\hat{d}$
Math score $\leq$ median	0.033	
	(0.021)	
CRT score $\leq$ median		0.023
		(0.021)
Constant	0.048***	0.053***
	(0.014)	(0.014)
Observations	277	277
Subjects	185	185
R^2	0.009	0.004

Notes: Standard errors in parentheses, clustered on the subject level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

to measure subjects' abilities that might be related to such effects by assessing math skills via an incentivized mental-arithmetic task. In this task, subjects were given five minutes time to calculate as many sums as they could of decimal numbers. These sums were of a similar kind as the monetary payments presented to subjects in the main experiment. The median subject calculated six sums correctly.

Moreover, individuals who are less able to control their impulsivity might be more prone to concentration bias. To assess cognitive reflection, we use the CRT from Frederick (2005) as a measure of impulsiveness.

For both measures, we perform a median split and set the associated dummy variable to one for all subjects below the median. We regress our measure of concentration bias, $\hat{d}$, on the dummy for math or CRT. As also evident from Table 3.4, we do not find any significant influence of these two measures of heterogeneity on concentration bias.

3.3 Robustness

We have shown above evidence for a bias toward concentrated payments in intertemporal choice that is at odds with standard discounting but can be explained by the focusing model of Kőszegi and Szeidl (2013). In this section, we analyze potential alternative explanations of the observed behavior that resemble but are not identical to Kőszegi–Szeidl-type focusing.

These alternative drivers of behavior would have to rely on one of the two characteristics in which the dispersed and concentrated payments differ from

each other: First, it might be the case that subjects are averse against small payments. For instance, subjects may have a distaste against receiving lots of change (i.e., small coins) rather than bills. While a distaste against small payments should not matter in the context of our experiment as payments are conducted though bank transfers, we cannot rule out that subjects feel this way. Second, the computational complexity of evaluating an earnings sequence is greater when it includes dispersed and not only concentrated payments. This greater computational complexity may cause errors in subjects' calculations. White-noise errors would only result in inconsistent choices but not in a preference for concentrated payments. However, it is conceivable for our task that these mathematical errors are systematic in the direction of "subadditivity"—meaning that people calculate the sum of the dispersed payments to be too small.[7] As a consequence, subjects would undervalue dispersed payments vis-à-vis concentrated sequences.

To control for these two potential effects, we made use of a control experiment in which all dispersed payments were "dispersed within a day" and not over different payment dates. Recall that the last bank transfer of a dispersed payment in $DISP_a$ and $DISP_b$ is always completed at the same date as the respective concentrated payment in $CONC_a$ and $CONC_b$. In our "dispersed within a day" control experiment, we therefore mirrored the combined features of the DISP and CONC conditions: we made the dispersed payments de facto identical to the concentrated ones, by scheduling all "dispersed" payments on the date of the concentrated payment. In other words, the "dispersed within a day" payments are completely equivalent to the concentrated payments except the difference in the display: subjects saw 2, 4, or 8 relatively small monetary amounts that they would have to add to calculate the total earnings that they would receive at that date. Figure 3.5 displays a screenshot of the graphical representation that was shown to subjects who participated in this control experiment.

Overall, subjects in the control experiment made the same amount of allocation decisions as subjects in the main experiment did. The only difference between these decisions was that in the main experiment DISP-type allocation decisions concern dispersion over multiple payment dates, while in the control experiment DISP-type allocation decisions concern dispersion within a payment date, as explained above. Thus, we can calculated the same average difference of money allocated to "concentrated"—or not dispersed—payments, i.e., $\hat{d}$, for subjects in the control experiment as we did for subjects of the main experiment.

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[7] There is evidence that people suffer from "left-digit bias"—a "tendency to focus on the leftmost digit of a number while partially ignoring other digits"—even in situations in which they have monetary incentives to overcome such bias (Lacetera et al., 2012). A left-digit bias could lead to undervaluation of dispersed compared to concentrated payoffs in our experiment. One might argue that left-digit bias is a potential "micro-foundation" of Kőszegi and Szeidl's (2013) focus weights; however, one might also view left-digit bias and resulting undervaluation of a sum as a mathematical error that should be ruled out as an explanation.
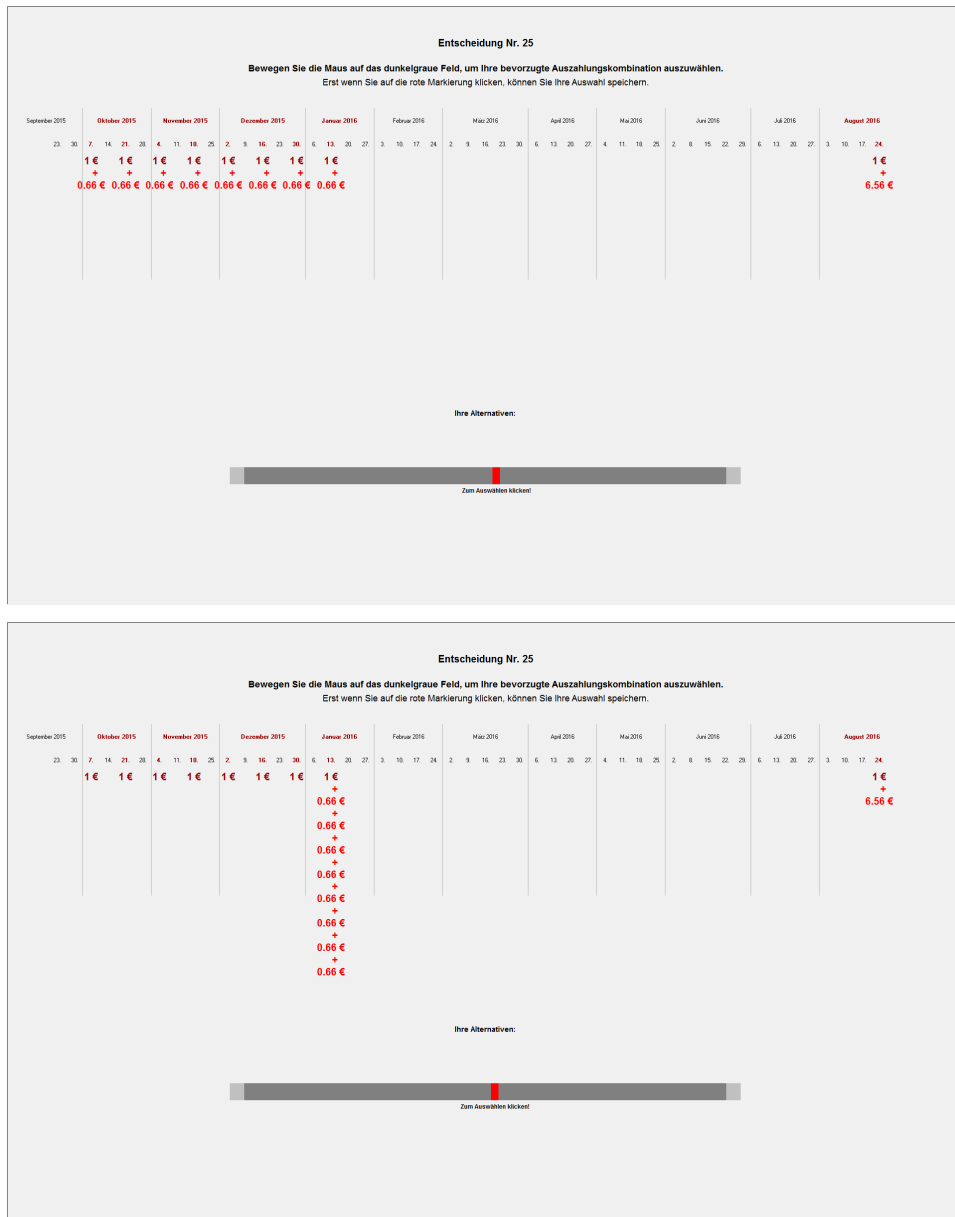
Figure 3.5. Budget Sets: Screenshots of a DISP$_{b8}$ Condition in the Main (top) and in the Respective Condition in the Control (bottom) Experiment

While $\hat{d}$ measures concentration bias in the main experiment, it measures small payments aversion and computational complexity in the control experiment. In case our estimated measure of $\hat{d}$ is statistically larger in the main rather than in the control experiment, then this would imply that the evidence for concentration bias in the main experiment cannot be explained by small numbers aversion or computational complexity.

Table 3.5. Difference-in-Difference Analysis of Concentration Bias, $\hat{d}$, in the Main Experiment (Dispersed over Time) vis-à-vis the Control Experiment (Dispersed within a Day)

Dependent variables	$\hat{d}$
Dispersed within a day	-0.036***
	(0.013)
Constant	0.063***
	(0.011)
Observations	562
Subjects	374
R^2	0.016

Notes: Standard errors in parentheses, clustered on the subject level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

We compare $\hat{d}$ between our main and control experiments in an OLS regression. This comparison is between subjects and involves overall 374 subjects of which 185 participated in the main experiment and 189 participated in the control experiment.[8]

To compare the main experiment with the control experiment, we regress $\hat{d}$ on a dummy variable that takes on the value 1 for all subjects who participated in the ("dispersed within a day") control experiment instead of the main experiment.[9] Hence, the (negative of the) coefficient on this dummy measures to what extent merely splitting up a larger monetary payment into the sum of multiple small payments can generate concentration bias. As Table 3.5 shows, the coefficient on this control dummy is negative and significant on the 1% level. That is, concentration bias in the main experiment cannot be explained by small numbers aversion or computational complexity.

Merely splitting up a larger payment into several smaller ones thus cannot explain the concentration bias that we observed in our main experiment. This is additional evidence for the model by Kőszegi and Szeidl (2013), since according to this model, the dispersion of consequences *over time* is crucial in generating concentration bias.

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[8] Except for the first three sessions, the main and control experiments were conducted during the same sessions. In these latter sessions, participants of our study were randomly assigned either to the main or control experiment.

[9] We have up to two values for the dependent variable per subject, depending on whether a subject participated in both the $CONC_a$ and $DISP_a$ as well as the $DISP_b$ and $CONC_b$ conditions or only one of the two. Consequently, we cluster standard errors on the subject level.

3.4 Conclusion

This paper is the first to provide causal evidence for a bias toward concentrated payoffs in intertemporal choice, as it is predicted by Kőszegi and Szeidl (2013). Building on the "convex budget sets" method of Andreoni and Sprenger (2012), we designed a novel choice task that implements different types of intertemporal budget constraints. More specifically, both earlier and later benefits in this task take on the form of either one-time payments or payments that are dispersed over several payments dates. We used this choice task in a laboratory experiment to test how spreading payments over time influences people's intertemporal decisions. We find that the payments' degree of dispersion influences subjects' choices in a way that is incompatible with discounted utility but in line with concentration bias, as the Kőszegi–Szeidl model predicts. Our findings are relevant not only for positive economics, i.e., for understanding and forecasting people's behavior, but also for normative economics, as we argue below.

To academic economists as well as politicians and entrepreneurs, how people make intertemporal decisions is of great interest. For instance, how people's savings respond to changes in interest rates or how their health decisions respond to tax incentives is relevant for both the insurance sector and the government. The model most widely used for analyzing intertemporal decisions is discounted utility in combination with exponential discounting. However, people's decisions often seem to contradict exponential discounting. One example is low gym attendance (DellaVigna and Malmendier, 2006). A different example—with huge monetary stakes—is the "annuity puzzle" (see, e.g., Yaari, 1965; Warner and Pleeter, 2001; Davidoff et al., 2005; Benartzi et al., 2011). It describes the phenomenon that many people choose an earlier lump-sum payment over a future rent that is paid periodically (the annuity) even when the rent has a substantially higher expected present value. In fact, many other decisions from everyday life are similar in that available options are also characterized by payoffs or costs that are dispersed over time—such as the benefits of not smoking or payment plans for smartphones.

Deviations from exponential discounting have frequently been interpreted as resulting from present bias, i.e., as stemming from a lack of self-control, which can be formalized via (quasi-)hyperbolic discounting (see, e.g., Laibson, 1997). Recently, Kőszegi and Szeidl (2013) have offered an alternative explanation of the departures from exponential discounting that occur in such decisions: a bias of decision makers toward options whose consequences are concentrated in fewer periods. Unfortunately, based on empirical data from the field alone, it will be hard or even impossible to discriminate between the competing explanations. More concretely, the "annuity puzzle" (Benartzi et al., 2011) could be the product of Kőszegi–Szeidl-type focusing. However, other factors, such as uninsured medical expenses, bequest motives, and adverse selection (see the dis-

cussions in Modigliani, 1986; Davidoff et al., 2005; Benartzi et al., 2011), may also, at least partially, explain the empirically observed low degree of annuitization. Moreover, the predictions of hyperbolic discounting and the Kőszegi–Szeidl model coincide regarding under-annuitization.

By providing a controlled environment in which particular motives are ruled out or at least held constant, our lab experiment allows for establishing that the dispersion of consequences indeed causally affects discounting. Our variant of the "convex budget set" method (Andreoni and Sprenger, 2012) implements both concentrated and dispersed payments. In other words, we implement different types of intertemporal budget constraints (different types of "investment products"/"savings technologies"). For paying subjects, we use forward-dated money transfers to their bank accounts. In the concentrated conditions ($CONC_a$ and $CONC_b$), each payment is transferred on a single day. In contrast, in the associated dispersed conditions ($DISP_a$ and $DISP_b$), parts of the payments are dispersed over several days (importantly, these are always earlier than the associated CONC payments): in $DISP_a$, later payments are dispersed, while in $DISP_b$, earlier payments are dispersed.

Concentration bias, as modeled by Kőszegi and Szeidl (2013), predicts that subjects allocate more money to future payments (save more) in $CONC_a$ than in $DISP_a$. Importantly, this prediction is not made by discounted-utility models (i.e., neither by exponential nor by hyperbolic discounting). Indeed, we find that subjects exhibit concentration bias. Thus, they violate predictions of discounted-utility models, while their decisions are compatible with the focusing model by Kőszegi and Szeidl (2013). Via a control treatment, we show that merely splitting up a larger payment into several smaller ones cannot explain the concentration bias that we observed in our main experiment. This is crucial because according to Kőszegi and Szeidl (2013), it is the dispersion of consequences *over time* that generates a bias toward concentration.

Our study contributes to the literature on intertemporal choice in two important ways: First, it helps explain why in recent experiments, the observed degree of present bias and the incidence of time-inconsistent behavior are fairly low (Andreoni and Sprenger, 2012; Augenblick et al., 2015), while they are found to be much more severe in the analysis of field data (e.g., low gym attendance, DellaVigna and Malmendier (2006); resistance to the annuitization of pension plans, Warner and Pleeter (2001) and Davidoff et al. (2005)). Our study does so by identifying the lack of dispersed payments in previous lab experiments as a plausible source of this discrepancy. Second, and more importantly, we find that the dispersion of payments influences subjects' decisions in a way captured neither by exponential nor by (quasi-)hyperbolic discounting.

Hence, not only previous experimental studies but also theoretical accounts of discounting, except Kőszegi and Szeidl (2013), seem to have neglected an

important facet of intertemporal choice. At the same time, our findings are also evidence against other recent approaches such as the "model of relative thinking" by Bushong et al. (2015), since relative thinking predicts effects in the opposite direction of the effects predicted by focusing.

We are aware of one previous study that used multiple bank transfers to remunerate subjects: Attema et al. (2015). Attema et al. propose a highly elegant method of "measuring discounting without measuring utility." Crucially, however, their method relies, first, on the assumption that lifetime utility is time-separable and, second, on measuring indifference between payment streams that consist of dispersed payments.[10] However, the focusing model predicts a violation of time separability if at least one of the functions u or g is nonlinear. Furthermore, also without any reference to the focusing model, our results indicate limitations to Attema et al.'s approach. Given that we find that the degree of an option's dispersion affects expressed discounting, it is likely that the estimates obtained by applying Attema et al.'s method will be sensitive to the exact payment streams employed, i.e., to both the size of the payments and their degree of dispersion.

Outside the laboratory, concentration bias is essential information, for instance, regarding the assessment of the effectiveness of policy measures. It is conceivable, for instance, that taxes on annuities are perceived as less severe by people than taxes on lump-sum payments or on current income. Related to this, Chetty et al. (2009) have shown that taxes of different salience affect consumer demand to different degrees, and they also assess the consequences of this effect for consumer welfare. Thus, understanding how bias toward concentration affects people's intertemporal choices is equally important for both positive and normative economics.

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

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Appendix 3.A   Instructions

Main and Control Experiment[11]

Screen 1—Welcome

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

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

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

Screen 2—Information about the Procedure

Part 1

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

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

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

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

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

Part 2

In the second part of the experiment, we would like to ask you to exercise a task. You will receive money for doing this task. We will provide information about the exact payment right before the beginning of the second part. Your payment

---

[11]While the text of the instructions for the main and control experiment are the same, the income sequences displayed on the respective screens are different, as discussed in Section 3.3.

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

Here subjects have to enter their banking data.

Screen 4—Choice Lists[12]

Part 1a

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

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

Screen 5—Example 1

In this example, the first alternative has been chosen. The slider is positioned in a way that payment-alternative 1 is displayed. In this example, payment-alternative 1 corresponds to a payment of €8 at the earliest possible date. Additionally, €1 is transferred to your bank account at nine different dates.

Screen 6—Example 2

In this example, the sixth alternative has been chosen. The slider is positioned in a way that payment-alternative 6 is displayed. In this example, payment-alternative 6 corresponds to a multiple payment of €1.50 at each highlighted date. Additionally, €1 is transferred to your bank account at nine different dates.

Screen 7—Example 3

You can choose your preferred option out of nine alternatives. All alternatives distinguish themselves in the total amount of money and the points in time where transfers are realised. The following is always the case: If you choose a later payment, you receive, in total, more money than choosing an earlier payment.

At the next screen, all nine payment-alternatives of this decision are shown in an animation.

The transfer dates are highlighted in red.

---

[12]We analyze the intertemporal decisions with respect to the the choice lists in Section 3.B.

After the animation you have the possibility to have another look at all payment-alternatives and you can choose your most preferred alternative.

This hint will be shown for the first four decisions.

Screen 8—Budget Sets[13]

In part 1b you have to make 36 decisions.

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

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

You can practice the use of the slider:

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

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

Screen 9—End of Part 1

This was the last decision of the first part of the experiment.

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

Please click on the button "Continue."

Screen 10—Part 2

In this part we would like to ask you to add up figures as often as you can manage.

You have 5 minutes time for exercising this task.

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

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

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

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

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[13]We analyze the intertemporal decisions with respect to the the budget sets in main text of this paper.

Screen 11

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

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

Screen 12

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

Screen 13—CRT 1

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

Screen 14—CRT 2

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

Screen 15—CRT 3

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

Appendix 3.B   Choice Lists

To show that our findings are not specific for one single method, we use choice lists as an additional robustness check for Hypothesis 4. Each subject was endowed with 24 choice lists with nine options $j = 1, \ldots, 9$.[14] Subjects faced three different types of choice lists as illustrated in Figures 3.C.1, 3.C.2 and 3.C.3 CONC$_{CL}$ with one concentrated payment for each option j, DISP$_{aCL}$ with an increasing degree of dispersion of the payment and DISP$_{bCL}$ with a decreasing degree of dispersion. We varied time w (in weeks) between two periods, budget B (in €) in the first period and interest i (in €) additionally paid when picking choice j instead of $j-1$. In the first wave, each of the ($N = 93$) subjects faced 8 earnings sequences ($w \in \{3, 6\}$; $B \in \{8, 11\}$; $i \in \{0.2, 0.5\}$) for CONC$_{CL}$,

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[14] As in the budget sets, each option included nine money transfers $t = 1, \ldots, 9$ to subjects' bank account with €1 fixed payment at each date plus an additional amount of money, depending on the option chosen.

$DISP_{aCL}$ and $DISP_{bCL}$[15]. In the second wave, all ($N = 92$) subjects made 8 decisions ($w \in \{2, 3\}$; $B \in \{8, 11\}$; $i \in \{0.2, 0.8\}$) for $CONC_{CL}$ and $DISP_{aCL}$ and another 8 decisions ($w \in \{4, 6\}$; $B \in \{8, 11\}$; $i \in \{0.2, 0.8\}$) for $DISP_{bCL}$. As above, we compare within-subject average choice between $CONC_{CL}$ and $DISP_{aCL}$, and $CONC_{CL}$ and $DISP_{bCL}$, respectively. We consider a choice of a higher option j as more patient.

Predictions

Discounted Utility

We start with the predictions for standard discounting utility. Individuals compare utilities $U(c)$ of each option c within a choice list C and pick the option $c^\star$ with the highest utility. We examine the comparison between $CONC_{CL}$ and $DISP_{aCL}$ first. In $CONC_{CL}$, individuals pick the option:

$$c^\star_{CONC_{CL}}(B, i) \equiv$$

$$\arg\max_j \sum_{t=1, \neq j}^{9} D(t)\, u(1) + D(j)\, u(1 + B + ji), \qquad (3.B.1)$$

and in $DISP_{aCL}$:

$$c^\star_{DISP_{aCL}}(B, i) \equiv$$

$$\arg\max_j \sum_{t=1}^{j} D(t)\, u(1 + \frac{B + (j-1)i}{j}) + \sum_{t=j+1}^{9} D(t)\, u(1). \qquad (3.B.2)$$

When comparing utilities between $CONC_{CL}$ and $DISP_{aCL}$ for some specific option j, utility for the latter one is always higher due to the (weak) concavity of the utility function u and a lower degree of discounting for a big part of the

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

dispersed payments:

$$\sum_{t=1,\neq j}^{9} D(t)\,u(1) + D(j)\,u(1+B+ji)$$

$$\leq \quad \sum_{t=1}^{j} D(t)\,u(1+\frac{B+(j-1)i}{j}) + \sum_{t=j+1}^{9} D(t)\,u(1)$$

$$\Longleftrightarrow \quad \sum_{t=1}^{j-1} D(t)\,u(1) + D(j)\,u(1+B+ji)$$

$$\leq \quad \sum_{t=1}^{j} D(t)\,u(1+\frac{B+(j-1)i}{j}) \tag{3.B.3}$$

As a consequence, individuals are (weakly) better off in DISP_{aCL} and choose an at least as patient option as in CONC_{CL}, i.e.,

$$d^{\star}_{1,CL} \equiv c^{\star}_{\text{CONC}_{\text{CL}}} - c^{\star}_{\text{DISP}_{\text{aCL}}} \leq 0. \tag{3.B.4}$$

We consider CONC_{CL} and DISP_{bCL} next. Here we use the same concentrated treatment as benchmark as above. Optimal choice for DISP_{bCL} is:

$$c^{\star}_{\text{DISP}_{\text{bCL}}}(B,i) \equiv$$

$$\arg\max_{j} \sum_{t=1}^{j-1} D(t)\,u(1) + \sum_{t=j}^{9} D(t)\,u(1+\frac{B+(j-1)i}{9-(j-1)}) \tag{3.B.5}$$

When comparing utility between CONC_{CL} and DISP_{bCL} for some option j, the (weak) concavity of the utility function u makes the individual in the dispersed case better off, but, at the same time, payments occur later and are discounted more strongly. To weaken the second motive, we doubled the time between consecutive payment dates in DISP_{bCL}. We show in Appendix 3.D that under exponential discounting and linear utility, individuals should be more patient in CONC_{CL} than in DISP_{bCL}:

$$d^{\star}_{2,CL} \equiv c^{\star}_{\text{DISP}_{\text{bCL}}} - c^{\star}_{\text{CONC}_{\text{CL}}} \leq 0. \tag{3.B.6}$$

Concentration Bias

When investigating predictions of Kőszegi and Szeidl's concentration bias, note that each choice list consists of nine possible options which span the utility range for each period. For CONC_{CL}, minimum possible utility is always $u_t(1)$ whereas maximum possible utility for period t is $u_t(1+B+ji)$ which corresponds to option $j=t$. Hence, $\Delta_t(C)$ differs from $u_1(1+B)-u_1(1)$ to $u_9(1+B+8i)-$

$u_9(1)$. Whereas for DISP_{aCL}, $\Delta_t(C)$ differs from $u_1(1+B) - u_1(1 + \frac{B+8i}{9})$ to $u_9(1 + \frac{B+8i}{9}) - u_9(1)$. As one can see, the relative weighting of the last period is higher in CONC_{CL}, i.e., $g_9^{\text{CONC}_{\text{CL}}} / g_1^{\text{CONC}_{\text{CL}}} > g_9^{\text{DISP}_{\text{aCL}}} / g_1^{\text{DISP}_{\text{aCL}}}$. If this under-weighting is sufficiently strong, focus-weighted utility predicts a less patient choice in DISP_{aCL} than in CONC_{CL} and one gets

$$d_{1,CL}^{\star\star} \equiv c_{\text{CONC}_{\text{CL}}}^{\star\star} - c_{\text{DISP}_{\text{aCL}}}^{\star\star} > 0 \tag{3.B.7}$$

We study the implications of focus-weighted utility on the comparison between CONC_{CL} and DISP_{bCL} next. For DISP_{bCL} —opposite to the increasing dispersion— is the utility-range of the first period much smaller than for the last period: $u_1(1 + B/9) - u_1(1)$ versus $u_9(1 + B + 8i) - u_9(1 + B/9)$. That results in a stronger relative overweighting of the last period for DISP_{bCL}, i.e., $g_9^{\text{DISP}_{\text{aCL}}} / g_1^{\text{DISP}_{\text{aCL}}} > g_9^{\text{CONC}_{\text{CL}}} / g_1^{\text{CONC}_{\text{CL}}}$. Here, if this underweighting is sufficiently strong, focus-weighted utility predicts a more patient choice in DISP_{bCL} than in CONC_{CL}:

$$d_{2,CL}^{\star\star} \equiv d_{\text{DISP}_{\text{bCL}}}^{\star\star} - d_{\text{CONC}_{\text{CL}}}^{\star\star} > 0, \tag{3.B.8}$$

Results

Subjects made multiple allocation decisions in our experiment. In particular, they made 8 allocation decisions for each of the three different treatments, CONC_{CL}, DISP_{aCL} and DISP_{bCL}. This allows us to calculate for each individual the average difference of choices between CONC and DISP earnings sequences. Denote by $\hat{c}$, $\hat{d}$ the empirical counterparts of the variables introduced above.

We find supportive evidence in both directions for Hypothesis 4. Subjects chose a lower option of $\hat{d}_{1,CL} = 0.573$ in DISP_{aCL}, compared to CONC_{CL}, and a higher option of $\hat{d}_{2,CL} = 0.933$ in DISP_{bCL} than in CONC_{CL}. These treatment effects are statistically significant in both a t-test and sign-rank test ($p < 0.001$ according to both tests).

Appendix 3.C Choice Lists: Schematic Illustrations

	w weeks					w weeks		
$c = 1$:	1 $+$ B	1	1	1	1	1	1	1	1
$c = 2$:	1	1 $+$ $B + i$	1	1	1	1	1	1	1
$c = 3$:	1	1	1 $+$ $B + 2i$	1	1	1	1	1	1
$c = 4$:	1	1	1	1 $+$ $B + 3i$	1	1	1	1	1
$c = 5$:	1	1	1	1	1 $+$ $B + 4i$	1	1	1	1
$c = 6$:	1	1	1	1	1	1 $+$ $B + 5i$	1	1	1
$c = 7$:	1	1	1	1	1	1	1 $+$ $B + 6i$	1	1
$c = 8$:	1	1	1	1	1	1	1	1 $+$ $B + 7i$	1
$c = 9$:	1	1	1	1	1	1	1	1	1 $+$ $B + 8i$
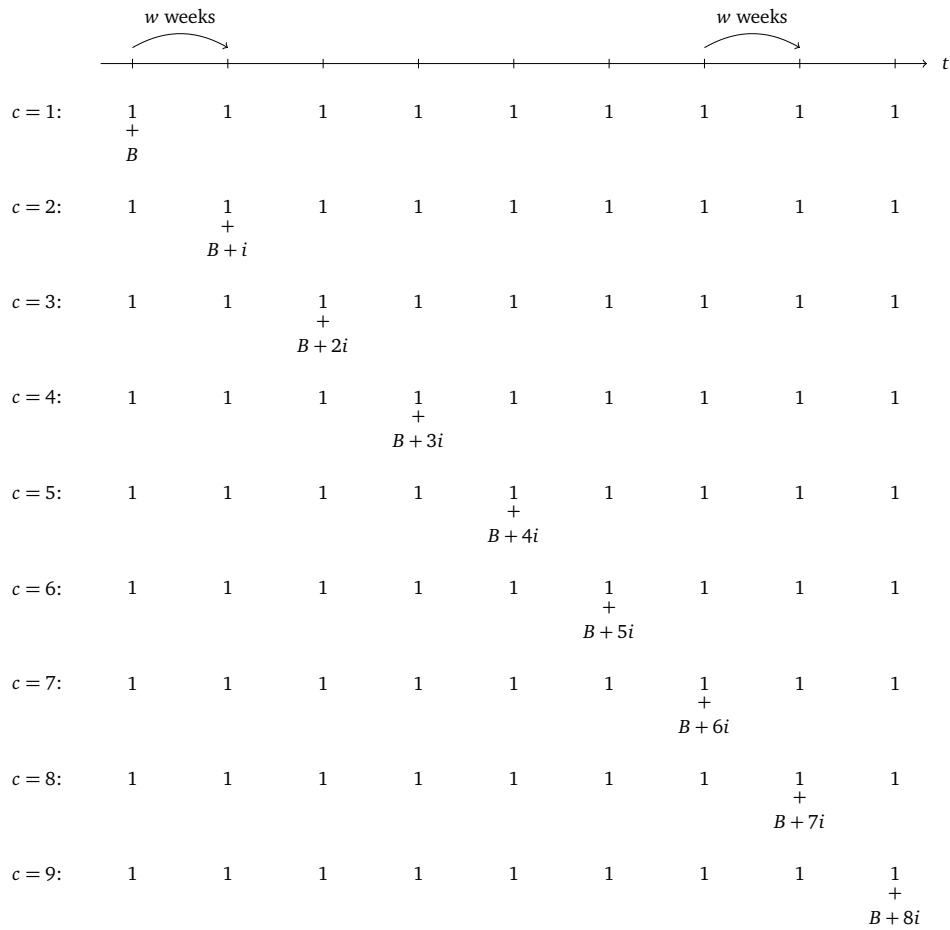
Figure 3.C.1. Choice Lists: CONC_{CL} Conditions

Note: For the values of B, i, and w that we used, see Section 3.B.

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

$\xrightarrow{\hspace{10cm}} t$

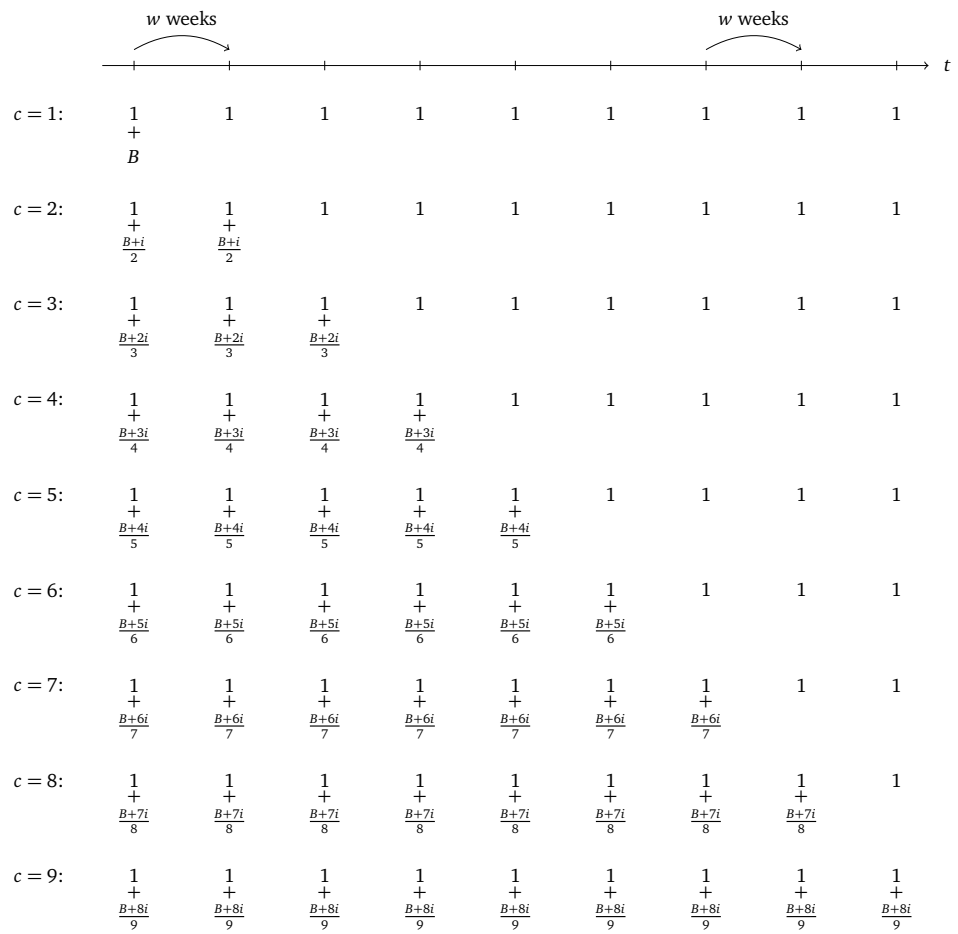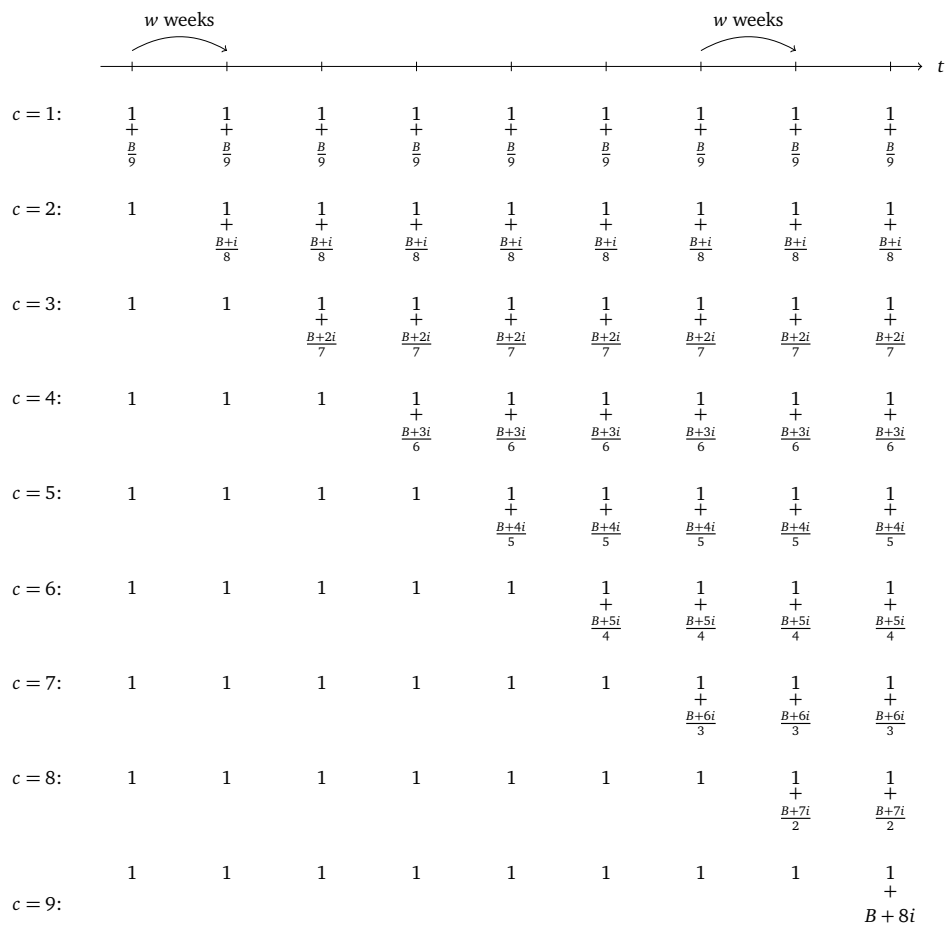|								
$c = 1$:	$1+B$	1	1	1	1	1	1	1	1
$c = 2$:	$1+\frac{B+i}{2}$	$1+\frac{B+i}{2}$	1	1	1	1	1	1	1
$c = 3$:	$1+\frac{B+2i}{3}$	$1+\frac{B+2i}{3}$	$1+\frac{B+2i}{3}$	1	1	1	1	1	1
$c = 4$:	$1+\frac{B+3i}{4}$	$1+\frac{B+3i}{4}$	$1+\frac{B+3i}{4}$	$1+\frac{B+3i}{4}$	1	1	1	1	1
$c = 5$:	$1+\frac{B+4i}{5}$	$1+\frac{B+4i}{5}$	$1+\frac{B+4i}{5}$	$1+\frac{B+4i}{5}$	$1+\frac{B+4i}{5}$	1	1	1	1
$c = 6$:	$1+\frac{B+5i}{6}$	$1+\frac{B+5i}{6}$	$1+\frac{B+5i}{6}$	$1+\frac{B+5i}{6}$	$1+\frac{B+5i}{6}$	$1+\frac{B+5i}{6}$	1	1	1
$c = 7$:	$1+\frac{B+6i}{7}$	$1+\frac{B+6i}{7}$	$1+\frac{B+6i}{7}$	$1+\frac{B+6i}{7}$	$1+\frac{B+6i}{7}$	$1+\frac{B+6i}{7}$	$1+\frac{B+6i}{7}$	1	1
$c = 8$:	$1+\frac{B+7i}{8}$	$1+\frac{B+7i}{8}$	$1+\frac{B+7i}{8}$	$1+\frac{B+7i}{8}$	$1+\frac{B+7i}{8}$	$1+\frac{B+7i}{8}$	$1+\frac{B+7i}{8}$	$1+\frac{B+7i}{8}$	1
$c = 9$:	$1+\frac{B+8i}{9}$	$1+\frac{B+8i}{9}$	$1+\frac{B+8i}{9}$	$1+\frac{B+8i}{9}$	$1+\frac{B+8i}{9}$	$1+\frac{B+8i}{9}$	$1+\frac{B+8i}{9}$	$1+\frac{B+8i}{9}$	$1+\frac{B+8i}{9}$

Figure 3.C.2. Choice Lists: DISP$_{\text{aCL}}$ Conditions

Note: For the values of B, i, and w that we used, see Section 3.B.

	w weeks					w weeks			t
$c=1$:	$1+\dfrac{B}{9}$	$1+\dfrac{B}{9}$	$1+\dfrac{B}{9}$	$1+\dfrac{B}{9}$	$1+\dfrac{B}{9}$	$1+\dfrac{B}{9}$	$1+\dfrac{B}{9}$	$1+\dfrac{B}{9}$	$1+\dfrac{B}{9}$
$c=2$:	1	$1+\dfrac{B+i}{8}$	$1+\dfrac{B+i}{8}$	$1+\dfrac{B+i}{8}$	$1+\dfrac{B+i}{8}$	$1+\dfrac{B+i}{8}$	$1+\dfrac{B+i}{8}$	$1+\dfrac{B+i}{8}$	$1+\dfrac{B+i}{8}$
$c=3$:	1	1	$1+\dfrac{B+2i}{7}$	$1+\dfrac{B+2i}{7}$	$1+\dfrac{B+2i}{7}$	$1+\dfrac{B+2i}{7}$	$1+\dfrac{B+2i}{7}$	$1+\dfrac{B+2i}{7}$	$1+\dfrac{B+2i}{7}$
$c=4$:	1	1	1	$1+\dfrac{B+3i}{6}$	$1+\dfrac{B+3i}{6}$	$1+\dfrac{B+3i}{6}$	$1+\dfrac{B+3i}{6}$	$1+\dfrac{B+3i}{6}$	$1+\dfrac{B+3i}{6}$
$c=5$:	1	1	1	1	$1+\dfrac{B+4i}{5}$	$1+\dfrac{B+4i}{5}$	$1+\dfrac{B+4i}{5}$	$1+\dfrac{B+4i}{5}$	$1+\dfrac{B+4i}{5}$
$c=6$:	1	1	1	1	1	$1+\dfrac{B+5i}{4}$	$1+\dfrac{B+5i}{4}$	$1+\dfrac{B+5i}{4}$	$1+\dfrac{B+5i}{4}$
$c=7$:	1	1	1	1	1	1	$1+\dfrac{B+6i}{3}$	$1+\dfrac{B+6i}{3}$	$1+\dfrac{B+6i}{3}$
$c=8$:	1	1	1	1	1	1	1	$1+\dfrac{B+7i}{2}$	$1+\dfrac{B+7i}{2}$
$c=9$:	1	1	1	1	1	1	1	1	$1+B+8i$

Figure 3.C.3. Choice Lists: DISP$_{\text{bCL}}$ Conditions

Note: For the values of B, i, and w that we used, see Section 3.B.

Appendix 3.D Choice Lists: Comparison between CONC$_b$ and DISP$_b$

We look at people whose behavior can be described with exponential discounting. We intend to show that those who are patient in the long decreasingly dispersed treatment must be patient in the short concentrated treatment, too. For instance, if someone chooses slider position n in the first treatment, he should choose at least n in the latter one. We use linear utility and show (numerically) that there is a bigger interval of possible δs such that option n is chosen over option $n-1$ for the short concentrated treatment than the long decreasingly dispersed treatment.

Concentrated (short)

Choosing slider position n over $n-1$ amounts to:

$$\delta^n(b + r) > \delta^{n-1}b \iff \delta(b + r) > b \iff \delta > \frac{b}{b + r} =: \delta_{conc},$$

where b is the received payoff in period $n-1$ and r the interest payment accruing between the two payment dates.

Decreasingly dispersed (long)

Choosing slider position n over $n-1$ amounts to ($n \geq 2$):

$$\sum_{i=n-1}^{9-1} (\delta^2)^i \frac{b + r}{9 - (n - 1)} > \sum_{i=n-2}^{9-1} (\delta^2)^i \frac{b}{9 - ((n - 1) - 1)}$$

$$\iff \frac{b + r}{b} \frac{9 - ((n - 1) - 1)}{9 - (n - 1)} > \frac{\sum_{i=n-2}^{8}(\delta^2)^i}{\sum_{i=n-1}^{8}(\delta^2)^i}$$

$$= \frac{((\delta^2)^{n-2} - (\delta^2)^9)/(1 - \delta^2)}{((\delta^2)^{n-1} - (\delta^2)^9)/(1 - \delta^2)}$$

$$\iff \frac{b + r}{b} \frac{11 - n}{10 - n} > \frac{((\delta^2)^{n-2} - (\delta^2)^9)}{((\delta^2)^{n-1} - (\delta^2)^9)}.$$

4

How Stable Is Trust?
The Case of Personal Experience
of Unfair and Fair Treatment[*]

Joint with Florian Zimmermann

4.1 Introduction

Trust is a pervasive feature of human relationships. In economics, trust constitutes a social lubricant for any kind of transactions when contracts are incomplete or too costly to be enforced (Arrow, 1974). In particular, trust allows the realization of (efficiency) gains from trade and cooperation. However, it requires individuals to make themselves and their resources vulnerable to exploitation by others.

Ample evidence suggests that trust fosters aggregate social and economic outcomes (see, e.g., Putnam, 1995; La Porta et al., 1997; Knack and Keefer, 1997; Guiso et al., 2004). Understanding how individuals' willingness to trust can be encouraged hence poses important challenges for the social sciences and has potentially far-reaching implications for policy and workplace design. Whether individuals trust others is largely determined by an interplay of the institutional setting—capturing the incentives and constraints that individuals face—and individual primitives—i.e., beliefs and preferences.[1]

Traditionally, economic research has focused primarily on institutional factors. In the case of improving trust, institutions do so by facilitating reputational

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[1] This is not to say that institutions and individual primitives are unrelated entities (see, e.g., Greif, 1994).

concerns, for instance, through feedback mechanisms and competition (see, e.g., Camerer and Weigelt, 1988; Bolton et al., 2004; Huck et al., 2012).

In general, studying the evolution of beliefs and preferences of individuals has recently received increasing interest within economic research, as accumulating evidence suggests substantial heterogeneity of individual primitives within institutional settings.[2] An important first step in shedding light on the long term evolution of trust is provided by Dohmen et al. (2012), who document the transmission of trust attitudes from parents (and local peers) to children through both genetic predisposition and socialization (see also Nunn and Wantchekon, 2011).

However, surprisingly little is known about the short-term malleability of individuals' willingness to trust others (beyond institutional forces). In this paper, we study exactly this. We investigate potential nonstandard spillover effects of social interactions on trust.[3] More precisely, we test whether trust is affected by prior personal experiences of unfair and fair treatment caused by an unrelated third party. Our approach to study the malleability of trust through such exogenous spillover effects is motivated by the following two observations. First, individual behavior is embedded in a constant flux of social interactions that potentially cause personal experiences of unfair and fair treatment. Second, personal experiences are often powerful and particularly meaningful events to individuals, with the consequence that when "people go through experiences, frequently their loyalties, or their values, change" (Akerlof, 1983).[4]

We designed a laboratory experiment that allows us to provide causal evidence on the question whether trust behavior is fostered or mitigated by prior experiences of unfair and fair treatment. We measured trust behavior by employing a variant of Berg et al.'s (1995) trust game (TG).[5] We implemented the personal experience of unfair and fair treatment by conducting a production dictator game (DG) ahead of the TG. The recipients of the DG were the first movers of the TG. Dictator subjects either shared or did not share money with their ran-

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[2] For instance, Falk et al. (2015) show substantial within-country heterogeneity in preferences (and trust attitudes) that is greater than the corresponding between-country heterogeneity, based on a globally representative dataset.

[3] Nonstandard means that standard economic models, e.g., based on rational learning, would not predict these spillover effects.

[4] Suggestive evidence for how profound the influence of personal experiences potentially is, is provided by Malmendier and Nagel (2011) and Giuliano and Spilimbergo (2014), who show that the historical macroeconomic environment affects individuals stock market participation and preferences for redistribution even decades later.

[5] Variants of this trust game have been at the center of a vast literature showing that trust behavior is reducible to certain economic primitives: beliefs regarding the trustworthiness of the involved parties (Costa-Gomes et al., 2014); preferences with respect to "social risk taking," e.g., risk and betrayal aversion (Bohnet and Zeckhauser, 2004); and preferences with respect to the outcomes and anticipated actions of others (Cox, 2004; Ashraf et al., 2006). See, for instance, Fehr (2009) for an overview on trust.

domly matched first movers. First movers hence experienced either unfair or fair treatment before they entered the TG. This allows us to cleanly identify the effect of unfair versus fair treatment on their willingness to trust. Importantly, every first mover knew the frequency with which all first movers overall got paid or did not get paid. This allows us to rule out rational learning from belief updating as an explanation for a potential treatment effect.

We find a strong spillover effect of fair versus unfair treatment on trust behavior. Subjects who experienced fair treatment in the DG, prior to the TG, showed a greater willingness to trust on the intensive and extensive margin than subjects who experienced unfair treatment. More precisely, first movers entrusted more than twice as much of their endowment after a fair than an unfair treatment. This treatment effect is statistically significant and cannot be explained by income effects or income-related mood and disappointment effects, as we show through a control experiment.

Our results suggest that heterogeneity in experiences of fair and unfair treatment may lead to differences in individuals' willingness to trust (within institutional settings). They also imply that in designing policies and workplaces, one should pay attention to spillover effects of social interactions in order to improve short-term trust. While it remains to be shown whether and how long such spillover effects last, we conjecture that an accumulation of experiencing fair or unfair treatment may generate substantial differences in individuals' long-term trust.

The remainder of this paper is organized as follows. We present the experimental design in Section 4.2 and show the empirical results of the experiments in Section 4.3. Section 4.4 concludes with a discussion of our results.

4.2   Experimental Design

We designed the main experiment to allow for a precise measurement of subjects' willingness to trust following an experience of unfair or fair treatment. Between two experimental conditions, UF and F, we exogenously varied whether subjects, who conducted a real-effort task, were paid for this task by a peer subject (F) or were not paid (UF). Despite the fact that subjects knew that 50% got paid for the real-effort task and the remaining 50% were not paid, those who actually got paid for their effort provision experienced fair treatment, while receiving no payment constituted an experience of unfair treatment. A between-subject comparison of subjects' willingness to trust after they were paid versus not paid allows us to identify the effect of experiencing fair versus unfair treatment on trust.

Additionally, we designed a control experiment to measure subjects' willingness to trust following an experience of bad luck versus luck. Subjects in the con-

trol experiment also conducted a real-effort task, but here, whether they were paid or not was determined by a coin toss. Thus, some subjects were paid (experimental condition L) and some subjects were not paid (BL). A between-subject comparison of subjects' willingness to trust in the control experiment allows us to identify potential income effects or income-related mood and disappointment effects on trust.

Our main focus is to compare the treatment effect of the main experiment to the control experiment. This difference-in-difference approach tests whether the experience of fair versus unfair treatment exceeds potential income effects or income-related mood and disappointment effects.

4.2.1 Main Experiment

We randomly assigned subjects to distinct roles—dictator, first mover, and second mover—into groups of three. In the first stage of the experiment, the dictator subjects and first-mover subjects participated in a production dictation game. In the second stage, the first and second movers participated in a trust game. Only the first movers participated in both games. We call these subjects first movers because of their role in the trust game. In the production dictator game, however, they were assigned the receiver role. The dictator and second movers did not know about the game they did not participate in. The first-mover subjects were made aware of this. They knew that no dictator subject could also be a second-mover subject. This allows us to investigate spillover effects of unrelated social interactions on trust rather than revenge motives.

4.2.1.1 Production Dictator Game Stage

In the production DG, the dictator subjects and the first-mover subjects were paired randomly in groups of two. Within each group, both subjects worked on a real-effort task. The real-effort task required subjects to type multiple combinations of letters and numbers, e.g., Ldh24tHuixY5Th21o7FzTT35, into the keyboard. Subjects had as much times as they needed to correctly type 10 different combinations. Completing the real-effort task—which was achieved by every subject—generated €5, respectively, that were stored in a shared virtual account. The dictator subjects could choose to keep the entire amount of money in the account (€10) for themselves or split it with the first mover. Depending on the dictator subjects' decision, the first movers either received €5 or received no payment at all.[6]

Importantly, after the first-mover subjects were told whether they received €5 or not, they learned the frequency with which first-mover subjects received

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[6] Note that we did not ask first-mover subjects whether they wanted to complete the real-effort task.

the €5 payment or not: in 50% of all cases, first-mover subjects received the €5 payment and in 50% of all cases, first-mover subjects received no payment. In case a first mover received €5 (no payment), she knew that she belonged to the fortunate (unfortunate) half of the first-mover subjects.

By telling first movers about the relative frequencies with which first movers received €5 or not, we designed the experience of fair and unfair treatment to contain the same information about the distribution of "selfish" and "unselfish" subjects in the pool. Rational first movers who experienced fair treatment cannot hold the belief that it is more likely to encounter "unselfish" subjects later on in the experiment than rational first movers who experienced unfair treatment. First movers in both experimental conditions knew that 50% of all them received €5 and 50% did not. Therefore, any difference in behavior following the experience of fair versus unfair treatment cannot be explained by rational learning from the respective personal experiences. In that sense, our treatment manipulation allows us to focus on the effect of a personal experience of unfair versus fair treatment.

To be able to provide first movers with the information on the relative frequency with which first movers received the €5 or not, we did the following. Dictator subjects took part in the production dictator game at an earlier date than the first movers. Thus, when the first movers participated in the production dictator game, all dictator decisions were already documented—allowing us to administer them to the first movers.

4.2.1.2   Trust Game Stage

In the TG, first and second movers were paired randomly in groups of two. Within each group, both first- and second-mover subjects were endowed with €5. In a sequential setup, the first-mover subjects first sent any amount of money between €0 and €5 (in 10-cent intervals) to their respective second-mover subject. The amount received by the second movers was doubled. Second movers then decided how much money they would like to send back. The second mover could send back any amount out of the sum of their endowment and the doubled amount sent to them by the first movers. For instance, if a first mover send €5, the second mover could sent back any amount between €0 and €15. In case a first mover send 50 cents, the second mover could send back any amount between €0 and €6.

The amount sent by the first movers measures their willingness to trust their second movers. This entrusted amount will be behavioral outcome of interest in the empirical anaylsis in Section 4.3.

4.2.1.3 Experiencing Fair and Unfair Treatment

We combine the production dictator game and the trust game to test whether the experience of fair versus unfair treatment affects how much first movers trust second movers. Thereby, we label receiving no money in the production dictator game as an experience of unfair treatment. Both the dictator and the first mover had to work equally hard in the production game, making equal sharing of the produced €10 the salient fairness benchmark. First movers that do not receive their fair share of the produced €10 experience this as being treated unfairly by their dictator subjects, as has been found by numerous previous studies on the (production) DG (for instance, Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Konow, 2000; Falk and Fischbacher, 2006; Cappelen et al., 2007; Andreoni and Bernheim, 2009; Krupka and Weber, 2013).

4.2.2 Control Experiment

The control experiment differed from the main experiment only in one respect. The first stage was not a production DG but a real-effort task in which a coin toss determined whether first movers were paid €5 (L condition or "Luck" condition) or not paid (BL or "Bad Luck" condition) for their provided effort. Thus, instead having a dictator decide whether first movers were paid for the real-effort task, first movers in the control experiment were paid based on pure luck. Essentially, first movers played a production DG with the computer instead of a dictator. After first movers in the control experiment experienced luck or bad luck, they moved on to the TG and were randomly matched with second-mover subjects.[7]

By removing nothing more than the origin of the choice that determined whether first movers got paid, we kept the potential for income effects and income-related mood or disappointment effects constant between experiments. After all, first movers provided the same effort in the main and control experiment and in both experiments did only half of the subjects receive a payment for the exerted effort. A comparison of treatment effects on trust between experiments, therefore, allows us to identify the net effect of experiencing an unfair versus fair treatment.

4.2.3 Procedure

Both experiments were conducted at the BonnEconLab of the University of Bonn in spring (main experiment) and summer (control experiment) of 2014. Both experiments in this paper were computerised using softwares z-Tree, ORSEE, and BoXS (Fischbacher (2007) and Greiner (2004), and Seithe (2012)). In total, 258 subjects participated in the main experiment (96 dictators, 96 first movers and

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[7] See Section 4.2.1.2 for a description of the TG.
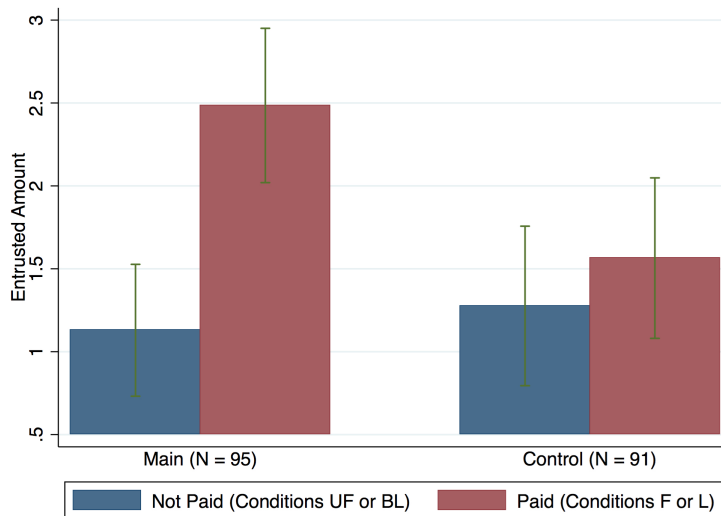
Figure 4.1. Average of Entrusted Amounts per Experiments and Conditions

96 second movers), and 182 subjects participated in the control experiment (91 first movers and 91 second movers). Because of software malfunction, data are missing for a single first mover from the trust game of the main experiment. Average earnings were €7.50 for dictator subjects, €4.50 for first movers and €7.10 for second movers. First movers earned on average less than their endowment. This finding is in line with what other studies find (see, for instance Camerer, 2003; Ashraf et al., 2006).

4.3 Results

In the following, we focus the amounts of money entrusted by the first movers.[8] Figure 4.1 shows the average entrusted amounts per experiments and conditions and reveals a substantial treatment effect on trust in the main experiment and an, at most, small treatment effect in the control experiment. In Section 4.3.1, we show that the treatment effect on trust is statistically significant in the main experiment and present a second finding regarding the extensive margin of entrusting money. In Secton 4.3.2, we continue with the findings of the control experiment, present a difference-in-difference analysis that tests whether the treatment effect on trust is larger in the main relative to the control experi-

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[8] We do not discuss the second movers' choices in this paper, since our primary interest is how the fair versus unfair treatment experiences affect first movers' behavior. As the average payoff for first movers is below their endowment of €5, see Section 4.2.3, sending money to second movers does not pay off on average in our TGs. This finding is in line with what other studies on TGs find (see, for instance, Camerer (2003), and Ashraf et al. (2006)).
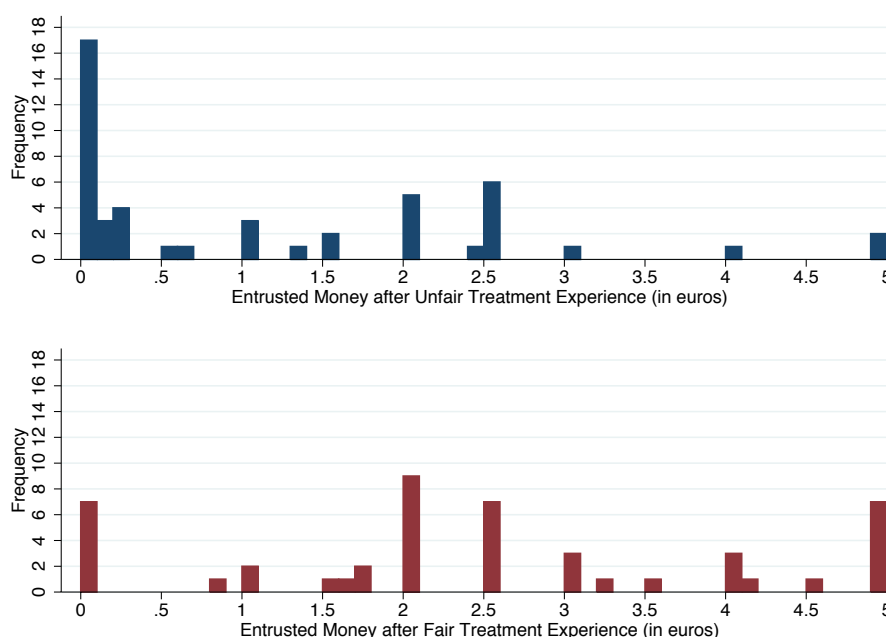
Figure 4.2. Frequencies of Entrusted Amounts in the Main Experiment per Condition

ment, and discuss whether the main treatment effect is driven by fair or unfair treatment experiences.

4.3.1 Results of the Main Experiment

First-moving subjects who were paid by their dictator subjects for the real-effort task sent on average approximately half of their endowment (€2.49) to the second mover in the trust game, while first-moving subjects that were not paid sent less than a quarter of their endowment (€1.13). The difference in the entrusted amount of money is €1.36, which represents a substantial marginal effect of 120% when comparing the experience of unfair treatment to the experience of fair treatment. This treatment effect on trust is statistically significant in an OLS regression (see Table 4.1, Column 1 and Row 1).

Result 9. *First movers sent on average more than twice as much money to their respective second mover after experiencing fair treatment rather than unfair treatment.*

Figure 4.2 shows the histograms of amounts of money entrusted by first movers. The upper panel shows the histogram for the UF condition, and the lower panel shows the histogram for the F condition. A comparison of the two

Table 4.1. Comparing Treatment Effects on Trust between Main and Control Experiment

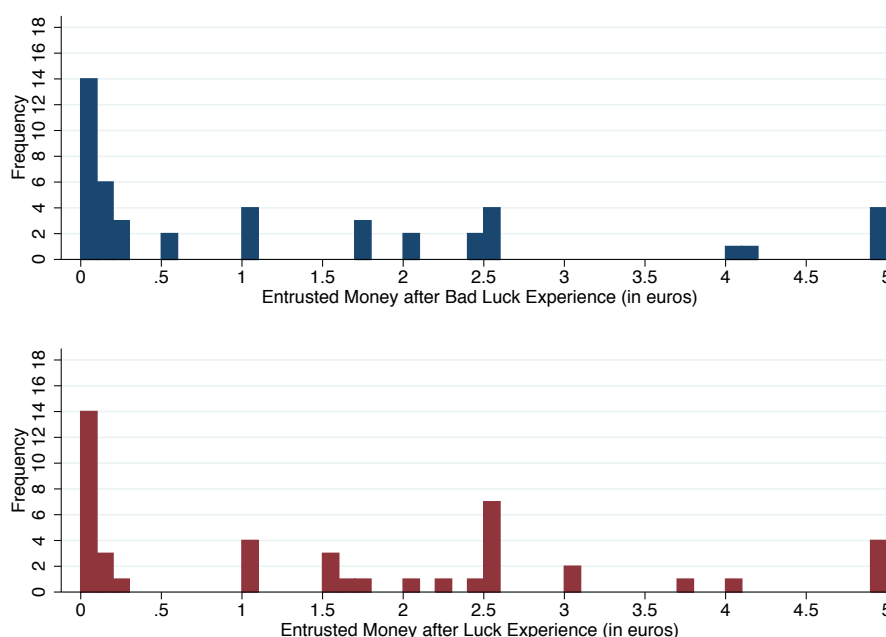|	OLS: Entrusted Amount (1)	Logit: Entrusted Amount > 0 (2)
1 if Not Paid	-1.36^{***}	-1.14^{**}
	(0.30)	(0.51)
1 if Control Experiment	-0.92^{***}	-0.95^{*}
	(0.34)	(0.52)
1 if Not Paid and Control	1.07^{**}	1.17^{*}
	(0.46)	(0.68)
Constant	2.49^{***}	1.74^{***}
	(0.27)	(0.41)
Observations	186	186
Adj./Pseudo R^2	0.09	0.03

Notes: In Column 1, we regress the entrusted amount on a condition dummy ($= 1$ for UF or BL), an experiment dummy ($= 1$ for the control experiment) and an interaction variable between the two dummies. In Column 2, a logistic regression on a different dependent variable—whether the entrusted amount was larger than €0 ($= 1$) or not ($= 0$)—repeats the analyses of Column 1. We state (robust) standard errors in parentheses. Significant at the 1 (5) [10] percent level: *** (**) [*].

histograms provides further support for Result 9. While only 7 first movers did not send any money and only 9 subjects sent less than €1 to their respective second movers after experiencing fair treatment, 17 first movers did not send any money and 26 subjects sent less than €1 after experiencing unfair treatment. The likelihood that first movers sent a positive amount of money is larger in the F condition rather than the UF condition. This treatment effect is statistically significant in a logistic regression (see Table 4.1, Column 2 and Row 1).

Result 10. *The likelihood of first movers sending a positive amount of money to their respective second mover is significantly larger after experiencing fair rather than unfair treatment.*

4.3.2 Main versus Control Experiment

We now turn to the control experiment. Figure 4.3 reveals that first movers sent fairly similar amounts of money to their respective second movers in both L and BL conditions. On average, unlucky first movers (condition BL) sent €0.29 less than lucky first movers (condition L). This treatment effect is not statistically significant in an OLS regression ($p < 0.397$) and significantly smaller relative to the treatment effect of the main experiment (see Table 4.1, Column 1 and Row

Figure 4.3. Frequencies of Entrusted Amounts in the Control Experiment per Condition

3). Figure 4.3 also reveals that the likelihood that first movers sent a positive amount of money is similar between L-BL conditions in the control experiment. The treatment effect on the likelihood to send positive amounts in the main experiment is significantly (albeit weakly) larger than in the control experiment (see Table 4.1, Column 2 and Row 3).

Result 11. *The treatment effects addressed in Results 9 and 10 are significantly more pronounced in the main experiment relative to the control experiment.*

Figure 4.1 and a comparison of Figures 4.2 and 4.3 alludes to the conclusion that the fair treatment experience drives the treatment effect in the main experiment rather than the unfair treatment experience. Unfairly treated first movers seem to behave similarly to first movers from the control experiment. Since we ran the two experiments at two different points in time, we cannot rule out that this conclusion merely results from a level shift in trust do to some temporal shock on students trust.

4.4 Discussion

Our results show a substantial effect of personally experiencing unfair versus fair treatment on subjects' willingness to trust others. Result 11 reveals that this treatment effect on trust cannot be explained by income effects or income-related mood and disappointment effects. Additionally, we can rule out rational learning explanations, as we told first movers in the main experiment about the relative frequencies of dictator behavior before they entered the TG (see Section 4.2.1.1).

Based on these findings, policy makers and workplace designers who are interested in promoting trust should keep spillover effects from unrelated personal experiences in mind. By encouraging fairness between individuals, trust may be fostered as a welcomed side effect and virtuous circles may be initiated.[9]

Personal experiences of fair versus unfair treatment may affect individuals' trust because of the following reasons. First, individuals overweight personally experienced states of the world. In particular, fairly treated first movers may overweight—relative to the rational benchmark—the likelihood of being fairly treated again in the near future. Second, indirect reciprocity urges individuals to "respond in kind" to others based on previous interactions with someone else. In the experiment, fairly treated first movers may want to respond indirectly in kind to dictator behavior by offering the second mover efficiency gains from the TG.

Our design does not allow us to differentiate between the two accounts in terms of which one is driving our results. This remains to be an interesting question for future research.

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[9] However, one should be cautionary, as trust may be promoted in situations that lead to exploitation. In our TG, for instance, first movers who experienced fair treatment earned 54 Cents on average less than first movers who experienced unfair treatment.

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Appendix 4.A   Instructions

Main experiment, first mover, F [UF] condition

Production Dictator Game

This experiment consists of two parts. Your payments for part 1 and part 2 are independent of each other. You get paid in cash at the end of the experiment.

You participate with a different subject in each part of the experiment.

A participant is randomly assigned from a group of participants to participate with you in part 1. Your participant only participated in part 1 and has no knowledge regarding part 2. Your participant took part in part 1 at an earlier date and is not present in the laboratory today.

A participant from another group of participants is randomly assigned to participate with you in part 2. Your participant in part 2 participated only in part 2 and has no knowledge regarding part 1. Your participant in part 2 took part in part 2 at an earlier date and is not present in the laboratory today.

Please note that: First, the groups from which your first and your second participant are randomly chosen from are not identical. No member from one group is also a member in the other group. Second, you will never know anything about your two participants and your identify is not revealed to anyone in this experiment.

[next screen]

Part 1

Your task and the task of your first participant is to type combinations of numbers and letters into the keyboard.

For instance: Ldh24tHuixY5Th21o7FzTT35

You will see combinations of numbers and letters on your screen. Type these numbers and letters one by one into the entry field below and do this case-sensitive. After you have typed a combination into the keyboard, press the "continue"-button. After you have typed a combination into the keyboard and pressed the button, a new combination appears.
[next screen]

For you:

You receive 25 cents for any correctly typed combination. You need to accumulate €2.50 and you have as much time as you need to do so. You cannot accumulate more or less than €2.50. The experiment will not continue unless you have accumulated €2.50.

For your participant:

Your participant also accumulated €2.50, not more and not less.

Shared account:

Both amounts of money are stored in a shared account. The conductors of the experiment double this amount such that €10 are stored in your joint account.
[next screen]

Your payment:

As explained above, €10 are on your joint account.

Your participant will decide how the €10 are shared between the two of you. Your participant can allocate the money fairly such that both of you receive €5. Your participant can also allocate the money unfairly such that both your participant receives the entire €10 receive.

As explained above, your participant already took part in this experiment at an earlier date. Thus, your participant has already decided how to allocate the €10. Your participate has already decided whether you receive €5 or whether you receive no payment at all.

Comprehension question: Which amount of money cannot be earned by you in part 1? €0 or €2.50 or €5.00.

[next screen]

Before you will be made aware that you do or do not receive €5, we would like you to accumulate €2.50 in the typing-task.
[next screen]

typing-task
[next screen]

Thank you for accumulating €2.50. Your participant has also accumulated €2.50. Additionally, the conductors of the experiment added another €5 to your joint account such that €10 are on your joint account.

You will be informed how much money your participant shared with you on the next screen.

[next screen]

Your participant has allocated the €10 such that you receive €5 [0]. Therefore, your payoff for part 1 is €5 [0].

Please notice that your participate was randomly assigned to you from a group of potential participants. In this group of potential participants 48 out of 96 shared the €10 evenly and 48 out of 96 kept the entire €10 for themselves.

Trust Game

[next screen]

In part 2 you and your participant receive two different roles: You are the sender. Your second participant is the re-sender. The both of you receive an endowment of €5. Part 2 has two stages.

Stage 1:

In stage 1 you can send an amount of money to the re-sender. You can send any amount of money between €0 and €5 in steps of 10 cents. You can send €0, 10 cents, 20 cents, 30 cents, . . . , 90 cents, €1, . . . , €4.80, €4.90, €5. The amount of money that you send will be doubled by the conductors of the experiment. For instance, if you send €2.40, then the re-sender will receive €4.80, and if you send €0, your re-sender receives €0.

Stage 2:

In stage 2 the re-sender is asked to send back an amount of money to you. This amount is doubled. The re-sender chooses an amount between €0 and the sum of his endowment and the doubled amount that you send in the first stage.

Payments:

You will receive: 5 — Amount that you send to the re-sender + Amount that the re-sender sends back to you.
The re-sender receives: $5 + 2 \times$ Amount that you send to the re-sender — Amount that the re-sender sends back to you.

Example:

Consider the case that you send €2.40, then the re-sender received €4.80. The re-sender can send an amount of money between €0 and €9.80 back to you. For instance, the re-sender could send €3.60 back to you such that the overall amounts is shared fairly between the two of you which means that you both receive €6.20.

However, the re-sender could also behave unfairly and not send any amount of money back to you. In this case you would receive 5 — 2.40 = €2.60 and the re-sender would receive €9.80.

Please notice that if you do not send any money to the re-sender, your payment for part 2 of the experiment will be €5.
[next screen]

Which amount of money do you want to send to the re-sender?

Control experiment, first mover, L [BL] condition

Production Lottery

This experiment consists of two parts. Your payments for part 1 and part 2 are independent of each other. You get paid in cash at the end of the experiment.

You participate with a different subject in part 2 of the experiment.

A participant from another group of participants is randomly assigned to participate with you in part 2. Your participant in part 2 participated only in part 2 and has no knowledge regarding part 1. Your participant in part 2 took part in part 2 at an earlier date and is not present in the laboratory today.

Please note that: You will never know anything about your participant and your identify is not revealed to anyone in this experiment.

[next screen]

Part 1

Your task is to type combinations of numbers and letters into the keyboard.

For instance: Ldh24tHuixY5Th21o7FzTT35

You will see combinations of numbers and letters on your screen. Type these numbers and letters one by one into the entry field below and do this case-sensitive. After you have typed a combination into the keyboard, press the "continue"-button. After you have typed a combination into the keyboard and pressed the button, a new combination appears.
[next screen]

For you:

You receive 25 cents for any correctly typed combination. You need to accumulate €2.50 and you have as much time as you need to do so. You cannot accumulate more or less than €2.50. The experiment will not continue unless you have accumulated €2.50.

Your account:

Your money is stored in an account. The conductors of the experiment double this amount such that €5 are stored in the account.
[next screen]

Your payment:

As explained above, €5 are on the account.

Whether you will receive the €5 will be determined randomly. You could receive the entire €5 or you could receive no payment at all.

Comprehension question: Which amount of money cannot be earned by you in part 1? €0 or €2.50 or €5.00.

[next screen]

Before you will be made aware that you do or do not receive €5, we would like you to accumulate €2.50 in the typing-task.
[next screen]

typing-task

[next screen]

Thank you for accumulating €2.50. The conductors of the experiment added another €2.50 to your account such that €5 are on your account.

You will be informed how whether you were randomly selected to receive the €5 or not on the next screen.

[next screen]

The computer randomly chose that you receive €5 [0] for part 1. Therefore, your payoff for part 1 is €5 [0].

Please notice that whether you earned €5 or not was randomly determined. You can think of it as if a ball was was drawn from an urn. In the urn were 96 balls. 48 balls were red and 48 were blue. If a red ball would have been drawn, then you had received €5. If a blue ball would have been draw, then you had received €0. In your case, a red [blue] ball was drawn randomly.

Trust Game

Like in Section 4.A