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Conference Paper

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Beiträge zur Jahrestagung des Vereins für Socialpolitik 2015: Ökonomische Entwicklung -
Theorie und Politik - Session: Competition and Collusion, No. D12-V2

Provided in Cooperation with:

Verein für Socialpolitik / German Economic Association

Suggested Citation: Stracke, Rudi; Kerschbamer, Rudolf; Sunde, Uwe (2015) : Prevalence and Determinants of Choice Bracketing - Experimental Evidence, Beiträge zur Jahrestagung des Vereins für Socialpolitik 2015: Ökonomische Entwicklung - Theorie und Politik - Session: Competition and Collusion, No. D12-V2, ZBW - Deutsche Zentralbibliothek für Wirtschaftswissenschaften, Leibniz-Informationszentrum Wirtschaft

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Prevalence and Determinants of Choice Bracketing – Experimental Evidence*

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July 10, 2015

Abstract

This paper presents novel evidence for the prevalence and the determinants of choice bracketing in strategic environments. If a two-stage pairwise elimination contest is presented in a “separate reward” frame, where stage-1 winners receive an immediate material reward, decision makers tend to bracket narrowly by choosing stage-1 and stage-2 investment separately. By contrast, in a payoff-equivalent “integrated reward” frame, where the only apparent reward on stage 1 is the right to participate in stage 2, decision makers appear to bracket broadly. Disaggregate data analysis and the results of control treatments provide no support for alternative explanations and suggest that narrow bracketing is related to cognitive capacity limitation and the complexity of the decision environment.

JEL-Classification: C72, D81, M52, J33

Keywords: Choice bracketing; Multi-stage contest; Narrow bracketing; Framing; Experiment

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1 Introduction

“When making many choices, a person can broadly bracket them by assessing the consequences of all of them taken together, or narrowly bracket them by making each choice in isolation.” (Read, Loewenstein, and Rabin 1999, p. 171)

At least since the path-breaking work by Read and Loewenstein (1995) and Read, Loewenstein, and Rabin (1999), choice bracketing has been suggested as an explanation for many behavioral regularities that are inconsistent with standard rational decision models.¹ Despite its prevalence in colloquial discussions, there is surprisingly little systematic work on the importance and the determinants of narrow bracketing, and evidence for the widely shared conjecture that narrow bracketing is related to cognitive capacity limitations – going back to seminal contributions by Miller (1956), Kahneman (1973), and Simon (1957) – is scarce.² The existing evidence for narrow bracketing stems almost exclusively from studies that investigate this phenomenon in combination with prospect-theoretic concepts such as loss aversion or probability weighting (see, e.g., Benartzi and Thaler, 1995, Gneezy and Potters, 1997, Langer and Weber, 2001, and Hilgers and Wibral, 2014), even though those latter behavioral concepts are inherently different and not necessarily linked to choice bracketing.

This paper presents novel evidence for the prevalence and the determinants of choice bracketing. To investigate the importance and determinants of narrow choice bracketing for individual behavior, we study two versions of a two-stage pairwise elimination contest that differ only in how the rewards for winners are framed. In the “separate reward” frame, stage-1 winners receive an immediate reward for winning the stage-1 interaction before they compete for a second reward on stage 2. In the “integrated reward” frame, however, stage-1 winners receive no immediate reward for winning the stage-1 interaction, but only after the stage-2 winner is determined; the stage-2 winner is then rewarded for winning both stages, while the stage-2 loser receives a reward for participating on stage 2. The effective rewards as well as the associated realization probabilities are identical across reward frames, implying that the reward frame does not affect stage-1 or stage-2 investment choices in a standard framework with rational and forward looking decision makers, independent of risk attitudes. The reward frame arguably matters for decision makers who are prone to narrow choice bracketing in the sense of Read, Loewenstein, and

¹The related but broader concept of mental accounting, which does not only refer to choice-related evaluations of payoffs, goes back to Thaler (1985).

²The classical argument is that decision makers might bracket narrowly whenever broad bracketing would involve computational demands that are not in relation to the gains from incorporating all consequences of a particular choice. Recent support for this argument is provided by Abeler and Marklein (2010).

Rabin (1999), however, since the different frames are likely to affect how decision makers perceive the decision environment.³ To illustrate the argument, notice that stage-1 winners receive no immediate reward for winning stage 1 in the “integrated reward” frame. This might induce participants to view the stage-1 interaction as a qualification stage, thereby forcing them to take account of future stage-2 rewards already when choosing the stage-1 investment. In contrast, if the two-stage contest is framed as a sequence of interactions in each of which the winner receives a reward – as in the “separate reward” frame – decision makers may be (more) inclined to bracket narrowly around investment choices and focus only on the reward associated with the respective stage on which they currently choose their investments. Intuitively, decision makers who bracket narrowly are likely to omit the option value of future stage-2 rewards when choosing their stage-1 investment in the “separate reward” frame, but not in the “integrated reward” frame. Consequently, a model that incorporates choice bracketing predicts that stage-1 investment is lower in the “separate” than in the “integrated reward” frame, while stage-2 investment is predicted to be unaffected by the reward frame.

We test these predictions in a controlled laboratory experiment by implementing two treatments, one with ‘separate rewards’ (SR) and the other one with ‘integrated rewards’ (IR). The data show that the investment choices of subjects on stage 2 are almost identical across treatments, in line with the predictions of the standard rational-choice model as well as the model accounting for choice-bracketing. At the same time, the reward frame appears to strongly affect stage-1 investments. In particular, average stage-1 investments are significantly lower in the SR treatment than in the IR treatment. This is inconsistent with the prediction of the standard rational-choice model, but it is in line with the choice-bracketing prediction. Further analysis of the data at the individual level provides additional support for the choice-bracketing explanation of the observed treatment effect. First, cognitive capacity – approximated by self-reported math grades – seems to matter for the size of the treatment effect. Subjects who are satisfactory or worse in math invest less in stage 1 of the SR treatment than in stage 1 of the IR treatment, while stage-1 investment choices of subjects with good or very good math grades do not differ across treatments. Given that the decision environment with strategic interactions at multiple stages considered here is fairly complex, this finding supports the conjecture that narrow bracketing is a means for subjects with cognitive capacity limitations to reduce complexity. In addition, we find little evidence that subjects with satisfactory or worse grades learn to bracket broadly as they become more experienced; the treatment effect remains

³The focus of this paper lies on the investigation of narrow choice bracketing. To focus ideas, we treat rational and forward looking decision making and broad choice bracketing as synonymous in what follows.

sizeable even after several rounds of the experiment. Second, we find that the treatment difference is larger for subjects that reveal risk aversion in small-stakes lotteries, and just as in case of math grades, the treatment effect of subjects whose choices reveal risk aversion remains sizeable even after several rounds of the experiment. Since treatments are designed such that both the standard rational-choice and the choice-bracketing prediction are independent of risk attitudes, this finding provides evidence in support of a conjecture by Rabin and Thaler (2001) according to which narrow bracketing contributes to the excessive demand for risk premia in choices that involve small stakes. In particular, it seems that decision makers who fail to integrate their wealth when evaluating small stakes lotteries are likely to apply narrow choice bracketing.

To further investigate the determinants of narrow bracketing, we implement two control treatments **SRc** and **IRc** that have the same payoff structure as the baseline treatments, but where the stage-2 interaction is replaced by a simple lottery. On the one hand, this difference across main and control treatments reduces the complexity of the stage-1 investment decision, as stage-2 winning probabilities are now exogenously given rather than endogenously determined in a second strategic interaction. Specifically, there is no need to think about either expected stage-2 opponent investment or own future investment in stage 2. On the other hand, the control treatments introduce a variation in the dimension around which decision makers may bracket narrowly or broadly. In particular, by eliminating the choice on the second stage, the reward for winning stage 1 in the “separate reward” frame remains directly linked to a choice, whereas the reward for winning stage 1 in the “integrated reward” frame might be seen as not directly linked to a choice. This implies that decision makers who have a tendency to bracket narrowly might actually invest higher amounts in **SRc** than in **IRc**, simply because they perceive rewards associated with a choice in **SRc**, whereas they perceive rewards as linked to a lottery rather than a choice in **IRc**. Choice bracketing should thus have opposing effects in the control as compared to the main treatments. The data show that stage-1 investment choices are indeed differently affected by the reward frame in the control treatments, suggesting that some decision makers fail to integrate rewards that are linked to choices and lottery rewards, and bracket narrowly even in less complex environments.

The behavior observed in the control treatments also suggests that any difference in the reward frame that is unrelated to choice bracketing, such as the label attached to rewards in the experimental instructions or the size of rewards related to particular choices, are unable to explain the pattern we observe, as these are identical across main and control treatments. When analysing the data at the individual level, we find no evidence that cognitive capacity matters for choice bracketing in the control treatments, in contrast

to the behavior observed in the main treatments that are far more complex due to the strategic interactions on stage 2. This indicates that limited cognitive capacity is not a general proxy for narrow bracketing, but that it instead interacts with the complexity of the decision environment.⁴ Just as in the main treatments, however, the findings indicate that subjects whose choices exhibit risk aversion in small-stakes lotteries are more likely to bracket narrowly. This suggests that the failure of subjects to integrate their wealth when evaluating small stakes lotteries might indeed be a proxy for narrow bracketing behavior in other settings.

The results of this paper contribute to the literature on narrow bracketing in several dimensions. First, by documenting the prevalence of narrow bracketing using different reward frames in multi-stage elimination contests, the results suggest that narrow bracketing might not only affect choices between *given* lotteries, as in, e.g., Tversky and Kahneman (1981), Gneezy and Potters (1997), or Rabin and Weizsäcker (2009), but also investment choices in the context of strategic interactions where the winning probabilities and therefore the rewards are determined endogenously.⁵ Second, by investigating a context where the payoffs of all decision makers are strictly positive, the results of this study indicate that choice bracketing is not necessarily linked to other behavioral concepts like loss aversion, while most existing contributions combine narrow bracketing with loss aversion and investigate the joint effect of myopic loss aversion on behavior (Benartzi and Thaler 1995, Gneezy and Potters 1997, Langer and Weber 2001). Third, this study provides novel evidence on the determinants of narrow bracketing, lending support to the conjectures of Miller (1956), Kahneman (1973), or Simon (1957) according to which cognitive capacity limitations induce narrow bracketing behavior in complex decision problems. The fact that we find little evidence for the importance of cognitive capacity limitations in the comparably simpler control treatments suggests an interaction between cognitive capacity, the complexity of the respective decision problem, and the prevalence of choice bracketing. This complements earlier evidence by Frederick (2005) and Benjamin, Brown, and Shapiro (2013) according to which individuals with lower cognitive capacity tend to exhibit behavior that is less consistent with the predictions from rational choice models, and evidence by Abeler and Marklein (2010) that individuals with lower cognitive ability are more likely to apply mental accounting. Finally, the feature of our experimental design that behavior is independent of risk attitudes allows us to investigate the conjecture by Rabin and Thaler (2001) that narrow bracketing matters for the frequently observed

⁴This might explain why previous studies that rely on choices between two simple lotteries – as Rabin and Weizsäcker (2009), for example – find no link between measures of cognitive capacity and the propensity of decision makers to bracket narrowly.

⁵The only other contribution we are aware of that investigates choice bracketing in the context of strategic interactions is Ding (2012), who considers behavior of subjects in different roles of a trust game.

excessive demand for risk premia in choices that involve small stakes. In line with their arguments, we indeed find that subjects whose choices exhibit risk aversion in small-stakes lotteries are more likely to bracket narrowly. This suggests that risk aversion expressed in small-stakes lotteries might help to predict if a decision maker is likely to bracket narrowly in other settings.

This paper also contributes to the literature on multi-stage promotion contests. In particular, the results of this paper complement the results of Altmann, Falk, and Wibrall (2012), who find, based on evidence from lab experiments, that option values have powerful incentive effects in multi-stage promotion contests, in line with the seminal contribution by Rosen (1986). Our results replicate their finding that subjects account for the option value of future competition in stage 1 of the “integrated reward” frame – which is the one they use in their experiments – and extend it by showing that subjects are more likely to omit the option value in the “separate reward” frame. This paper also contributes to the literature on optimal prizes in multi-stage contests. While existing studies investigate how changes in the convexity of rewards across stages affects stage-1 and stage-2 behavior – see, e.g., Delfgaauw, Dur, Non, and Verbeke (2015) or Stracke, Höchtl, Kerschbamer, and Sunde (2014) – the results of the present paper show that a pure framing variation may also have important implications for investment incentives. In this sense, this paper is related to the contribution by Hossain and List (2012) who identify behavioral responses to framing manipulations in a field experiment.

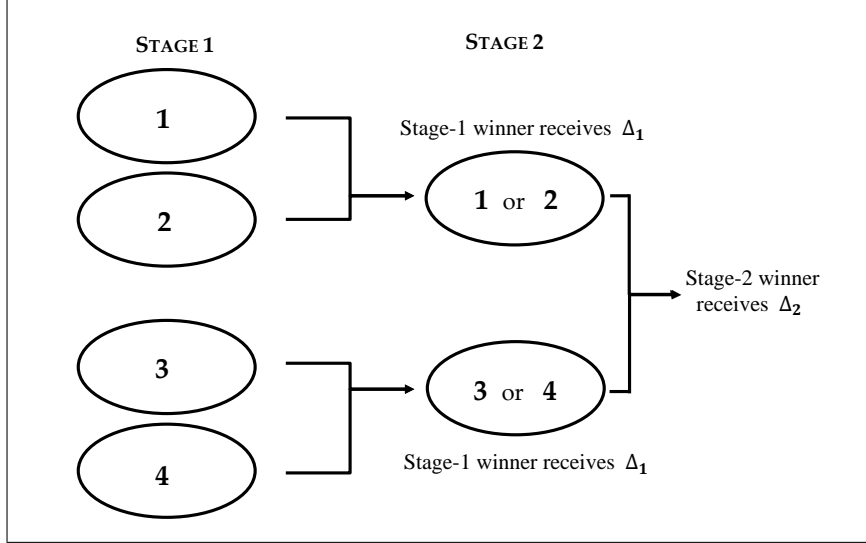
The remainder of this paper is structured as follows. Section 2 introduces the formal model and derives behavioral predictions. The experimental design is explained in Section 3. Section 4 presents and discusses the main findings, Section 5 discusses the plausibility of alternative explanations, and Section 6 concludes.

2 Theoretical Analysis

2.1 The Model

Consider a two-stage pairwise elimination contest with four identical, risk-neutral agents. As depicted in Figure 1, the four agents initially compete in pairs on stage 1 for two open stage-2 positions. Subsequently, the two stage-1 winners compete against each other on stage 2 to determine the winner of the contest. The reward for winning stage 1 is Δ_1 , and additional reward for also winning stage 2 is Δ_2 . In each interaction, agents choose the amount they want to invest into the contest. Contest investments increase the probability to win an interaction, but are costly independent of success or failure. For each unit invested, an agent incurs the same constant marginal cost of one. In particular,

Figure 1: Structure of the Contest



the probability to win is given by a lottery contest success function à la Tullock (1980), where – given individual (non-negative) investment choices x_{si} and x_{sj} by agents i and j ($\neq i$) on stage $s \in \{1, 2\}$ – the probability that agent i wins the stage- s competition equals

$$p_{si}(x_{si}, x_{sj}) = \begin{cases} \frac{x_{si}}{x_{si} + x_{sj}} & \text{if } x_{si} + x_{sj} > 0 \\ \frac{1}{2} & \text{if } x_{si} = x_{sj} = 0. \end{cases}$$

The equilibrium concept is subgame perfect Nash. Therefore, we start by solving the stage-2 interaction, before considering the parallel stage-1 pairings.

Stage-2 Investment. Since agents are homogeneous, the identity of agents who compete on stage 2 does not affect behavior. Therefore, it is without loss of generality to consider the interaction between agents i and j . The formal optimization problem for agent i reads

$$\max_{x_{2i} \geq 0} \Pi_{2i}(x_{2i}, x_{2j}) = \frac{x_{2i}}{x_{2i} + x_{2j}} \Delta_2 - x_{2i} \quad .$$

Combining first-order optimality conditions and imposing symmetry delivers stage-2 equilibrium investment

$$x_2^* \equiv x_{2i} = x_{2j} = \frac{\Delta_2}{4}. \quad (1)$$

Optimal stage-2 investment is thus fully determined by the reward Δ_2 that is awarded to the stage-2 winner. Inserting equilibrium investments in the respective objective function delivers the value of participation in stage 2, i.e., the expected stage-2 equilibrium payoff.

Defining $\Pi_2^* := \Pi_{2i}(x_{2i}^*, x_{2j}^*) = \Pi_{2j}(x_{2j}^*, x_{2i}^*)$ we obtain

$$\Pi_2^* = \frac{\Delta_2}{4} .$$

Reaching stage 2 thus has an expected value of $\Pi_2^* = \Delta_2/4$ for agents who compete on stage 1. Consequently, Π_2^* is the relevant option value for the stage-1 investment decision.

Stage-1 Investment. As before, the identity of agents who compete against each other in a pairwise interaction does not matter for the stage-1 solution, since agents are homogeneous by assumption. Therefore, it is without loss of generality to consider the interaction between agents k and l ($\neq k$). Agent k faces the optimization problem

$$\max_{x_{1k} \geq 0} \Pi_{1k}(x_{1k}, x_{1l}) = \frac{x_{1k}}{x_{1k} + x_{1l}} [\Delta_1 + \Pi_2^*] - x_{1k}.$$

First-order and symmetry conditions jointly determine stage-1 equilibrium investment. Inserting the formal expression for Π_2^* gives

$$x_1^* \equiv x_{1k}^* = x_{1l}^* = \frac{4 \cdot \Delta_1 + \Delta_2}{16}. \quad (2)$$

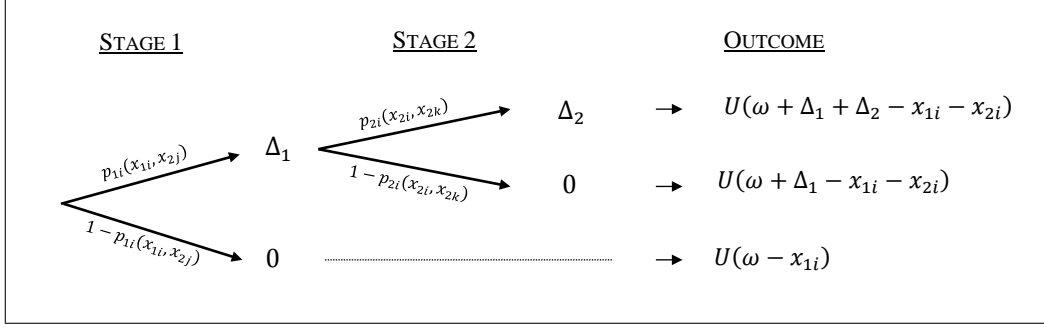
Optimal stage-1 investment is thus determined by two components: First, investment increases in the reward Δ_1 for winning stage 1. Second, winning stage 1 entails an option value of future competition on stage 2. Forward-looking agents take this into account, such that stage-1 investment is also increasing in the reward Δ_2 that is awarded to the stage-2 winner.

2.2 Framing of Rewards

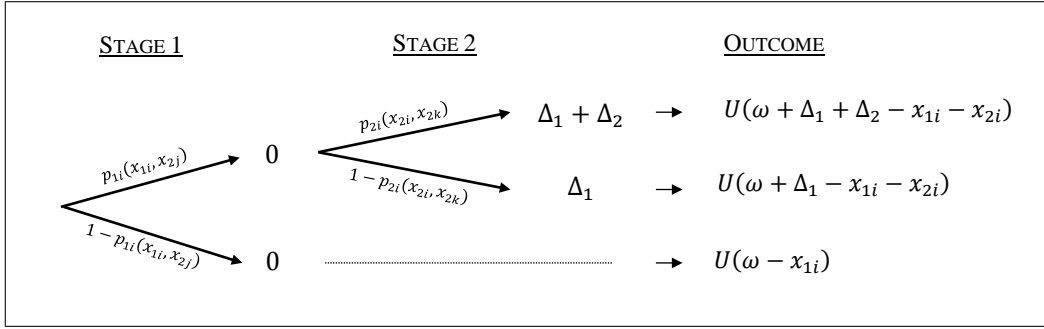
Recall that the two agents who lose on stage 1 receive no reward, that the agent who wins the stage-1 interaction but loses on stage-2 receives Δ_2 , and that the agent who wins on both stages receives the sum $\Delta_1 + \Delta_2$.

There are different framing variants that achieve this allocation of rewards. First, rewards for winning stage 1 and stage 2 can be *separated*, as depicted in Figure 2(a). In this *separate-reward* frame (SR), each stage-1 winner receives the reward Δ_1 before competing on stage 2. Subsequently, the two stage-1 winners compete for the remaining reward Δ_2 on stage 2. Alternatively, rewards for winning stage 1 and stage 2 can be *integrated*, as in the *integrated-reward* frame (IR) depicted in Figure 2(b). In this case, the stage-2 winner receives the sum $\Delta_1 + \Delta_2$ for winning both stages, while the stage-2 loser receives the reward Δ_1 for participating on stage 2 (i.e., winning stage 1).

Figure 2: Separate and Integrated Rewards



(a) Separate Rewards (SR)



(b) Integrated Rewards (IR)

In the absence of discounting across stages, the framing of rewards leaves investment incentives unaffected. To illustrate this claim, assume that all agents share a common utility function $U(\cdot)$ and receive some initial endowment ω prior to the competition to cover contest investments.⁶ Figure 2 provides all possible outcomes both in the separate- and integrated-reward frame and shows that the final payoff is independent of framing, even if decision makers are risk averse. Intuitively, it is irrelevant for an agent who wins stage 1 whether he receives the reward Δ_1 before or after choosing stage-2 investment, as long as he cares about final wealth.

The framing of rewards might affect how agents perceive the decision environment, however. In particular, note that the separate-reward frame presents the contest as a sequence of two interactions. The winner of each interaction receives a reward, while the loser receives nothing. This allows decision makers to mentally separate the stage-1 from the stage-2 interaction. When deciding on their stage-1 investments, decision makers are thus more likely to focus entirely on the immediate reward accruing on stage 1, ignoring the option value to receive additional rewards in future interactions in case they prevail on stage 1. The integrated-reward frame, however, presents the contest as a sequence of a qualification-stage without rewards and final-stage with rewards for both the winner and the loser. Given that stage-1 winners receive no immediate reward, agents are forced to

⁶The endowment is necessary if decision makers are risk averse and also part of the experimental design – see section 3 for details.

take the final stage into account and integrate all rewards when choosing their stage-1 investment. In particular, the only reason for a non-zero investment choice on stage 1 is the prospect to receive a reward on stage 2. In this sense, the integrated-reward frame enforces forward-looking behavior. The framing of rewards might thus induce different degrees of choice bracketing: Separate rewards allow decision makers to bracket narrowly by focussing entirely on the current stage, while integrated rewards force decision makers to take the next stage into account and bracket broadly instead.

2.3 Theoretical Predictions

Let $x_1^*(T)$ and $x_2^*(T)$ be the stage-1 and stage-2 equilibrium investment choices in reward frame $T = \{\text{SR}, \text{IR}\}$. Then,

Proposition 1 (Rational Choice). *If decision makers are fully rational, optimizing, forward looking and not discounting, the reward frame leaves equilibrium investment choices in a two-stage pairwise elimination contest unaffected – independent of risk attitudes:*

$$\begin{aligned} (a) \quad & x_1^*(\text{SR}) = x_1^*(\text{IR}) \\ (b) \quad & x_2^*(\text{SR}) = x_2^*(\text{IR}). \end{aligned}$$

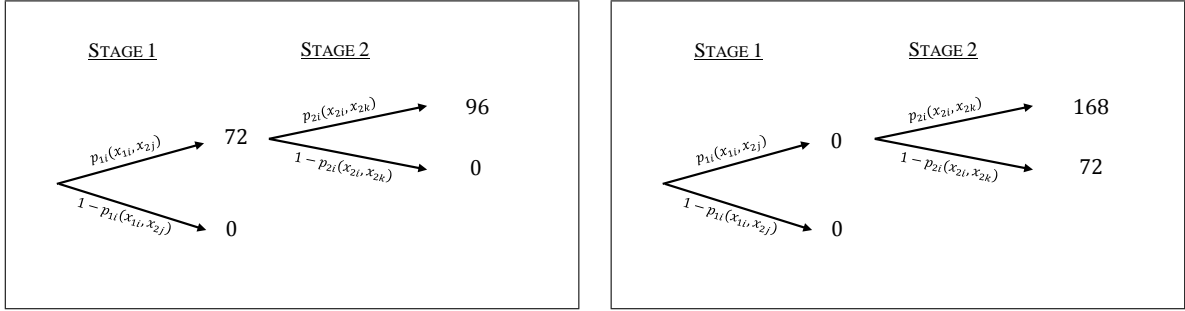
Proof. Follows directly from Figure 2. □

Consider next decision makers that are prone to choice bracketing. Choice bracketing in the separate-reward frame implies that decision makers omit the option value of future competition when choosing stage-1 investment – in contrast to the integrated-reward frame which induces decision makers to bracket broadly.⁷ Stage-1 investment is thus predicted to be lower in the immediate- than in the integrated-reward frame – see equation (2) which shows that stage-1 equilibrium investment increases in the value of future competition. Stage-2 investment is unaffected by choice bracketing, however, since equilibrium investment is independent of future or previous choices – see equation (1).

Proposition 2 (Choice Bracketing). *If decision makers are prone to choice bracketing, the framing of rewards affects stage-1 equilibrium investment choices in a two-stage pairwise*

⁷Strictly speaking, the rational-choice model implies broad bracketing but not vice versa. What we have in mind is that the behavior of decision makers prone to choice bracketing is likely to be affected by the reward frame. In particular, choice bracketing decision makers employ narrow choice bracketing in the separate reward frame, as compared to broad choice bracketing in the integrated-reward frame.

Figure 3: Experimental Treatments: SR (left) and IR (right)



elimination contest, but leaves stage-2 investment unaffected.

$$(a) \quad x_1^*(\text{SR}) < x_1^*(\text{IR})$$

$$(b) \quad x_2^*(\text{SR}) = x_2^*(\text{IR}).$$

Proof. Follows directly from equations (1) and (2). □

3 Design of the Experiments

Experimental Treatments and Parameters. The experimental design features two main treatments which only vary in how the rewards are framed. In particular, $\Delta_1 = 72$ and $\Delta_2 = 96$ hold in both treatments. Subjects in treatment **SR** – short for ‘*separated rewards*’ – were told to receive the reward $\Delta_1 = 72$ immediately after winning stage 1. In contrast, subjects were informed that rewards are awarded only after the realization of the stage-2 outcome in the ‘*integrated reward*’ treatment **IR**. The sequence of events in the treatments **SR** and **IR** is illustrated in Figure 3.

For each of the two main treatments, **SR** and **IR**, we ran 5 sessions. In addition, we conducted 2 sessions each of two control treatment **SRc** and **IRc** that are discussed in section 4.3. All sessions were computerized, using the software z-Tree (Fischbacher 2007). The 280 subjects were students from the University of Innsbruck and recruited using ORSEE (Greiner 2004). The experimental currency was defined such that 200 units corresponded to 1.00 Euro. Sessions lasted approximately 70 minutes (including distribution of instructions at the start and payment at the end of the session). Participants earned between 9 and 13 Euro (approximately 11 Euro on average).

Implementation. The protocol in an experimental session was identical across treatments: First, the subjects received some general information about the experimental

session, including the information that the experiment had two parts. Then, instructions for part 1 of the experiment – either treatment **SR**, **IR**, **SRc**, or **IRc** – were distributed.⁸ After each subject confirmed on the computer screen that he/she had read the instructions, subjects had to answer a series of control questions correctly to ensure that they had fully understood the instructions. Each experimental subject then played 30 rounds of the contest game in the respective treatment. Subjects were informed that the strategic interaction was the same in each of the 30 decision rounds, but that subjects would be randomly rematched before each new round. Matching groups corresponded to the entire session. After each decision round, subjects were informed about their own decision, the decision of their immediate opponent on stage 1 and stage 2 (if applicable), and about their own payoff. This allows for an investigation of whether experimental subjects learn when completing the same task repeatedly. To minimize the potential impact of income effects, subjects were told that only four decision rounds (out of 30) would be chosen randomly and paid out at the end of the experiment. In part 2 of the experiment, we used a choice list similar to the one employed by Dohmen, Falk, Huffman, and Sunde (2010) to elicit risk attitudes.⁹ At the end of the session individual information regarding socio-economic characteristics was collected in a questionnaire. The questionnaire also asked for the field of study and the last math grade in school. The questionnaire was not incentivized and participation was voluntary. The participation rate was nevertheless quite high – between 85 and 100% responded, depending on the question asked. Participants were informed about their earnings after the questionnaire.¹⁰

Decision Environment. The decision environment in the experiment was framed neutrally in that we did not relate the contest to a particular application. The Tullock-type contest success function was explained to subjects using a lottery analogy: Participants were told that they could buy a discrete number of balls in each interaction. The balls

⁸A translated version of the instructions is provided in Appendix B. The original instructions (in German) are available from the authors upon request.

⁹In the Dohmen, Falk, Huffman, and Sunde (2010) procedure, each subject is exposed to a series of choices between a safe payment (which is systematically varied) and a binary lottery (which remains constant across choices). This is cognitively simpler than the procedure employed by Holt and Laury (2002), where a subject is confronted with a series of choices between two binary lotteries that are both varied systematically. The instructions which experimental subjects received right before the risk-elicitation part are provided in Appendix B.

¹⁰Table 13 in Appendix A provides descriptive information about the balancing of the pool of subjects across treatment, regarding field of study, gender, the last math grade in school, risk attitude and the number of mistakes when answering the control questions. The absolute values suggest that subjects are, on average, weakly risk-averse, fairly good in mathematics, frequently from the econ department, and able to correctly answer the control questions. Regarding differences across treatments, the table indicates that there are slightly more male students in the **SR** than in the **IR** treatment; yet, the difference across treatments is insignificant (MWU-test: $p=0.1502$). Risk attitudes, the share of students from the econ department, the average math grade, and the number of incorrectly answered control questions are almost identical across treatments.

Table 1: Predictions – Treatments SR and IR

DECISION MODEL	RATIONAL CHOICE		NARROW BRACKETING	
	SR	IR	SR	IR
Stage-1 Investment (x_1)	24	24	18	24
Stage-2 Investment (x_2)	24	24	24	24
Prizes (Δ_1, Δ_2)	(72,96)	(72,96)	(72,96)	(72,96)

purchased by the subject as well as those purchased by their respective opponents were then said to be placed in the same ballot box, out of which one ball was randomly drawn subsequently. This setting reflects the experimental implementation of the Tullock (1980) contest technology from the theoretical set-up. Participants had to buy (and pay for) their desired number of balls before they knew whether or not they won a pair-wise interaction in the contest. To avoid the possibility of losses and the associated limited liability problems, each subject received an endowment of 240 points in each round. This endowment could be used to buy balls in both stages, that is, a subject that reached stage 2 could use whatever remained of the endowment (after the costs for the number of balls acquired on stage 1 were deducted) to buy balls in the stage-2 interaction. Subjects knew that the share of the endowment which they did not use to buy balls was added to the payoffs of that round. Therefore, purchasing balls implies real (monetary) costs. Since the endowment was as high as the sum of all prizes that were awarded in the contest, participants were not budget-constrained at any time. To ensure that the strategic interaction is identical in each decision round, experimental subjects were told that the endowment could only be used in a given round. This procedure is also conservative in highlighting the connection between both levels in terms of the joint budget, thus biasing behavior against finding a difference in the two treatments.

Testable Hypotheses. Table 1 presents the predicted choices of a risk neutral decision maker in a standard rational-choice model and in a setting characterized by choice-bracketing as previously described. The numbers reveal that rewards Δ_1 and Δ_2 were chosen such that incentives to invest are maintained across stages under risk neutrality. Recall, however, that differences across treatments – on which the theoretical predictions in section 2.3 are based – are independent of risk attitudes. Given that discounting across stages is irrelevant in the experimental setting – subjects receive their final payoff at the end of the experimental session in all treatments – Proposition 1 delivers the hypothesis

that, according to the standard model of rational choices, the reward frame does not affect behavior:

$$x_2(\text{SR}) = x_2(\text{IR}) \tag{3}$$

$$x_1(\text{SR}) = x_1(\text{IR}). \tag{4}$$

The alternative hypothesis is based on the notion that decision makers apply some sort of choice bracketing. For this case, Proposition 2 predicts that the reward frame influences the decision to bracket narrowly or broadly, respectively. In particular, stage-1 investment choices are predicted to differ across treatments (since decision makers view $\Delta_2 = 0$ in condition (2) under choice bracketing), while stage-2 investments are predicted to be equal across treatments:

$$x_2(\text{SR}) = x_2(\text{IR}) \tag{5}$$

$$x_1(\text{SR}) < x_1(\text{IR}). \tag{6}$$

Even though it seems that stage-1 behavior is sufficient to assess whether decision makers are either rational and forward looking, or instead prone to choice bracketing, stage-2 behavior is crucially important for our hypotheses. Given that stage-2 behavior determines the option value of future competition, differences in stage-2 investment affect stage-1 behavior whenever decision makers are bracketing broadly. In this sense, the equality of stage-2 investment choices across treatments **SR** and **IR** that is predicted by conditions (3) and (5) is a prerequisite for an assessment of the investment on the first stage.

4 Experimental Results

4.1 Main Results

The results for treatments **SR** and **IR** are summarized in Table 2 in terms of the average investment levels by stage and treatment. Consider stage-2 investment first. Recall that stage-2 investment choices are predicted to be unaffected by the rewards frame and thus identical across treatments **SR** and **IR** – independent of choice bracketing. The averages of stage-2 investment across subjects in Table 2 – 43.237 in **SR** vs. 45.976 in **IR** – indicate that what we observe in the experiments is qualitatively in line with this theoretical prediction.

Given that subjects interact repeatedly, receive feedback, and are randomly rematched after each round, we must account for interdependencies across subjects within the same session. When collapsing the data by treatment and stage on the session level across all

Table 2: Experimental Results – Treatments SR and IR

	SR	IR
Stage-1 Investment (x_1^*)	35.494 (1.874)	45.238 (2.957)
Stage-2 Investment (x_2^*)	43.237 (1.852)	45.976 (2.614)

Note: The numbers denote average investments across 30 decision rounds and all subjects (in experimental currency). Standard errors are provided in parentheses.

decision rounds, the non-parametric Mann-Whitney U-test (MWU-test) indicates that stage-2 investments do not differ across treatments (p-value of 0.347) – as expected according to the predictions (3) and (5). An alternative way to test these predictions is regression analysis. We estimate a random effects model on the basis of individual decisions, but allow standard errors to be clustered on the session level. Standard errors are allowed to be correlated at the session level to account for the fact that matching groups correspond to the entire session. All specifications include round fixed effects to control for potential experience effects. Table 3 reports the results from regressions of individual stage-2 investment on the treatment dummy SR. Column (1) provides the baseline specification using stage-2 investment choices of all subjects and all rounds of the experiment. In Column (2), additional control variables (gender, risk attitudes, econ major, self-reported math scores, and the number of mistakes in the control questions) are included in the specification. In light of evidence from previous contest experiments that risk attitudes and gender matter for competitive behavior, one might want to rule out that lacking randomization in these dimensions affects the results.¹¹ Column (3) restricts attention to investment decisions in the second half of the experiment (rounds 16-30). Throughout all specifications, the results indicate that stage-2 investments are not significantly different across treatments, as indicated by the insignificant coefficient of the treatment-dummy.¹² Taken together, hypotheses (3) and (5) are not rejected by the data.

Result 1. *In line with the standard rational-choice and the choice-bracketing predictions,*

¹¹The subject pool is fairly balanced across both treatments, as indicated in Table 13 in Appendix A and Footnote 10 above.

¹²The table (and all subsequent estimation tables) report inference based on standard errors clustered on the session level – see table notes for details. In Appendix A, we also report results from panel regressions using session means (i.e., collapsing the data at the session-round level), with session random effects and round fixed effects. The unit of observation in these regressions is a session round. The regression results for different specifications are reported in Table 11. In particular, the investment measure is the mean (columns (1) and (3)) or the median (column (2)), while the sample is restricted to decision rounds 16-30 in the specification reported in column (3).

Table 3: Testing Equality of Stage-2 Investment Across Treatments

	STAGE-2 INVESTMENT		
	(1)	(2)	(3)
Treatment SR	-2.316 (0.573) [-12.17; 3.533]	-3.134 (0.266) [-7.844; 0.054]	0.550 (0.881) [-9.542; 6.771]
Rounds	ALL	ALL	16–30
Round FE	YES	YES	YES
Controls	NO	YES	NO
Sessions	10	10	10
Individuals	200	200	200
Obs.	3,000	2,528	1,500

Note: Random-effects panel estimates; in parentheses: p-values based on bootstrapped robust standard errors clustered on session level; in brackets: 95% confidence interval of coefficient estimate based on standard errors clustered on individual and session level (bootstrapped). **IR** is a dummy variable that equals 1 in treatment **SR** and 0 in treatment **IR**. Control variables include gender, risk attitudes, econ major, self-reported math scores, as well as the number of mistakes in the control questions.

*equality of stage-2 investment choices across treatments **SR** and **IR** is not rejected by the data.*

Even though the data is in line with the qualitative prediction regarding treatment differences in stage-2 investment decisions, the comparison between realized and predicted investment levels reveals that subjects invest more in absolute terms than predicted in both treatments. This over-investment is a standard observation in the experimental contest literature, however. Previous research suggests that three aspects are likely to matter for over-investment in the setting we consider:¹³ First, the endowment that experimental subjects receive at the beginning of each decision round may lead to over-provision if subjects perceive the endowment as ‘play money’ (Thaler and Johnson 1990). In this case, subjects invest more due to this perception than they would without an endowment. A second explanation for over-provision is that experimental subjects are prone to making mistakes in experimental settings (Potters, de Vries, and van Winden 1998). If this is the case, a higher endowment increases the strategy space and thus the chance to make mistakes. Finally, subjects may experience a ‘joy of winning’ in strategic interactions, which amplifies the valuation of rewards awarded in contests. Since individual invest-

¹³See Sheremeta (2013) for an excellent survey of the existing literature and an insightful discussion of existing explanations for this phenomenon.

ments are strictly increasing in rewards, non-monetary values of winning can rationalize over-investment. We do not further elaborate on these explanations for over-provision since all of them are orthogonal to the treatment variation we consider.¹⁴ In this sense, over-investment should leave the hypotheses unaffected – the equality of stage-2 investment choices across treatments suggests that this is indeed the case.

Consider now the investment on stage-1. The rational-choice model with forward-looking decision makers predicts that stage-1 investment choices are unaffected by the reward frame – and are thus identical across treatments **SR** and **IR**. In contrast, choice bracketing predicts that stage-1 investment is lower in treatment **SR** than in treatment **IR**, since the reward frame **SR** facilitates the separation of decisions across stages and may thus induce narrow bracketing. The averages of stage-1 choices across subjects in Table 2 – 35.494 in treatment **SR** vs. 45.238 in treatment **IR** – are consistent with the prediction based on choice bracketing. Even though the absolute amount invested on stage 1 exceeds the prediction from choice bracketing in both treatments, the relative magnitudes are well in line with the choice-bracketing prediction. In particular, the choice-bracketing prediction implies that stage-1 investment is 25% lower in **SR** than in **IR** if the option value is not taken into account on stage 1. This is remarkably close to the 23% difference in stage-1 investments across treatments – see tables 1 and 2 for details.

Statistical inference shows that the observed treatment effect is not only economically sizeable, but also statistically significant. When collapsing the data by treatment and stage on the session level across all decision round, the non-parametric Mann-Whitney U-test (MWU-test) indicates that stage-1 investments are significantly higher in **IR** than in **DR** (p-value of 0.047) – in line with the choice-bracketing prediction, but in contrast to the rational-choice prediction. Employing similar panel estimation models as before confirms this finding. The respective regression output is provided in Table 4. Regardless of the specification and observation rounds used for the estimation, the estimated treatment effect is always significantly different from zero.¹⁵ We summarize this as:

Result 2. *The rational-choice prediction of equality of stage-1 investment choices across treatments **SR** and **IR** is rejected by the data. By contrast, the choice-bracketing prediction that stage-1 investment choices are higher in treatment **IR** than in treatment **SR** is not rejected by the data.*

¹⁴Further aspects that are likely to matter for over-investment, as well as references to papers that explicitly test different potential explanations for over-investment are provided in the aforementioned survey by Sheremeta (2013).

¹⁵In the Appendix, we collapse the data at the session-round level and report estimates for mean or median investment choices across all rounds – see Table 12. These results also confirm the main findings.

Table 4: Testing Equality of Stage-1 Investment Across Treatments

	STAGE-1 INVESTMENT		
	(1)	(2)	(3)
Treatment SR	-9.744 (0.003) [-15.57; -2.671]	-8.989 (0.021) [-15.12; -4.016]	-5.235 (0.037) [-9.986; -1.058]
Rounds	ALL	ALL	16–30
Round FE	YES	YES	YES
Controls	NO	YES	NO
Sessions	10	10	10
Individuals	200	200	200
Obs.	6,000	5,103	3,000

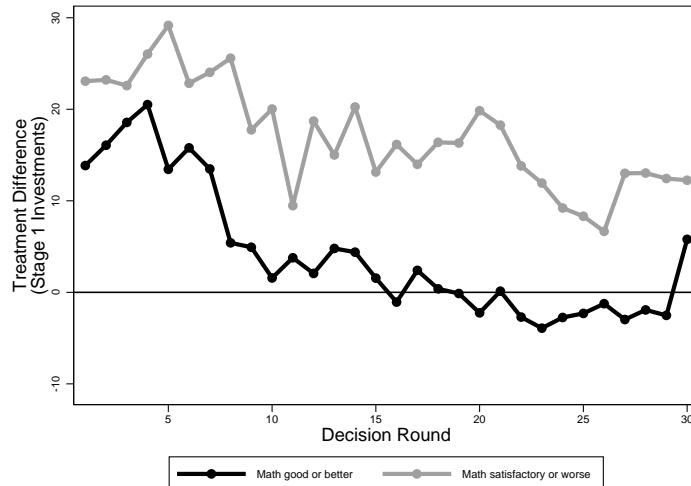
Note: Random-effects panel estimates; in parentheses: p-values based on bootstrapped robust standard errors clustered on session level; in brackets: 95% confidence interval of coefficient estimate based on standard errors clustered on individual and session level (bootstrapped). **SR** is a dummy variable that equals 1 in treatment **SR** and 0 in treatment **IR**. Control variables include gender, risk attitudes, econ major, self-reported math scores, as well as the number of mistakes in the control questions.

4.2 Additional Evidence in Support of the Choice Bracketing Explanation

Taken together, the observed behavior in the experiment is in line with the choice-bracketing prediction, but not in line with the prediction from the standard rational choice model. In the following, we provide more direct evidence supporting the choice-bracketing explanation.

Cognitive Capacity Limitations. Among the most frequently mentioned reasons for choice bracketing are cognitive limitations and inertia. Cognitive limitations of decision makers might induce narrow bracketing due to misperception (Miller 1956), inattention (Kahneman 1973), or analytical processing procedures in complex decision environments (Simon 1957). This suggests that decision makers with comparatively low cognitive abilities are particularly prone to choice bracketing. Intuitively, narrow bracketing substantially simplifies the task with which subjects are confronted in the experiment: Subjects who bracket broadly on stage 1 must account for their future stage-2 investment, the expected stage-2 investment by the future opponent, as well as for the expected stage-1 investment by their immediate opponent when choosing their own stage-1 investment. Decision makers who bracket narrowly, by contrast, need only an estimate for the stage-1

Figure 4: Difference in Stage-1 Investments across IR and SR by Decision Round



investment of their immediate opponent – which is arguably a much less demanding task.

Lacking a direct measure of cognitive ability, we use the self-reported high-school math grade as a proxy for the cognitive ability of an experimental subject (see, e.g., Spinath et al., 2006). Figure 4 shows the average stage-1 investment difference across our main treatments IR and SR by decision round separately for subjects who are good or very good in math, and for subjects who are satisfactory or worse.¹⁶ The figure reveals that the stage-1 treatment effect is inversely related to this proxy for cognitive ability. Moreover, the evolution of the difference across decision rounds indicates that subjects with better math grades learn that they initially might have made mistakes. After about 10 decision rounds, stage-1 investment choices of the subgroup whose proxy for cognitive ability is high are almost identical across treatments. Learning is much less apparent in the choices of subjects with a comparably lower cognitive ability index, however. For this subgroup, the treatment effect remains sizeable throughout.

In order to formally test the hypothesis that cognitive limitations matter for the differences between the stage-1 investment choices across treatments IR and SR, we replicate the estimation analysis from before for the two subgroups separated by the self-reported high-school math grade. In particular, we separately estimate the treatment effect for subjects who self-report a good or very good math grade, and for subjects who self-report a satisfactory or worse math grade using the same classification as in Figure 4. The results are displayed in Table 5. Consider subjects with satisfactory or worse math grades and the respective entries in columns (1) and (2) first: According to Column (1), stage-1 investment choices differ significantly across treatments for this subgroup. Even though

¹⁶The threshold for splitting subjects corresponds to a math grade 2.3 (where 1 is the best and 5 is the worst), which ensures that both subgroups are of almost equal size. Moreover, the threshold is close to the mean math grade provided in Table 13.

Table 5: Cognitive Ability and Stage-1 Investments Across Treatments

	STAGE-1 INVESTMENT			
	math satisfactory or worse		math good or very good	
	(1)	(2)	(3)	(4)
Treatment SR	-17.084 (0.001) [-22.53; -11.83]	-13.441 (0.001) [-20.233; -7.622]	-4.613 (0.457) [-16.782; 7.556]	0.475 (0.901) [-7.041; 7.991]
Rounds	ALL	16–30	ALL	16–30
Round FE	YES	YES	YES	YES
Sessions	10	10	10	10
Individuals	87	87	107	107
Obs.	2,610	1,305	3,210	1,605

Note: Random-effects panel estimates; in parentheses: p-values based on bootstrapped robust standard errors clustered on session level; in brackets: 95% confidence interval of coefficient estimate based on standard errors clustered on individual and session level (bootstrapped). SR is a dummy variable that equals 1 in treatment SR and 0 in treatment IR.

Column (2) suggests that the treatment effect is somewhat weaker in the second half of the experiment, it is still highly significant. This suggests that subjects with satisfactory or worse math grades employ choice bracketing even after they gained experience. For subjects with high math grades, we find no comparable difference in stage-1 investments across treatments. The estimated coefficients across all decision rounds and in the second half of the experiment are insignificant and close to zero. Taken together, the pattern we observe is consistent with the hypothesis that choice bracketing is particularly relevant among decision makers who face cognitive limitations.

To address the question whether subjects with low self-reported math grade merely fail to understand the instructions provided for the decision environment they subsequently encounter in the experiment, we investigate whether the treatment effect is related to the number of incorrectly answered control questions.¹⁷ A greater number of mistakes could be an indication for confusion or misunderstanding of the strategic situation.¹⁸ Table 6 provides the means of stage-1 investments for both treatments for different sub-samples. Excluding subjects who have problems with the control questions leaves the main finding unaffected: Stage-1 investment choices are higher in the IR than in the SR treatment even when considering only subjects who answer all questions correctly. Moreover, we find that

¹⁷Notice that all participants eventually had to answer all control questions correctly before the experiment started, as this was a requirement for the experiment to begin.

¹⁸The translated version of all control questions is provided in Appendix C. The original German version is available from the authors upon request.

Table 6: Stage-1 Investments by Treatment and Mistakes in Control Questions

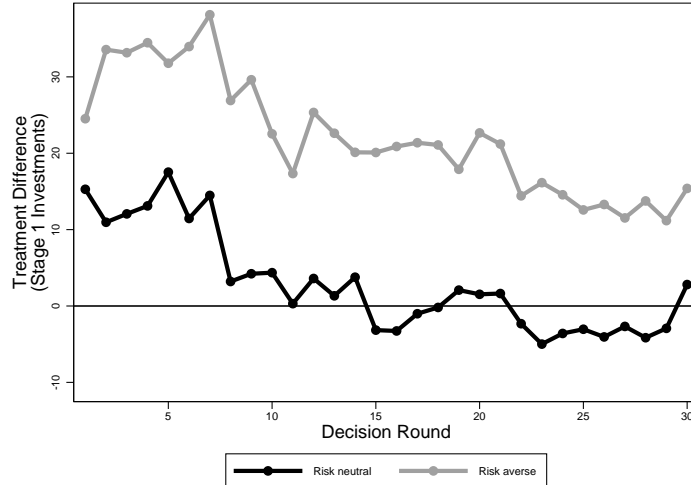
	SR		IR	
	N	MEAN	N	MEAN
all observations	100	35.494	100	45.238
# mistakes ≤ 3	99	35.391	93	44.096
# mistakes ≤ 2	96	35.478	90	41.517
# mistakes ≤ 1	73	32.894	70	41.450
# mistakes = 0	33	28.510	32	44.000

misunderstanding reflected in incorrectly answered control questions is not systematically related to cognitive ability as approximated by the self-reported math grade. The correlation coefficient between these characteristics is 0.037 (p-value 0.5486). Taken together, this evidence provides further support for our hypothesis that the experimental results are not driven by misunderstandings of the experimental task, but rather by cognitive ability limitations that induce choice bracketing as a means to reduce complexity.

Risk Aversion in Small-Stakes Lotteries. Another possible indicator for narrow bracketing (that might be related to cognitive limitations) is the degree of risk aversion revealed in the standard lottery-choice tables used in the experimental literature as a means to elicit risk attitudes. As argued by Rabin (2000), risk averse behavior in lotteries involving extremely small stakes compared to an individual’s total wealth implies incredibly high degrees of risk aversion when extrapolated to risky choices involving larger stakes. The typical explanation for this type of behavior is that individuals narrowly bracket their choices in the lab and fail to integrate their overall wealth or even their typical experimental earnings, see, e.g., Rabin and Thaler (2001). Instead, subjects make choices as if their wealth was zero in the respective situation. Thus, while elicited choices can still be useful in predicting risky choices in real life situations (see, e.g., Dohmen et al., 2011), elicited risk aversion might also be an indicator for narrow bracketing. Intuitively, decision makers who reveal high degrees of risk aversion in small-stakes lotteries arguably fail to integrate their overall wealth when bracketing choices over uncertain outcomes. It therefore seems plausible that those subjects also bracket narrowly in the strategic interaction considered here. Alternatively, elicited risk aversion might reflect cognitive limitations of subjects in environments that involve uncertainty.¹⁹ In both cases, revealed risk aversion could

¹⁹Consistent with the latter view, Dohmen, Falk, Huffman, and Sunde (2010) document a correlation between cognitive skills and risk attitudes as revealed in choices between safe payments and lotteries

Figure 5: Difference in Stage-1 Investments across IR and SR by Decision Round



be related to the treatment effect if choice bracketing is responsible for the difference in stage-1 investments across treatments.

A particular feature of our treatment design is that risk attitudes *per se* do not differently affect behavior across treatments if subjects are exclusively interested in their final wealth. Given that final payoffs are independent of the reward frame – as discussed in more detail in section 2.2 – we do not face the problem of separating ‘pure risk’ from ‘narrow bracketing’ or ‘cognitive limitation’ dimensions of the elicited risk measure. Only in one special case this separation might be difficult, namely if decision makers evaluate stage-outcomes rather than final payoffs and, at the same time, their preferences satisfy decreasing absolute risk aversion. In this case, investment incentives might be affected by wealth effects in the direction observed in the data.²⁰ The separate evaluation of stage-payoffs rather than of final payoffs is itself some form of narrow bracketing, however. Moreover, both the qualitative stage-2 investment pattern and the quantitative stage-1 investment pattern across treatments is inconsistent with this explanation. First, even though numerical simulations suggest that stage-1 investment is higher in this case for IR than for SR, in line with what is observed in the experimental data, the predicted difference across treatments is rather small in size, even for extremely high degrees of risk aversion. This implies that this explanation based on risk aversion is unable to explain quantitatively the substantial stage-1 treatment effect that is observed in the data. Second, numerical simulations also indicate that stage-2 investments should be lower for IR than for SR if this explanation is correct. However, this prediction is in contrast to what

involving small stakes.

²⁰Strictly speaking, wealth effects matter whenever preferences do not satisfy constant absolute risk aversion. Existing empirical studies find no evidence that preferences satisfy increasing absolute risk aversion, which is why we omit this case in the subsequent discussion. Details available upon request.

Table 7: Small Stakes Risk Aversion and Stage-1 Investment Across Treatments

	STAGE-1 INVESTMENT			
	risk averse		risk neutral/seeking	
	(1)	(2)	(4)	(5)
Treatment SR	-22.074 (0.001) [-34.944; -9.203]	-16.533 (0.037) [-32.039; -1.026]	-2.946 (0.587) [-13.572; 7.680]	1.611 (0.597) [-4.357; 7.578]
Rounds	ALL	16–30	ALL	16–30
Round FE	YES	YES	YES	YES
Sessions	10	10	10	10
Individuals	57	57	120	120
Obs.	1,710	855	3,600	1,800

Note: Random-effects panel estimates; in parentheses: p-values based on bootstrapped robust standard errors clustered on session level; in brackets: 95% confidence interval of coefficient estimate based on standard errors clustered on individual and session level (bootstrapped). **SR** is a dummy variable that equals 1 in treatment **SR** and 0 in treatment **IR**.

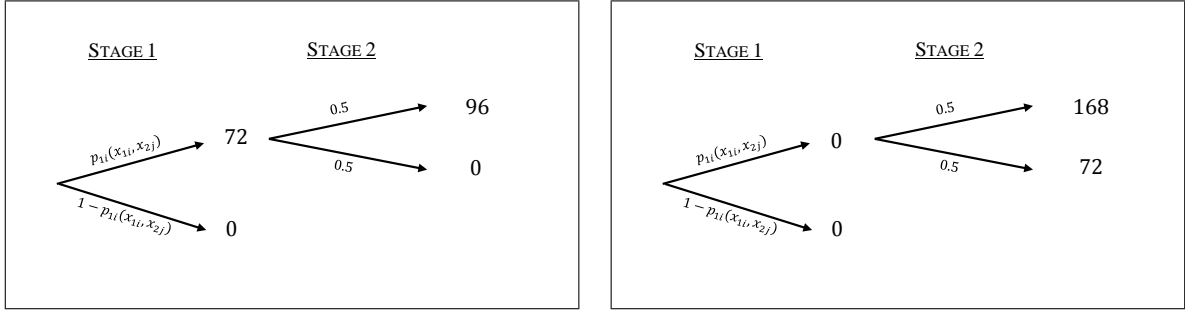
we observe in the data. If anything, the average stage-2 investment is slightly higher (and not lower) in the **IR** treatment than in the **SR** treatment, see Table 2.²¹ As a consequence, in what follows we view elicited risk aversion either as a measure for the inclination to bracket narrowly, or alternatively as a proxy for cognitive limitations of subjects in environments that involve uncertainty.

Figure 5 shows the average stage-1 investment difference across treatments **IR** and **SR** by decision round separately for subjects classified as risk neutral or risk loving, and for subjects whose choice reveal risk aversion.²² The figure reveals that the stage-1 treatment effect is much larger for risk averse than for risk neutral subjects throughout. Moreover, the treatment effect observed in the subgroup of risk neutral subjects disappears after a few rounds, while it remains positive throughout in the subgroup of risk averse subjects. Regression analysis confirms this pattern. Table 7 presents the regression output of separate estimations for the two subgroups, where the sample is split by subjects' risk aversion as revealed in incentivized low-stakes lottery-choices. The estimates support the pattern displayed in Figure 5, with significant treatment differences in stage-1 investment choices for individuals that exhibit risk averse behavior, see columns (1) and (2). There is no significant treatment difference for subjects that make approximately risk-neutral

²¹Details on numerical simulations are available from the authors upon request.

²²Nothing changes if we drop subjects that are classified as risk loving and only compare those subjects who exhibit risk neutral and risk averse behavior. Overall, choices of 120 subjects reveal risk neutrality or risk seeking, while choices of 57 subjects are consistent with risk aversion. The remaining 23 subjects behave inconsistently and switch multiple times between the lottery and the safe payoff.

Figure 6: Control Treatments: SRc (left) and IRc (right)



choices according to the estimates provided in columns (3) and (4).

4.3 Control Treatments

To further explore the validity and robustness of the choice-bracketing explanation, we designed two control treatments, SRc and IRc. In these control treatments, subjects chose stage-1 investments – just as in the main treatments SR and IR – but they did not choose stage-2 investments – in contrast to the main treatments. In particular, the stage-2 interaction was replaced by a lottery that subjects either win or lose with equal probability. The sequence of events illustrated in Figure 6 shows that the lottery implements the same stage-2 winning probability as the (symmetric) equilibrium behavior in the stage-2 interaction that it replaces. Apart from this difference, the sequence of events, the values of all rewards as well as the labels attached to these rewards were identical across treatments SR/IR and SRc/IRc, respectively.

Rational-Choice Model. The prediction from the standard rational-choice model that stage-1 investment choices are unaffected by the reward frame still holds in the control treatments. If decision makers correctly assess the value of winning the stage-1 interaction, which is composed of a sure win plus lottery in IRc, and of a payoff-equivalent lottery in SRc. Stage-1 investment choice are thus predicted to be identical across treatments:²³

$$x_1(\text{SRc}) = x_1(\text{IRc}). \quad (7)$$

Choice Bracketing. The choice-bracketing prediction is affected by the missing stage-2 investment choice, however. First, the fact that only one decision has to be made removes the most natural dimension around which decision makers might bracket, namely the two choices. Second, the decision problem is arguably much simpler in the control treatments, given that stage-2 winning probabilities are exogenously given rather than endogenously

²³Stage-1 investment is also predicted to be quantitatively somewhat higher than in the main treatments due to the lack of costly stage-2 investment in the control treatments.

Table 8: Experimental Results – Control Treatments

	SRc	IRc
Stage-1 Investment (x_1^*)	59.906 (4.105)	47.944 (2.492)

Note: The numbers denote average investment observed in all rounds across individuals (in experimental currency). Standard errors are provided in parentheses.

determined by the choices of the two decision makers competing on stage-2. In particular, in the control treatments, there is no need for decision makers to think either about own stage-2 investment or stage-2 investment by the opponent when choosing own stage-1 investment. In this sense, even decision makers with cognitive capacity limitations are likely to take all rewards into account in the control treatments. This suggests that stage-1 investments are likely to be equal across the control treatments even in a model that incorporates choice bracketing. However, one might argue that the predictions regarding stage-1 investments of decision makers who bracket narrowly are even reversed in the control treatments. In particular, decision makers who succeed on stage 1 of treatment **SRc** receive a sure and immediate reward of 72, and on top a lottery that delivers 96 or 0 with equal probabilities. A decision maker who is successful on stage 1 of treatment **IRc**, however, receives no immediate reward that is directly associated to the investment choice; the only reward is associated with the lottery that delivers 168 or 72 with equal probabilities on stage 2 – see Figure 6. In this sense, reward and choice are disconnected in **IRc**, but not in **SRc**. This implies that decision makers who fail to integrate rewards that are linked to choices and lottery rewards – and in this sense bracket narrowly – might actually invest higher amounts in **SRc** than in **IRc** simply because they assign different values to rewards that are linked to choices as compared to rewards that are linked to a lottery. Taken together, the impact of choice bracketing on stage-1 investments of decision makers in treatments **SR** and **IR** should either be absent in the control treatments **SRc** and **IRc** given that decision makers can no longer bracket around their choices and that complexity is reduced – or even reversed if decision makers fail to integrate rewards that are linked to choices and lottery rewards:

$$x_1(\mathbf{SRc}) \geq x_1(\mathbf{IRc}) . \tag{8}$$

Results. Table 8 displays the average stage-1 choices across subjects in treatments SRc and IRc. In contrast to the pattern observed in treatments SR and IR, subjects’ stage-1 investment in the control treatments is now higher (not lower) in the ‘separate-reward’ than in the ‘immediate-reward’ frame (59.906 vs. 47.944). Table 9 presents the corresponding regressions for statistical inference, using the same empirical framework as before. Column (1) estimates the average treatment effect across all decision rounds controlling for round fixed effects. The estimated coefficient suggests that the difference in mean stage-1 investment is statistically significant. In particular, stage-1 investment is significantly higher in SRc than in IRc. This suggests that subjects fail to fully integrate lottery rewards when making their investment choice. In particular, it appears that subjects discount rewards they receive from a lottery as compared to rewards that can be linked to investment choices. This pattern is consistent with decision making under narrow bracketing in the sense that decision makers fail to integrate all payoff consequences of their actions into their choices. Columns (2) and (3) indicate that this relation across control treatments SRc and IRc is not as robust as the corresponding (opposite) result we observed for our main treatments SR and IR – potentially due to the comparatively lower number of independent observations. In particular, the estimated coefficient for the treatment effect is not statistically significant once control variables are added – see Column (2) – and only weakly significant and lower when restricting attention to the second half of the experiment.²⁴

When analysing the data at the individual level, we find no evidence that subjects with comparatively low cognitive abilities are particularly prone to choice bracketing in the control treatments.²⁵ In particular, using the self-reported math grade as a proxy for cognitive capacity of experimental subjects, the treatment effect for subjects who are satisfactory or worse in math, or good or very good in math, respectively, is almost identical – in contrast to what we observed in the main treatments. Consequently, it seems that cognitive capacity is not a general proxy for narrow bracketing, but instead only important in sufficiently complex decision environments. Said differently, the failure of experimental subjects to integrate deterministic and stochastic rewards appears to be unrelated to cognitive capacity. This might explain why previous studies that rely on choices between simple lotteries and that thus consider decision environments with a degree of complexity that is similar to the one in the control treatments – as, e.g., Rabin

²⁴Table 14 in Appendix A presents the corresponding estimation results for investment choices averaged at the session level. In this case, the respective difference across control treatments is only significant at the 10% level in the baseline specification in Column (1), and not significant when considering medians (in Column (2)) or restricting attention to the second half of the experiment (in Column (3)).

²⁵If anything, the estimated coefficient across all decision rounds is slightly higher for subjects who are good or very good in math, as compared to subjects who are satisfactory or worse. The coefficient estimates are statistically insignificant in both cases, however. Details available upon request.

Table 9: Testing Equality of Stage-1 Investment Across Control Treatments

	STAGE-1 INVESTMENT		
	(1)	(2)	(3)
Treatment SRc	11.962 (0.021) [3.970; 19.95]	8.413 (0.209) [-4.477; 21.304]	5.897 (0.084) [-0.796; 12.590]
Rounds	ALL	ALL	16–30
Round FE	YES	YES	YES
Controls	NO	YES	NO
Sessions	4	4	4
Individuals	80	71	80
Obs.	2,400	2,130	1,200

Note: Random-effects panel estimates; in parentheses: p-values based on bootstrapped robust standard errors clustered on session level; in brackets: 95% confidence interval of coefficient estimate based on standard errors clustered on individual and session level (bootstrapped). **SRc** is a dummy variable that equals 1 in treatment **SRc** and 0 in treatment **IRc**. Control variables include gender, risk attitudes, econ major, self-reported math scores, as well as the number of mistakes in the control questions.

and Weizsäcker (2009) – find no link between measures of cognitive capacity and the propensity of decision makers to bracket narrowly.

Just as in the main treatments, however, we still find that subjects whose choices exhibit risk aversion in small-stakes lotteries are more likely to bracket narrowly. The respective regression output is provided in Table 10.²⁶ Column (1) provides the estimated treatment effect for the subgroup of risk neutral or risk seeking subjects. Even though the estimated coefficient is positive, it is far from being significant at conventional levels. According to Column (2), however, the treatment effect is much larger and highly significant for the subgroup of subjects whose choices exhibit risk aversion in small-stakes lotteries. This suggests that the failure of subjects to integrate their wealth when evaluating small-stakes lotteries might indeed be a proxy for narrow-bracketing behavior in other settings. When further disaggregating the data of this subgroup by decision rounds, however, we find that the treatment effect disappears as subjects gain experience. In particular, the estimated coefficient is sizeable and highly significant in the first half of the experiment (see Column (3) for details), but much smaller and insignificant in the second half of the experiment according to Column (4) – in contrast to what we find in the main treatments, suggesting that experience may facilitate broad bracketing in comparatively simple, but

²⁶For details on the estimation, please refer to the table notes.

Table 10: Small-Stakes Risk Aversion and Stage-1 Investment Across SRc and IRc

	STAGE-1 INVESTMENT			
	risk neutral/seeking	risk averse		
	(1)	(2)	(3)	(4)
Treatment SRc	7.771 (0.238) [-5.140; 20.681]	16.535 (0.026) [1.941; 31.128]	29.141 (0.004) [9.482; 48.799]	3.929 (0.419) [-5.592; 13.449]
Rounds	ALL	ALL	1–15	16–30
Round FE	YES	YES	YES	YES
Sessions	4	4	4	4
Individuals	41	32	32	32
Obs.	1230	960	480	480

Note: Random-effects panel estimates; in parentheses: p-values based on bootstrapped robust standard errors clustered on session level; in brackets: 95% confidence interval of coefficient estimate based on standard errors clustered on individual and session level (bootstrapped). SRc is a dummy variable that equals 1 in treatment SRc and 0 in treatment IRc.

not in complex decision environments.

Overall, stage-1 investment choices in the control treatments are clearly affected differently by the framing of rewards than the corresponding choices in the main treatments SR and IR. In particular, we find no evidence that decision makers omit the option value of stage-2 participation in a ‘separate-reward’ frame once endogenous, choice related winning probabilities are replaced by exogenous winning probabilities that are unrelated to both own investment choices and investment choices of the opponent. This indicates that the complex decision environment and the possibility to bracket narrowly around choices matter for the stage-1 investment difference we observe across the main treatments SR and IR. In addition, the control treatments provide evidence for a different type of narrow bracketing according to which it matters whether choices lead to immediate rewards as in SRc, or instead to indirect gains as a consequence of lottery outcomes (as in IRc). Behavior in the control treatments suggests that the failure of experimental subjects to integrate deterministic and stochastic rewards appears to be unrelated to cognitive capacity. This suggests that cognitive capacity limitations induce narrow bracketing only if the decision environment is sufficiently complex. Risk aversion in small stakes lotteries appears to be a proxy for narrow bracketing behavior both in simple and complex decision environments, however.

5 Discussion and Alternative Explanations

The previous analysis not only shows that the treatment effect observed across the main treatments **SR** and **IR** is consistent with the choice bracketing explanation, but additionally provides evidence indicating that plausible determinants of choice-bracketing behavior such as cognitive capacity limitations or elicited risk aversion in small-stakes lotteries matter for the effect we observe. Finally, evidence from the control treatments shows that the treatment effect can be reversed if reward and choice are disconnected and complexity is reduced – consistent with the presumption that decision makers who bracket narrowly fail to integrate rewards linked to choices and lottery rewards. One might nevertheless argue that there are alternative explanations for the observed pattern. In the following, we discuss some plausible candidates.

Reward Frame Differences That Are Unrelated to Choice Bracketing. It could be that aspects of the reward frame that are unrelated to the choice-bracketing explanation – such as the sequence of events, the label attached to rewards in the instructions, or the particular two-digit and three-digit numbers chosen for these rewards – explain the treatment effect we observe across the main treatments **SR** and **IR**. In particular, decision makers in treatment **IR** might make their stage-1 decisions based on the information that they receive either 168 or 72 units, provided they reach stage 2. In other words, subjects in **IR** choose stage-1 investments in the face of the total amount of prize money that can be appropriated in the contest. In contrast, subjects in treatment **SR** might decide on their stage-1 investment having in mind that they first compete for 72 units on stage 1 and that in addition, a reward of 96 units can be won if they succeed on stage 2. In this sense, the reward frame in treatment **SR** essentially hides the 72-unit reward for the winner in the parallel stage-1 interaction, such that the total rewards might appear smaller in **SR** than in **IR**. Alternatively, it could be that decision makers invest more on stage 1 of the ‘integrated-reward’ frame than on stage 1 of the ‘separate-reward’ frame because they prefer later rewards over immediate rewards – in contrast to predictions of standard discounting models that would suggest the opposite. The problem with these explanations is that the main and the control treatments are identical in all these dimensions. Consequently, treatment effects should have the same sign across main and control treatments if one of these aspects were important. However, the data reveal that main and control treatments deliver different behavioral patterns. Given that the treatment effects appear to work in opposite directions across main and control treatments, reward frame differences that are unrelated to the choice-bracketing explanation are unlikely to matter for the observed pattern.

Loss Aversion, Myopic Loss Aversion, and Probability Weighting. The literature has typically considered choice bracketing and loss aversion in combination even though these behavioral concepts are not necessarily related. One might nevertheless conjecture that loss aversion without choice bracketing can rationalize the observed pattern. Before we discuss how loss aversion might affect behavior across treatments, note that experimental subjects cannot make losses in the experiment, since the lowest possible payoff in any decision round is determined by the share of the endowment that is not invested and by rewards that are received. Given that subjects receive the same endowment in every decision round and that transfers across decision rounds are impossible, payoffs are necessarily positive in every round. Moreover, subjects receive feedback after every decision round about their positive payoff. Loss aversion could thus only matter if subjects ignore their endowment when choosing their stage-1 and stage-2 investment and when computing their payoffs – even though they are informed that the (remaining) endowment determines the amount that can be invested, and even though they repeatedly receive feedback on positive payoffs at the end of each decision round.

For the sake of completeness, assume that experimental subjects fail to take account of the endowment they receive. Consider a very simple representation of loss aversion in terms of a piece-wise linear utility function.²⁷ The optimization problem of a loss-averse agent i who competes against another loss-averse agent with the same degree of loss aversion can then be written as

$$\max_{x_i} p(x_i, x_j)[P - x_i] + [1 - p(x_i, x_j)]b(-x_i)$$

where P is the reward, x_j is the investment of the opponent, and $b > 1$ is the degree of loss aversion applied to a reference point of zero. In this contest, the equilibrium investment is given by $x_i = \frac{P}{3+b}$. If decision makers bracket broadly and have the same reference point (that is related to final outcomes) in both treatments, the predictions for the differences across our two main treatments **SR** and **IR** are the same as for the standard rational-choice framework. Intuitively, the only effect of loss aversion on choices is that equilibrium investments are below the rational-choice benchmark with risk-neutral agents in both stages of the main treatments **SR** and **IR**, as well as on stage-1 of the control treatments for any $b > 1$.

Even though loss aversion without choice bracketing cannot rationalize the observed pattern, one might still argue that myopic loss aversion – a combination of loss aversion and narrow bracketing – can better explain what we observe. In order to address this

²⁷See, e.g., Cornes and Hartley (2012) for a similar representation.

point, assume the same representation of loss aversion as before, and that decision makers bracket choices narrowly in **SR**, but not in **IR** (as suggested by the findings in the main treatments). Such a setting delivers similar predictions as a model with choice bracketing alone. To see this, suppose decision makers integrate their gain for winning stage 1 in treatment **SR** when choosing their stage-2 investment. This delivers the same set of predictions as for narrow bracketing without loss aversion.²⁸ Consequently, myopic loss aversion cannot better explain the observed pattern in the data, suggesting that choice bracketing rather than loss aversion is the relevant element for the explanation.

Finally, one might argue that probability weighting matters. It is not entirely clear how probability weighting might affect behavior in the main treatments. In particular, it is unclear how to think of probability weighting in a setting of strategic interactions where winning probabilities are determined endogenously rather than imposed exogenously, and where the equilibrium probability is 0.5. Probability weighting could be important in the control treatments, however, since the stage-1 investment decision is much simpler and the stage-2 lottery comprises exogenous probabilities. In particular, decision makers face only one investment decision in the controls, and they do not have to form expectations regarding choices of future opponents. Presume that stage-1 investment choices are affected by the value that decision makers attach to the compound lottery that delivers 72 for sure plus 96 and 0 with probability 0.5 each, relative to the value they attach to the simple lottery that delivers 168 and 72 with probability 0.5 each. If the weight attached to a probability of 0.5 is close to 0.5 – as suggested by, e.g., Fehr-Duda and Epper (2012) – probability weighting would not greatly affect the theoretical predictions. Evidence by Tversky and Kahneman (1992) or van der Kuilen and Wakker (2011), however, suggests that the weight attached to a probability of 0.5 might in fact be slightly below 0.4, implying that rewards are seen as more valuable in **SRc** than in **IRc** – consistent with what we observe in the experimental data. In particular, the value of winning in **SRc** would then be $1 \cdot 72 + 0.4 \cdot 96 = 110.4$, compared to $0.4 \cdot 72 + 0.4 \cdot 168 = 96$ in **IRc**. Investments are close to half of the reward in the main treatments, suggesting empirical investment levels of roughly 55 in **SRc** and 48 in **IRc**. Given that this is close to what we observe, probability weighting is a potential alternative to our preferred explanation based on narrow bracketing.

Summing up, loss aversion is unlikely to matter for the treatment effect we observe, since the endowment ensures positive payoffs in every decision round – independent of the choices made. Moreover, loss aversion cannot explain the observed patterns in isolation, and myopic loss aversion delivers the same predictions as the choice-bracketing model

²⁸In particular, we would expect that $x_2(\mathbf{SR}) = x_2(\mathbf{IR})$ and $x_1(\mathbf{SR}) < x_1(\mathbf{IR})$ hold.

without loss aversion. Probability weighting, however, might potentially be relevant for the behavioral patterns observed in the control treatments.

Status Concerns and Joy of Winning. An alternative explanation for the observed behavior could be status concerns related to the performance in the contest, or joy of winning. Joy of winning alone cannot explain the treatment differences, however, independent of whether the concept is applied to the entire interaction or separately to each of the two stages. To see this, notice that joy of winning would somehow have to be related to the reward at stake at each stage, either by complementing or substituting a monetary reward. Maintaining the standard assumption of broad bracketing, final rewards are identical across both treatments, as should be the psychological benefit of prevailing on all stages of the contest. In order to explain why stage-1 investment in IR (which involves no immediate monetary reward) exceeds stage-1 investment in SR (which involves an immediate monetary reward), one might be tempted to assume that the joy of winning is inversely related to the respective reward at stake. If joy of winning substitutes monetary rewards (thus delivering higher investments when the immediate reward on stage 1 is zero) this might indeed explain the observed treatment difference in stage-1 investments. However, this argument itself involves some sort of choice bracketing and focusing on nominal values, and it is contradicted by the finding of equal investment on stage 2 (where rewards are unequal). Moreover, the reverse finding in the control treatments – where the reward structure is identical to the main treatment – also speaks against this explanation. Taken together, while it is not implausible that joy of winning affects investment choices, it seems hard to construct an explanation for the observed behavioral patterns in the main and the control treatments based on such a mechanism without the need for additional assumptions that are potentially even stronger than those invoked by the choice-bracketing explanation.

6 Concluding Remarks

This paper has presented novel evidence for the prevalence and the determinants of narrow choice bracketing from lab experiments that implement framing variations of the reward structure in two-stage elimination contests. The experimental results suggest that different frames affect behavior of decision makers by influencing their perception of the consequences associated with their decisions. In particular, the data suggest that decision makers bracket narrowly and treat stage-1 and stage-2 investment choices as separate if success on each stage of the contest is separately rewarded, as in the “separate reward”

frame. If rewards for success are integrated across stages, however, as in the “integrated reward” frame where stage-1 winners receive no immediate reward for winning stage 1, decision makers appear to bracket broadly instead and take account of future stage-2 rewards already when competing on stage 1. When disaggregating the data by individual characteristics to investigate potential determinants of narrow bracketing, we find that cognitive limitations influence whether and to what extent decision makers bracket narrowly in complex decision environments. Using self-reported math grades as a proxy for cognitive ability, it appears that the effect of the reward frame on behavior quickly disappears for subjects with relatively high cognitive skills as they gain experience. Subjects with comparably lower cognitive skills tend to bracket choices narrowly throughout all decision rounds, however. Evidence from control treatments suggests that cognitive limitations do not affect whether and to what extent decision makers bracket narrowly in simpler decision environments. When interpreting this finding, one should keep in mind that all subjects in the sample are university students and thus more likely to belong to the top than to the bottom half of the ability distribution. Given that even these subjects tend to bracket narrowly on average, choice bracketing is likely to be an even more important behavioral determinant for the general population.

The results of this paper have important practical implications for the design of promotion contests. When thinking about the sequence of events in typical promotion contests, the “separate reward” frame, which induces narrow rather than broad bracketing, appears to be the one of more practical relevance. In particular, it appears natural to assume that workers who are promoted from one hierarchy level to the next receive an immediate reward for this promotion in terms of a pay rise – as in the “separate reward” frame. On the other hand, it seems unrealistic to stipulate that the reward for promotion from one level to the next is delayed until the identity of the worker who is promoted to the top-level position is determined – as in the “integrated reward” frame. This implies for human resource management that in realistic career ladder environments employees might not fully account for the option value of future promotions. This highlights the importance of explicitly communicating the potential future gains inherent in any promotion to employees to avoid narrow bracketing on a given career stage. In addition, our findings suggest that it might be an optimal strategy to delay rewards in order to induce broad bracketing by decision makers. In particular, the combination of low immediate and high future rewards might help to ensure that decision makers bracket broadly. Even though future benefits are likely to be discounted in reality, they might help to induce forward-looking behavior, providing an alternative reason for the preference for a particular timing of the revelation of information than investigated in the literature (e.g, Zimmermann, 2015). In this sense,

the results presented in this paper provide an alternative behavioral explanation for the frequent use of strongly convex reward structures in corporate contexts.

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Appendix

A Additional Results

Table 11: Testing Equality of Stage-2 Investment Across Treatments

	STAGE-2 INVESTMENT		
	Mean (1)	Median (2)	Mean (3)
Treatment IR	3.504 (4.338)	1.973 (4.507)	0.843 (3.887)
Rounds	ALL	ALL	16–30
Round FE	YES	YES	YES
Groups	10	10	10
Obs.	300	300	150

Note: Random-effects panel estimates, robust standard errors (in parentheses). IR is a dummy variable that equals 1 in treatment IR and 0 in treatment SR. Session means for stage-2 investment are the outcome variable in columns (1) and (3), while median values are considered in column (2).

Table 12: Testing Equality of Stage-1 Investment Across Treatments

	STAGE-1 INVESTMENT		
	Mean (1)	Median (2)	Mean (3)
Treatment IR	9.744*** (3.414)	3.277*** (1.010)	5.235** (2.547)
Rounds	ALL	ALL	16–30
Round FE	YES	YES	YES
Groups	10	10	10
Obs.	300	300	150

Note: Random-effects panel estimates, robust standard errors (in parentheses). IR is a dummy variable that equals 1 in treatment IR and 0 in treatment SR. Session means for stage-1 investment are the outcome variable in columns (1) and (3), while median values are considered in column (2).

Table 13: Individual Characteristics by Treatment

	SR			IR		
	N	MEAN	S.D.	N	MEAN	S.D.
Econ Department	100	0.47	0.502	100	0.46	0.501
Gender (male=1)	100	0.59	0.494	100	0.49	0.502
Math Grade	96	2.427	1.129	98	2.360	1.115
Risk Attitude	90	10.289	4.453	87	10.126	4.220
Control Questions	100	1.030	1.087	100	1.200	1.287

Note: Subjects with inconsistent risk preferences were excluded. A switching point at 11 is optimal for risk-neutral individuals; therefore, values below (above) 11 indicate that a subject is risk averse (loving). Grades in mathematics are on a scale from 1 to 6, where 1 is the best grade.

Table 14: Testing Alternative Explanations: Results from Control Treatments

	STAGE-1 INVESTMENT		
	Mean (1)	Median (2)	Mean (3)
Treatment IRc	-11.962* (6.792)	-5.897 (3.677)	-5.658 (8.359)
Rounds	ALL	ALL	16–30
Round FE	YES	YES	YES
Groups	4	4	4
Obs.	120	120	60

Note: We use a random-effects panel estimator with robust standard errors (in parentheses). IRc is a dummy variable that equals 1 in treatment IRc and 0 in treatment SRc. Session means for stage-1 investment are the outcome variable in columns (1), (2), and (3), while median values are considered in column (4).

B Experimental Instructions

The experimental instructions consist of three parts: First, experimental subjects receive some general information about the experimental session. Then, they are informed about the main treatment (Experiment 1), which is either the **SR** or the **IR** specification (both versions are provided). Finally, subjects receive instructions for the elicitation of risk attitudes (Experiment 2).

WELCOME TO THIS EXPERIMENT AND THANK YOU FOR YOUR PARTICIPATION

General Instructions:

You will participate in 2 different experiments today. Please stop talking to any other participant of this experiment from now on until the end of this session. In each of the two experiments, you will have to make certain decisions and may earn an appreciable amount of money. Your earnings will depend upon several factors: on your decisions, on the decisions of other participants, and on random components, i.e. chance. The following instructions explain how your earnings will be determined.

The experimental currency is denoted **Taler**. In addition to your Taler earnings in experiments 1 and 2, you receive 3 EURO show-up fee. You may increase your Taler earnings in experiments 1 and 2, where 2 Taler equal 1 Euro-Cent, i.e.

200 Taler correspond to 1 Euro.

At the end of this experimental session your Taler earnings will be converted into Euro and paid to you in cash.

Before the experimental session starts, you receive a card with your participant number. All your decisions in this experiment will be entered in a mask on the computer, the same holds for all other participants of the experiment. In addition, the computer will determine the random components which are needed in some of the experiments. All data collected in this experiment will be matched to your participant number, **not** to your name or student number. Your participant number will also be used for payment of your earnings at the end of the experimental session. Therefore, your decisions and the information provided in the experiments are completely anonymous; neither the experimenter nor anybody else can match these data to your identity.

We will start with experiment 1, followed by experiment 2. The instructions for experiment 2 will only be distributed right before this experiment starts, i.e. subsequent to experiment 1.

You will receive your earnings in cash at the end of the experimental session.

Experiment 1 [IR]

Overall, there are 30 decision rounds with two stages each in Experiment 1. The course of events is the same in each decision round. You will be randomly and anonymously placed into a group of **four participants** in each round, and the identity of participants in your group changes with each decision round.

Course of events in an arbitrary decision round

All four participants of each group receive an **endowment of 240 Taler** at the beginning of a decision round. The endowment can be used to buy a certain amount of balls in two subsequent stages of a decision round. It is important to note that you receive one endowment only which must suffice to buy balls in both stages. The costs for the purchase of a ball are the same for all participants: Participants have to pay **1.00 Taler** for each ball they buy in stage 1 **or** stage 2, i.e.

1 ball costs 1.00 Taler
2 balls cost 2.00 Taler
(and so on)

When deciding how many balls you want to buy, you do not know the decision of other participants. Also, your decision is not revealed to any other participant.

All interactions in the experiment are pairwise. Assume that you are in one group with participant A, participant B, and participant C. Then, you interact with participant A in stage 1, while participants B and C simultaneously meet each other in the second stage 1 interaction. If you reach stage 2, you will interact either with participant B or C, depending on the outcome in the second stage 1 interaction. In stage 1, there are two ballot boxes:

- all balls bought by you or participant A are placed in ballot box 1
- all balls bought by participants B and C are placed in ballot box 2

One ball is randomly drawn from each ballot box, and each ball drawn with the same probability. The two participants whose balls are drawn from ballot box 1 and 2, respectively, reach stage 2; the decision round is over for the other two participants (whose balls were not drawn), i.e. they drop out from this decision round. Any participant has to pay the balls he or she bought in stage 1, whether or not he/she reached stage 2. The respective amount is deducted from the endowment.

The two participants who reached stage 2 do again buy a certain number of balls, using whatever remains from the endowment they received after costs for balls in stage 1 were deducted. The balls are then placed into ballot box 3. One ball is randomly drawn from ballot box 3. The participant whose ball is drawn receives the main prize of **168 Taler**. The other participant of stage 2, whose ball is not drawn from ballot box 3, receives a runner-up prize of **72 Taler**. Independent of the prize which a stage 2 participant receives, he/she does always have to pay for the balls bought in stage 2. Participants who did not reach stage 2 do not receive any prize.

Let's take a closer look at the random draw of balls from ballot boxes. Assume, for example, that all balls which you bought are green colored, and that you interact with participant A in stage 1. Then, the probability that one of your balls is drawn (such that you make it to stage 2) satisfies

$$\text{probability}(\text{green ball is drawn}) = \frac{\# \text{ green balls}}{\# \text{ green balls} + \# \text{ balls by participant A}}$$

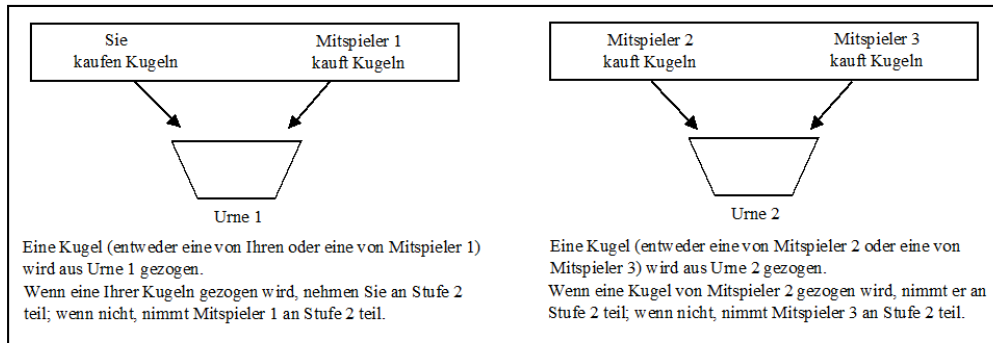
where # is short for number. The same probability rule does also hold for other participants in your group. Consequently, the probability that one of your balls is drawn is higher

- the more balls you purchased
- the less balls the other participant with whom you interact purchased.

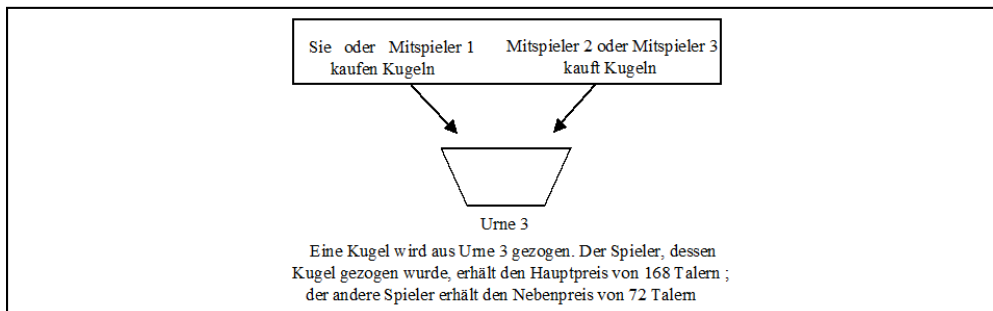
The computer simulates the random draw of a ball. If all participant of a group of two choose to buy zero balls, each participant wins with the same probability of 50%.

Jeder Spieler erhält eine Anfangsausstattung von 240 Talern. Er muss damit alle von ihm in Stufe 1 und Stufe 2 gekauften Kugeln bezahlen.

STUFE 1



STUFE 2



Your Payoff

Assume that you bought "X1" balls in stage 1, and that you buy "X2" balls whenever you reach stage 2. Then, there are three possibilities for your payoff:

- 1) None of your balls is drawn in stage 1
Your Payoff = endowment - X1 * 1 Taler
= 240 Taler - X1 * 1 Taler
- 2) one of your balls is drawn from the ballot box in stage 1; in stage 2, none of your balls is drawn
Your Payoff = endowment - X1 * 1 Taler - X2 * 1 Taler + runner up prize
= 240 Taler - X1 * 1 Taler - X2 * 1 Taler + 72 Taler
- 3) one of your balls is drawn from the ballot box in stage 1; also, one of your balls is drawn in stage 2
Your Payoff = endowment - X1 * 1 Taler - X2 * 1 Taler + main prize
= 240 Taler - X1 * 1 Taler - X2 * 1 Taler + 168 Taler

Therefore, your payoff is determined by the following components: by the number of balls you buy in stage 1 ("X1"); by the number of balls you buy in stage 2 ("X2") if you reach it; by up to two random draws (one of your balls is drawn/not drawn in stage 1 and potentially stage 2). The same holds for any other participants of the experiment.

Information:

- After you made your decision in stage 1, you are informed whether or not you can participate in stage 2, i.e. whether or not one of your balls was drawn from ballot box 1.
- If you did not reach stage 2, you are informed about how many balls participant A bought in stage 1.

- If you reach stage 2, you receive information about the remaining endowment (after costs for the purchase in stage 1 are deducted).
- After you made your decision in stage 2, you learn whether or not one of your balls was drawn from ballot box 3 and how many balls the participants who you met in stages 1 and 2, respectively, bought. Further, you learn your payoff for the respective decision round.

Decision: In each of the 30 decision rounds you have to decide how many balls you want to buy in stage 1. If you reach stage 2, you face a similar decision in stage 2. In both cases, you have to enter a number into a field on the computer screen. An example of the decision screen in stage 1 is shown below.

Experiment 1: Entscheidungsrunde 1 von 30

STUFE 1:

Sie haben insgesamt **240.00 Taler** zum Kauf von Kugeln auf beiden Spielstufen zur Verfügung

In beiden Spielstufen gilt:
 Sie müssen **1.00 Taler** pro Kugel bezahlen
 Ihr jeweiliger Mitspieler muss **1.00 Taler** pro Kugel bezahlen

Mit dem Kauf von Kugeln auf STUFE 1 erhöhen Sie Ihre Chancen an STUFE 2 teilzunehmen
 Wenn in STUFE 2 eine Ihrer Kugeln gezogen wird, erhalten Sie den Hauptpreis von **168.00 Taler**
 Wenn in STUFE 2 eine der Kugeln Ihres Mitspielers gezogen wird, erhalten Sie den Nebenpreis von **72.00 Taler**

Wie viele Kugeln wollen Sie in STUFE 1 kaufen?

(Bitte geben Sie eine Zahl zwischen 0 und 240 in das obige Feld ein)

Your Total Payoff: Four out of 30 decision rounds are paid. These rounds are randomly determined, i.e., the probability that some decision round is paid is identical ex-ante for all 30 decision rounds. You will receive the sum of payoffs for the respective decision rounds.

Remember:

You receive an endowment of 240 Taler at the beginning of each decision round and have to decide how many balls you want to buy in stage 1; if you reach stage 2, you have to decide again. Overall, there are three additional participants in each group who face the same problem. The identity of these participants is randomly determined in each decision round. Every participant has to pay **1.00 Taler** for each ball he/she buys in stage 1 **or** stage 2. Two prizes are awarded: the main prize of 168 Taler for the participant whose ball is drawn in stage 2, and the runner-up prize of 72 Taler for the other participant of the stage-2 interaction.

If you have any questions, please raise your hand now!

Experiment 1 [SR]

Overall, there are 30 decision rounds with two stages each in Experiment 1. The course of events is the same in each decision round. You will be randomly and anonymously placed into a group of **four participants** in each round, and the identity of participants in your group changes with each decision round.

Course of events in an arbitrary decision round

All four participants of each group receive an **endowment of 240 Taler** at the beginning of a decision round. The endowment can be used to buy a certain amount of balls in two subsequent stages of a decision round. It is important to note that you receive one endowment only which must suffice to buy balls in both stages. The costs for the purchase of a ball are the same for all participants: Participants have to pay **1.00 Taler** for each ball they buy in stage 1 **or** stage 2, i.e.

1 ball costs 1.00 Taler
2 balls cost 2.00 Taler
(and so on)

When deciding how many balls you want to buy, you do not know the decision of other participants. Also, your decision is not revealed to any other participant.

All interactions in the experiment are pairwise. Assume that you are in one group with participant A, participant B, and participant C. Then, you interact with participant A in stage 1, while participants B and C simultaneously meet each other in the second stage 1 interaction. If you reach stage 2, you will interact either with participant B or C, depending on the outcome in the second stage 1 interaction. In stage 1, there are two ballot boxes:

- all balls bought by you or participant A are placed in ballot box 1
- all balls bought by participants B and C are placed in ballot box 2

One ball is randomly drawn from each ballot box, and each ball drawn with the same probability. The two participants whose balls are drawn from ballot box 1 and 2, respectively, receive an intermediate prize of **72 Taler** and reach stage 2; the decision round is over for the other two participants (whose balls were not drawn), i.e. they drop out from this decision round and receive no prize. Any participant has to pay the balls he or she bought in stage 1, whether or not he/she reached stage 2. The respective amount is deducted from the endowment.

The two participants who reached stage 2 do again buy a certain number of balls, using whatever remains from the endowment they received after costs for balls in stage 1 were deducted. This implies that the intermediate prize which stage 2 participants received at the end of stage 1 cannot be used to purchase balls. All purchased balls are placed into ballot box 3. One ball is randomly drawn from ballot box 3. The participant whose ball is drawn receives the main prize of **96 Taler**. Independent of whether or not a participant receives the main prize, he/she does always have to pay for the balls bought in stage 2.

Let's take a closer look at the random draw of balls from ballot boxes. Assume, for example, that all balls which you bought are green colored, and that you interact with participant A in stage 1. Then, the probability that one of your balls is drawn (such that you make it to stage 2) satisfies

$$\text{probability}(\text{green ball is drawn}) = \frac{\# \text{ green balls}}{\# \text{ green balls} + \# \text{ balls by participant A}}$$

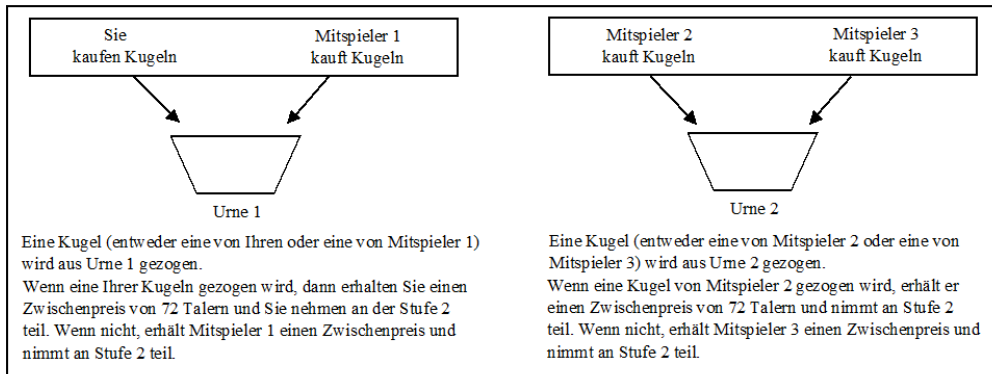
where # is short for number. The same probability rule does also hold for other participants in your group. Consequently, the probability that one of your balls is drawn is higher

- the more balls you purchased
- the less balls the other participant with whom you interact purchased.

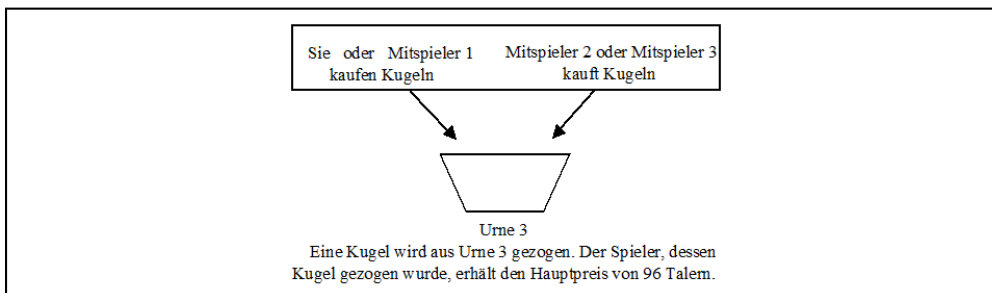
The computer simulates the random draw of a ball. If all participant of a group of two choose to buy zero balls, each participant wins with the same probability of 50%.

Jeder Spieler erhält eine Anfangsausstattung von 240 Talern. Er muss damit alle von ihm in Stufe 1 und Stufe 2 gekauften Kugeln bezahlen.

STUFE 1



STUFE 2



Your Payoff

Assume that you bought "X1" balls in stage 1, and that you buy "X2" balls whenever you reach stage 2. Then, there are three possibilities for your payoff:

- 1) None of your balls is drawn in stage 1
Your Payoff = endowment - X1 * 1 Taler
 = 240 Taler - X1 * 1 Taler
- 2) one of your balls is drawn from the ballot box in stage 1; in stage 2, none of your balls is drawn
Your Payoff = endowment - X1 * 1 Taler - X2 * 1 Taler + intermediate prize
 = 240 Taler - X1 * 1 Taler - X2 * 1 Taler + 72 Taler
- 3) one of your balls is drawn from the ballot box in stage 1; also, one of your balls is drawn in stage 2
Your Payoff = endowment - X1 * 1 Taler - X2 * 1 Taler + main prize
 = 240 Taler - X1 * 1 Taler - X2 * 1 Taler + 168 Taler

Therefore, your payoff is determined by the following components: by the number of balls you buy in stage 1 ("X1"); by the number of balls you buy in stage 2 ("X2") if you reach it; by up to two random draws (one of your balls is drawn/not drawn in stage 1 and potentially stage 2). The same holds for any other participants of the experiment.

Information:

- After you made your decision in stage 1, you are informed whether or not you can participate in stage 2, i.e. whether or not one of your balls was drawn from ballot box 1.
- If you did not reach stage 2, you are informed about how many balls participant A bought in stage 1.

- If you reach stage 2, you receive information about the remaining endowment (after costs for the purchase in stage 1 are deducted).
- After you made your decision in stage 2, you learn whether or not one of your balls was drawn from ballot box 3 and how many balls the participants who you met in stages 1 and 2, respectively, bought. Further, you learn your payoff for the respective decision round.

Decision: In each of the 30 decision rounds you have to decide how many balls you want to buy in stage 1. If you reach stage 2, you face a similar decision in stage 2. In both cases, you have to enter a number into a field on the computer screen. An example of the decision screen in stage 1 is shown below.

Experiment 1: Entscheidungsrunde 1 von 30

STUFE 1:

Sie haben insgesamt **240.00 Taler** zum Kauf von Kugeln auf beiden Spielstufen zur Verfügung

In beiden Spielstufen gilt:
 Sie müssen **1.00 Taler** pro Kugel bezahlen
 Ihr jeweiliger Mitspieler muss **1.00 Taler** pro Kugel bezahlen

Mit dem Kauf von Kugeln auf STUFE 1 erhöhen Sie Ihre Chancen auf den Zwischenpreis von **72.00 Taler** sowie auf eine Teilnahme an STUFE 2.
 Wenn in STUFE 2 eine Ihrer Kugeln gezogen wird, erhalten Sie den Hauptpreis von **96.00 Taler**

Wie viele Kugeln wollen Sie in STUFE 1 kaufen?

(Bitte geben Sie eine Zahl zwischen 0 und 240 in das obige Feld ein)

Your Total Payoff: Four out of 30 decision rounds are paid. These rounds are randomly determined, i.e., the probability that some decision round is paid is identical ex-ante for all 30 decision rounds. You will receive the sum of payoffs for the respective decision rounds.

Remember:

You receive an endowment of 240 Taler at the beginning of each decision round and have to decide how many balls you want to buy in stage 1; if you reach stage 2, you have to decide again. Overall, there are three additional participants in each group who face the same problem. The identity of these participants is randomly determined in each decision round. Every participant has to pay **1.00 Taler** for each ball he/she buys in stage 1 **or** stage 2. If one of your balls is drawn in stage 1, you receive an intermediate prize of 72 Taler. If, in addition, one of your balls is drawn in stage 2, you additionally receive the main prize of 96 Taler.

If you have any questions, please raise your hand now!

Experiment 1 [IRc]

Overall, there are 30 decision rounds with two stages each in Experiment 1. The course of events is the same in each decision round. You will be randomly and anonymously placed into a group of **four participants** in each round, and the identity of participants in your group changes with each decision round.

Course of events in an arbitrary decision round

All four participants of each group receive an **endowment of 240 Taler** at the beginning of a decision round. The endowment can be used to buy a certain amount of balls in stage 1 of a decision round. The costs for the purchase of a ball are the same for all participants: Participants have to pay **1.00 Taler** for each ball they buy in stage 1, i.e.

1 ball costs 1.00 Taler
2 balls cost 2.00 Taler
(and so on)

When deciding how many balls you want to buy, you do not know the decision of other participants. Also, your decision is not revealed to any other participant.

Interactions in stage 1 of the experiment are pairwise. Assume that you are in one group with participant A, participant B, and participant C. Then, you interact with participant A in stage 1, while participants B and C simultaneously meet each other in the second stage 1 interaction. If you reach stage 2, you will meet either participant B or C, depending on the outcome in the second stage 1 interaction.

In stage 1, there are two ballot boxes:

- all balls bought by you or participant A are placed in ballot box 1
- all balls bought by participants B and C are placed in ballot box 2

One ball is randomly drawn from each ballot box, and each ball drawn with the same probability. The two participants whose balls are drawn from ballot box 1 and 2, respectively, reach stage 2; the decision round is over for the other two participants (whose balls were not drawn), i.e. they drop out from this decision round. Any participant has to pay the balls he or she bought in stage 1, whether or not he/she reached stage 2. The respective amount is deducted from the endowment.

The two participants who reached stage 2 meet each other on stage 2. One of the two participants is randomly selected and receives the main prize of **168 Taler**, while the other stage-2 participant receive a runner-up prize of **72 Taler**. Any participant who reaches stage 2 is selected with the same probability.

Let's take a closer look at the random draw of balls from ballot boxes. Assume, for example, that all balls which you bought are green colored, and that you interact with participant A in stage 1. Then, the probability that one of your balls is drawn (such that you make it to stage 2) satisfies

$$\text{probability}(\text{green ball is drawn}) = \frac{\# \text{ green balls}}{\# \text{ green balls} + \# \text{ balls by participant A}}$$

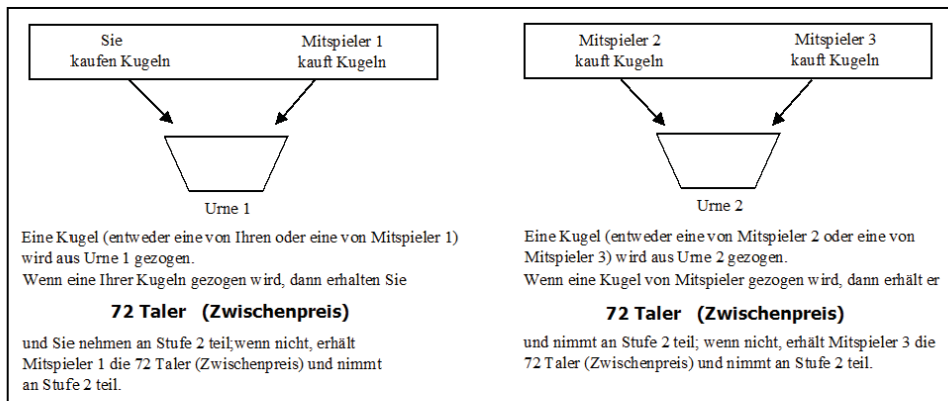
where # is short for number. The same probability rule does also hold for other participants in your group. Consequently, the probability that one of your balls is drawn is higher

- the more balls you purchased
- the less balls the other participant with whom you interact purchased.

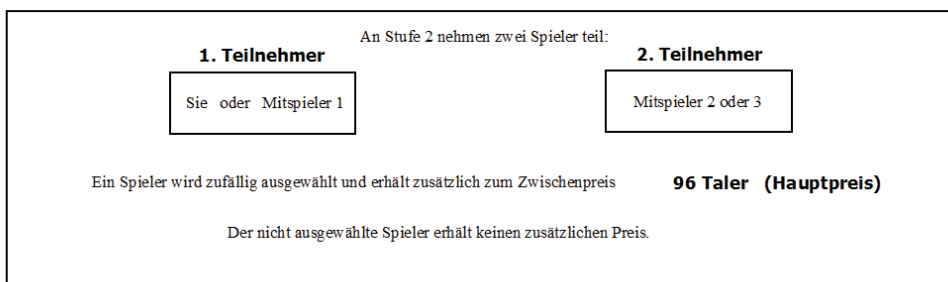
The computer simulates the random draw of a ball. If all participant of a group of two choose to buy zero balls, each participant wins with the same probability of 50%.

Jeder Spieler erhält eine Anfangsausstattung von 240 Talern. Er muss die von ihm in Stufe 1 gekauften Kugeln bezahlen.

STUFE 1



STUFE 2



Your Payoff

Assume that you bought "X" balls in stage 1. Then, there are three possibilities for your payoff:

- 1) None of your balls is drawn in stage 1
Your Payoff = endowment - X * 1 Taler
= 240 Taler - X * 1 Taler
- 2) one of your balls is drawn from the ballot box in stage 1; the other stage-2 participant is randomly selected in stage 2
Your Payoff = endowment - X * 1 Taler + runner up prize
= 240 Taler - X * 1 Taler + 72 Taler
- 3) one of your balls is drawn from the ballot box in stage 1; the other stage-2 participant is randomly selected in stage 2
Your Payoff = endowment - X * 1 Taler + main prize
= 240 Taler - X * 1 Taler + 168 Taler

Therefore, your payoff is determined by the following components: by the number of balls you buy in stage 1 ("X"), and by up to two random draws (one of your balls is drawn/not drawn in stage 1; you are randomly selected in stage 2 or not). The same holds for any other participants of the experiment.

Information:

- After you made your decision in stage 1, you are informed whether or not you can participate in stage 2, i.e. whether or not one of your balls was drawn from ballot box 1.
- If you did not reach stage 2, you are informed about how many balls participant A bought in stage 1.
- If you reach stage 2, you learn whether or not you were randomly selected by the computer to receive the main prize. Further, you learn your payoff for the respective decision round.

Decision: In each of the 30 decision rounds you have to decide how many balls you want to buy in stage 1. You have to enter a number into a field on the computer screen. An example of the decision screen in stage 1 is shown below.

[screenshot]

Your Total Payoff: Four out of 30 decision rounds are paid. These rounds are randomly determined, i.e., the probability that some decision round is paid is identical ex-ante for all 30 decision rounds. You will receive the sum of payoffs for the respective decision rounds.

Remember:

You receive an endowment of 240 Taler at the beginning of each decision round and have to decide how many balls you want to buy in stage 1. Overall, there are three additional participants in each group who face the same problem. The identity of these participants is randomly determined in each decision round. Every participant has to pay **1.00 Taler** for each ball he/she buys in stage 1. Two prizes are awarded: the main prize of 168 Taler for the participant whose is randomly selected in stage 2, and the runner-up prize of 72 Taler for the other participant of the stage 2 interaction.

If you have any questions, please raise your hand now!

Experiment 1 [SRC]

Overall, there are 30 decision rounds with two stages each in Experiment 1. The course of events is the same in each decision round. You will be randomly and anonymously placed into a group of **four participants** in each round, and the identity of participants in your group changes with each decision round.

Course of events in an arbitrary decision round

All four participants of each group receive an **endowment of 240 Taler** at the beginning of a decision round. The endowment can be used to buy a certain amount of balls in stage 1 of a decision round. The costs for the purchase of a ball are the same for all participants: Participants have to pay **1.00 Taler** for each ball they buy in stage 1, i.e.

1 ball costs 1.00 Taler
2 balls cost 2.00 Taler
(and so on)

When deciding how many balls you want to buy, you do not know the decision of other participants. Also, your decision is not revealed to any other participant.

Interactions in stage 1 of the experiment are pairwise. Assume that you are in one group with participant A, participant B, and participant C. Then, you interact with participant A in stage 1, while participants B and C simultaneously meet each other in the second stage 1 interaction. If you reach stage 2, you will meet either participant B or C, depending on the outcome in the second stage 1 interaction.

In stage 1, there are two ballot boxes:

- all balls bought by you or participant A are placed in ballot box 1
- all balls bought by participants B and C are placed in ballot box 2

One ball is randomly drawn from each ballot box, and each ball drawn with the same probability. The two participants whose balls are drawn from ballot box 1 and 2, respectively, receive an intermediate prize of **72 Taler** and reach stage 2; the decision round is over for the other two participants (whose balls were not drawn), i.e. they drop out from this decision round and receive no prize. Any participant has to pay the balls he or she bought in stage 1, whether or not he/she reached stage 2. The respective amount is deducted from the endowment.

The two participants who reached stage 2 meet each other on stage 2. One of the two participants is randomly selected and receives the main prize of

96 Taler

Any participant who reached stage 2 is selected with the same probability.

Let's take a closer look at the random draw of balls from ballot boxes. Assume, for example, that all balls which you bought are green colored, and that you interact with participant A in stage 1. Then, the probability that one of your balls is drawn (such that you make it to stage 2) satisfies

$$\text{probability}(\text{green ball is drawn}) = \frac{\text{\# green balls}}{\text{\# green balls} + \text{\# balls by participant A}}$$

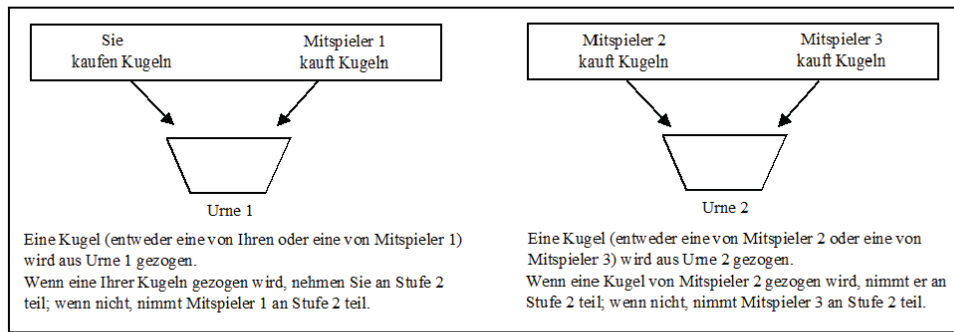
where # is short for number. The same probability rule does also hold for other participants in your group. Consequently, the probability that one of your balls is drawn is higher

- the more balls you purchased
- the less balls the other participant with whom you interact purchased.

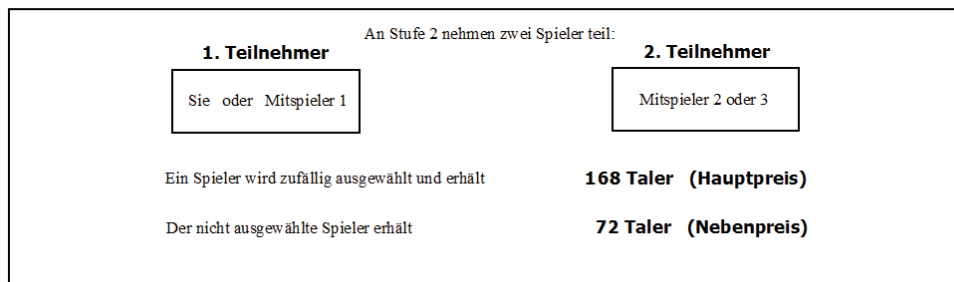
The computer simulates the random draw of a ball. If all participant of a group of two choose to buy zero balls, each participant wins with the same probability of 50%.

Jeder Spieler erhält eine Anfangsausstattung von 240 Talern. Er muss die von ihm in Stufe 1 gekauften Kugeln bezahlen.

STUFE 1



STUFE 2



Your Payoff

Assume that you bought "X" balls in stage 1. Then, there are three possibilities for your payoff:

- 1) None of your balls is drawn in stage 1
Your Payoff = endowment - X * 1 Taler
= 240 Taler - X * 1 Taler
- 2) one of your balls is drawn from the ballot box in stage 1; the other stage-2 participant is randomly selected in stage 2
Your Payoff = endowment - X * 1 Taler + intermediate prize
= 240 Taler - X * 1 Taler + 72 Taler
- 3) one of your balls is drawn from the ballot box in stage 1; the other stage-2 participant is randomly selected in stage 2
Your Payoff = endowment - X * 1 Taler + main prize
= 240 Taler - X * 1 Taler + 168 Taler

Therefore, your payoff is determined by the following components: by the number of balls you buy in stage 1 ("X"), and by up to two random draws (one of your balls is drawn/not drawn in stage 1; you are randomly selected in stage 2 or not). The same holds for any other participants of the experiment.

Information:

- After you made your decision in stage 1, you are informed whether or not you can participate in stage 2, i.e. whether or not one of your balls was drawn from ballot box 1.
- If you did not reach stage 2, you are informed about how many balls participant A bought in stage 1.
- If you reach stage 2, you learn whether or not you were randomly selected by the computer to receive the main prize. Further, you learn your payoff for the respective decision round.

Decision: In each of the 30 decision rounds you have to decide how many balls you want to buy in stage 1. You have to enter a number into a field on the computer screen. An example of the decision screen in stage 1 is shown below.

[screenshot]

Your Total Payoff: Four out of 30 decision rounds are paid. These rounds are randomly determined, i.e., the probability that some decision round is paid is identical ex-ante for all 30 decision rounds. You will receive the sum of payoffs for the respective decision rounds.

Remember:

You receive an endowment of 240 Taler at the beginning of each decision round and have to decide how many balls you want to buy in stage 1. Overall, there are three additional participants in each group who face the same problem. The identity of these participants is randomly determined in each decision round. Every participant has to pay **1.00 Taler** for each ball he/she buys in stage 1. If one of your balls is drawn in stage 1, you receive an intermediate prize of 72 Taler. If you are randomly selected in stage 2, you additionally receive the main prize of 96 Taler.

If you have any questions, please raise your hand now!

Experiment 2

In Experiment 2, you will face **21 decisions**. Each decision is a **choice between option 1 and option 2**. Each choice affects your own payoff, but not the payoff of any other participant of the experiment. When choosing option 1, your payoff is affected by chance, while option 2 implies a certain payment. You may be asked, for example, whether you prefer option 1, in which you receive either 400 Taler or 0 Taler with a 50% chance, or if you rather like option 2, which implies a sure payoff of c Taler. In the experiment, you will have to choose the option you prefer. This decision problem would be presented to you as follows:

Option 1	Option 2	Your Choice
with 50% probability 400 Taler with 50% probability 0 Taler	with certainty c Taler	Option 1 <input type="radio"/> <input type="radio"/> Option 2

As previously mentioned, you will encounter 21 decision problems of this kind. Your payoff from Experiment 2 is determined as follows:

At the end of all experiments, one of the 21 decision problems will be randomly chosen for each experimental participant. The option you chose in this decision problem determines your payoff. Assume, for example, that the previous example is chosen for you, and that you preferred option 1 over option 2. Then, you would receive 400 Taler or 0 Taler, each with a probability of 50%. Whether you receive 400 Taler or 0 Taler is determined by a simulated random draw of the computer.

C Control Questions

Overall, subjects of the experimental sessions were asked to answer seven control questions. Each control question provided subjects with three alternative answers, only one of which is correct. Five questions were identical across treatment, two were different. We start with the questions which were identical in both treatments:

1. How many other experimental subjects do you meet in each stage 1 interaction?
 - 1 other subject.
 - 2 other subject.
 - 3 other subject.
2. How many points does it cost you to buy 10 balls?
 - 7.50 points.
 - 10.00 points.
 - 15.00 points .
3. Assume that you bought 100 balls on stage 1. Then, what is the maximal number of balls that you can still buy on stage 2?
 - 140 balls.
 - 160 balls.
 - 200 balls.
4. What is the probability that one of your balls is drawn on stage 1 if all subjects of the experiment bought 20 balls on stage 1?
 - 25%.
 - 50%.
 - 55%.
5. What is your payoff in some decision round if you bought 30 balls on stage 1 and 50 balls on stage 2, and one of your balls is drawn on stage 1 and on stage 2?
 - 328 points.
 - 362 points.
 - 408 points.

The two remaining questions in treatment SR are:

6. In which case do you receive the 72 points prize? If
 - you buy at least 80 balls on stage 1.
 - you buy more balls on stage 1 than anybody else.
 - one of your balls is drawn from your stage 1 ballot box.
7. In which case do you additionally receive the main prize of 96 points? If
 - you reach stage 2.
 - you buy more balls in both stages than anybody else.
 - one of your balls is drawn from the stage 2 ballot box.

The two remaining questions in treatment IR are:

6. In which case do you receive the runner-up prize of 72 points? If
 - you buy at least 80 balls on stage 1.
 - you reach stage 2 and a ball from the other stage 2 subject is drawn.
 - one of your balls is drawn from the stage 2 ballot box.
7. In which case do you receive the main prize of 168 points? If
 - you reach stage 2.
 - you buy more balls in both stages than anybody else.
 - one of your balls is drawn from the stage 2 ballot box.