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Coping with complexity – Experimental evidence for narrow bracketing in multi-stage contests[☆]

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ABSTRACT

This paper investigates whether decision makers bracket their choices narrowly to facilitate complex decision problems. Evidence from a framing variation of rewards in experimental two-stage pairwise elimination contests indicates that decision makers neglect the option value of participation in future stages of the contest if the reward frame facilitates the separate consideration of stages, but not if the reward frame induces forward-looking behavior. Decision makers account for the option value of participation in future stages of the contest independently of the reward frame, however, when complex strategic interactions in future stages of the contest are replaced by simple lotteries that facilitate the determination of the option value. The results present novel evidence for the prevalence and the determinants of choice bracketing as a means to cope with complexity.

1. Introduction

It has been hypothesized repeatedly that decision makers break down complex decision problems into smaller components, which they then consider in isolation to reach a decision. While from the perspective of standard economic models of decision making, such behavior might appear biased, the intuitively appealing idea of "narrow choice bracketing" is likely to be relevant in many complex decision environments. To investigate its relevance, much of the existing literature has focused on the failure of decision makers to integrate outcomes of independent lotteries. The separate consideration of *independent* lotteries only affects behavior in specific cases – for instance, when decision makers are loss averse and naive about their loss aversion.[1] Conceptually, there is no obvious link between prospect-theoretic preferences and the inclination of decision makers to employ narrow bracketing as a means to reduce complexity, which itself constitutes a form of behavioral bias.

This paper investigates whether decision makers employ narrow bracketing as a means to cope with complexity in multi-period decision problems. In particular, we analyze whether decision makers omit future consequences of current actions to

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[1] See Benartzi and Thaler (1995), Gneezy and Potters (1997), Langer and Weber (2001), Rabin and Weizsäcker (2009), Hilgers and Wibral (2014), and the references cited therein.

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simplify investment choices in a complex dynamic decision environment with *interdependent* choices. Specifically, we analyze investment decisions in a two-stage pairwise elimination contest. Decision makers first choose their stage-1 investment and conditional on succeeding in stage 1, subsequently choose their stage-2 investment. Agents who have already won stage 1 trade-off the costs of the investment and the benefit of increasing their chances to receive the prize Δ_2 when choosing stage-2 investment, and the optimal solution to this trade-off depends on expected stage-2 investment by the opponent.

Of more interest for the purpose of the present paper is the stage-1 investment decision, however. This decision is considerably more complex than the stage-2 investment choice. In particular, decision makers in stage 1 of the contest compete for a prize Δ_1 awarded to the stage-1 winner *and* for the right to participate in stage 2 of the contest where they have a chance to win the additional reward Δ_2. Due to strategic interaction in stage 2 of the contest, the option value of participation in stage 2 is determined by the decision maker's own future stage-2 investment and by expected stage-2 investment of the future opponent. In addition to this option value, the investment decision is influenced by the interaction in stage 1. This implies that stage-1 investment decisions are influenced by several factors relating to stage 1 *and* the option value of stage 2. Omitting future consequences of current actions by ignoring the option value of participation in stage 2 when choosing stage-1 investment is thus suboptimal from the perspective of the standard decision model. However, the omission has the benefit that it substantially simplifies the stage-1 investment decision. Decision makers who bracket narrowly only take account of the expected stage-1 investment by the opponent, but omit future consequences of current actions. This is cognitively much less demanding as it reduces the mental computing power required to make the (optimal) stage-1 investment decision by also incorporating the option value into the decision. Narrow bracketing can thus be seen as a means to avoid the complexity inherent in the determination of the option value of participation in future strategic interactions. In contrast, broadly bracketing decision makers must take expected stage-1 investment by the opponent, expected stage-2 investment by the future opponent, and own future stage-2 investment into account when choosing their optimal stage-1 investment.

We analyze the prevalence and potential determinants of narrow bracketing in laboratory experiments by exploiting controlled variation in a two-by-two design that varies the salience of the second stage interaction and the complexity of the stage-1 investment decision. To manipulate the salience of the second stage, we implement different reward frames that involve identical stakes but either facilitate or complicate the separate consideration of interdependent choices. In particular, decision makers in the "separate reward" (SR) frame receive the reward Δ_1 for winning stage 1 before they compete for the second reward Δ_2 in stage 2. Rewards in the "integrated reward" (IR) frame are received only after decision makers have chosen both their stage-1 and their stage-2 investments. The stage-2 loser only then receives the reward Δ_1 for winning stage 1, while the stage-2 winner receives a large prize $\Delta_1 + \Delta_2$ that integrates the rewards for winning stage 1 and winning stage 2, respectively.

To manipulate the complexity of the stage-1 investment decision while maintaining the reward frames, we eliminate the strategic interaction in stage 2. In particular, in the control treatments SRc and IRc we replace the second stage of the contest by a lottery, such that the option value of participation in stage 2 is independent of the decision maker's own future stage-2 investment and of the expected stage-2 investment by the future opponent. Hence, the two-by-two design delivers two baseline treatments SR and IR where subjects choose both their stage-1 and stage-2 investment, and two control treatments SRc and IRc where stage 2 is replaced by a lottery such that subjects only choose their stage-1 investment.

The payoffs as well as the associated realization probabilities are identical in all reward frames as long as decision makers bracket broadly and act according to the textbook predictions, which thereby provides a natural null hypothesis of behavioral equivalence across the treatments. However, the different reward frames might affect stage-1 investment choices of decision makers who employ narrow bracketing as a means to cope with complexity. While the "integrated reward" frame forces decision makers to take account of the option value of participation in stage 2 – there is no immediate reward for winning stage 1 – decision makers in the "separate reward" frame might focus exclusively on the immediate stage-1 reward and omit the option value of participation in stage 2 when choosing their stage-1 investment. This would imply that average stage-1 investment differs across baseline treatments and is lower in SR than in IR. In addition, the comparison of stage-1 investment choices across control treatments SRc and IRc allows us to test whether the prevalence of narrow bracketing is related to the complexity of the decision environment. In particular, if the complexity associated with the stage-1 investment decision is the reason for narrow bracketing in SR, stage-1 investment choices should not be affected by the reward frame in the control treatments, or if anything to a lesser extent.

The major advantage of the setting considered in this paper in comparison to most existing studies that analyze narrow bracketing is that the impact of narrow bracketing on behavior is independent of how the utility function is shaped, which allows us to study the prevalence and potential determinants of narrow bracketing independent of myopia regarding loss aversion. Moreover, the strategic environment introduces an element of complexity that occurs frequently and naturally in many real life situations as well as in many lab studies.

The data reveal that stage-1 investments are significantly lower in SR than in IR, while the investment choices of subjects in stage 2 are almost identical across the two baseline treatments. In addition, we find that stage-1 investment choices are almost identical across the two control treatments with reduced complexity. These findings are consistent with the hypothesis that decision makers apply narrow bracketing as a means to cope with complexity. Additional analyses of the data at the individual level provide further support for the hypothesis. In particular, we find that cognitive capacity – approximated by self-reported math grades – is systematically related to the treatment effect in the baseline treatments: subjects with lower cognitive capacity 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 higher cognitive capacity do not differ across baseline treatments. At the same time, cognitive capacity does not differently affect stage-1 investment across reward frames in the control treatments where the stage-1 investment decision is less demanding. A similar finding emerges when using a proxy for the tendency to apply narrow bracketing in complex choice situations.

The remainder of this paper is structured as follows. The contribution of this paper in the context of the existing literature is discussed in Section 2. The experimental design is described in Section 3. Section 4 presents and discusses the main findings, Section 5 discusses potential concerns and alternative explanations, and Section 6 concludes.

2. Related literature

The results of this paper contribute to the literature on narrow bracketing in several ways. First, we introduce a novel framework, based on a framing variation, to study the phenomenon of narrow bracketing using a novel design that does not require additional assumptions on the curvature of the utility function. In particular, relative to the often used investment task with frequent and less frequent feedback (Gneezy and Potters, 1997; Thaler et al., 1997), the contest setting considered in this paper has the advantage that narrow bracketing affects behavior independently of how the utility function is shaped, and in particular, independent of whether decision makers are loss averse or not. Moreover, while varying the salience of the option value of participation on stage 2 and therefore the perceived possibility to bracket narrowly, the framing variation allows for a very clean identification by holding fixed all material aspects of the decision problem. In contrast, almost all existing contributions analyze the failure of decision makers to integrate outcomes of independent lotteries which affects behavior if and only if decision makers are loss averse, implying that narrow bracketing does not directly affect behavior in these settings, but only indirectly in combination with loss aversion.[2] In this sense, the results of this paper show that narrow bracketing is an independent behavioral phenomenon that depends on the perceived complexity of the decision environment.

Second, our findings provide direct evidence for the conjecture by Simon (1957) that decision makers employ narrow bracketing as a means to cope with complexity. Our notion of complexity is related to the requirements on decision makers' computing power that has been used in work on repeated games.[3] In particular, replacing the stage 2 interaction by a lottery arguably reduces the dimensionality of the stage-1 decision problem encountered by a subject in the experiment substantially. Decision makers appear to bracket narrowly in the baseline treatment where separate rewards facilitate narrow bracketing and where the determination of the option value of participation in stage 2 is complex due to the pending future strategic interaction. In contrast, they bracket broadly independent of the reward frame in the control treatments where the determination of the option value of participation in stage 2 is comparably simpler. These results are consistent with the hypothesis that bracketing is a behavioral strategy to cope with complexity when this appears feasible, as suggested by the SR frame. Our results also indicate that the prevalence of narrow bracketing is related to cognitive capacity limitations. This complements evidence by Frederick (2005) and Benjamin et al. (2013) according to which individuals with lower cognitive capacity tend to exhibit behavior that is less consistent with predictions from 'standard' choice models, and evidence by Abeler and Marklein (2017) and Hilgers and Wibral (2014) that individuals with lower cognitive ability are more likely to bracket narrowly.

Third, the property that narrow bracketing affects behavior independent of whether the utility function of decision makers is non-linear or not has the additional advantage that it allows us to test the conjecture by Rabin and Thaler (2001) according to which narrow bracketing matters for the frequently observed excessive demand for risk premia in choices that involve small stakes. In line with this hypothesis, we observe a positive correlation between the degree of risk aversion and the difference in stage-1 investment choices across the baseline treatments, suggesting that small-stakes risk aversion is indeed related to narrow bracketing, and that decision makers who bracket narrowly in one context do so in other contexts as well.

This paper also contributes to the recent experimental literature on multi-stage contests. Several contributions show that the degree of over-investment is typically higher in the first stage of 'winner-takes-all' contests with multiple stages than in later stages (see, e.g., Sheremeta, 2010), and that absolute stage-1 investments are significantly higher than stage-2 investments if prizes are chosen in such a way that theory predicts the equality of investments across stages (Altmann et al., 2012).[4] These findings indicate that option values have powerful incentive effects, as suggested in the theoretical contribution by Rosen (1986). Importantly, however, all existing studies consider an "integrated reward" frame. Our results show that decision makers may omit the option value in "separate reward" frames which are the rule rather than the exception in personnel promotion tournaments, for example. Intuitively, employees are typically rewarded with immediate

[2] See, e.g., van der Heijden et al. (2012); Langer and Weber (2001); Rabin and Weizsäcker (2009), or Hilgers and Wibral (2014). To our knowledge, the only exception in this context is work by Abeler and Marklein (2017) on mental accounting. Their contribution differs from this paper in several dimensions, however. In particular, our setup varies both the reward frame and the degree of complexity that decision makers face when choosing stage-1 investment, whereas Abeler and Marklein (2017) focus on mental accounting and violations of the assumption that money is fungible by analyzing whether labels attached to bonus payments affect subsequent spending decisions for different categories of goods. Unlike in this paper, their setting is not suited to test the implications of variation in complexity.

[3] For instance, Kalai and Stanford (1988) or Rubinstein (1998) measure complexity as the dimensionality of possible strategies and the difficulty encountered by players in checking the rationality of a particular strategy.

[4] See also chapter 4.5 of the survey by Dechenaux et al. (2015) for additional references and for an insightful discussion of the existing literature.

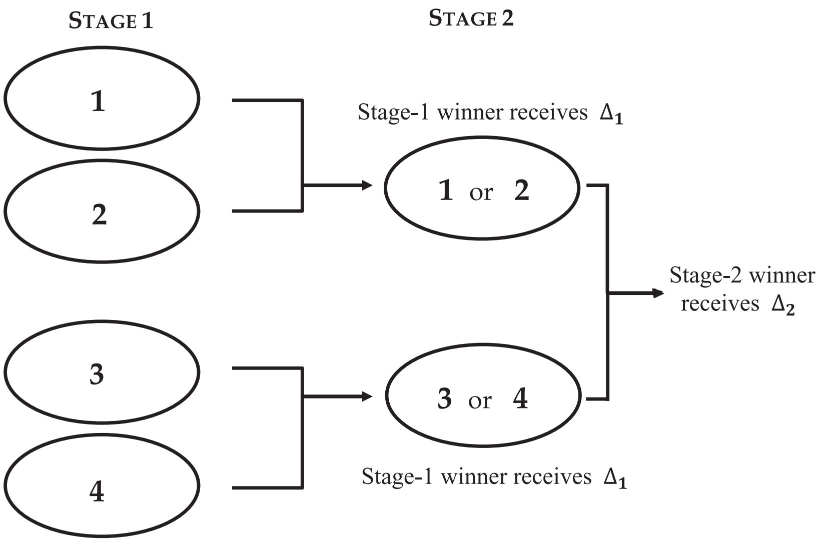

Fig. 1. Structure of the contest.

wage increases upon promotion, as in the "separate reward" frame, and not with delayed wage increases that are paid out only after they fail or succeed in the competition for future promotions, as in the "integrated reward" frame. In this sense, our findings imply that employees might not fully account for the option value of future promotions in corporate tournaments due to narrow bracketing. From the perspective of human resource management, 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.

Finally, our paper contributes to the literature on optimal prizes in contests and tournaments. While existing studies investigate how changes of winner and runner-up prizes affect effort provision in different stages of multi-stage tournaments – see, e.g., Delfgaauw et al. (2015), or Stracke et al. (2014) – the results of the present paper show that a pure framing variation may also have important implications for performance incentives. In this sense, this paper complements previous evidence by Hossain and List (2012) and Englmaier et al. (2017) that suggests that manipulations in the way incentives are communicated may affect incentives to provide effort. In particular, our findings indicate that deferred rather than immediate compensation might help to induce broad bracketing and forward-looking behavior.

3. Experimental design

3.1. The setting

Consider a two-stage pairwise elimination contest with four decision makers. As depicted in Fig. 1, subjects initially compete in pairs in stage 1 for two open stage-2 positions. Subsequently, the two stage-1 winners compete against each other in stage 2 to determine the winner of the contest. In each interaction, subjects choose the amount they want to invest into the contest. Higher investments increase the probability to win an interaction, but are costly independent of success or failure. For each unit invested, a subject incurs the same constant marginal cost of one. The probability to win is given by a lottery contest success function à la (Tullock, 1980). Given individual (non-negative) investment choices x_{si} and x_{sj} by subjects i and j $(\neq i)$ in stage $s \in \{1, 2\}$, the probability that subject i wins the stage-s competition equals

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

The reward for winning stage 1 is Δ_1, and the additional reward for also winning stage 2 is Δ_2. Subjects who have already won stage 1 thus compete for the reward Δ_2. In particular, when choosing stage-2 investment, they trade-off the costs of investment that are independent of success or failure and the benefit of increasing their chances to receive Δ_2. The optimal solution to this trade-off depends on expected stage-2 investment by the opponent. The stage-1 investment decision is more complicated. In particular, decision makers in stage 1 of the contest compete for the reward Δ_1 and for the right to participate in stage 2 of the contest where they have a chance to win the additional reward Δ_2. Consequently, they need to determine the option value of participation in stage 2 of the contest. Due to strategic interaction in stage 2 of the contest, this option value depends on intended own stage-2 investment as well as on expected stage-2 investment by the future opponent. Taken together, the investment decision in stage 1 of the contest is therefore the result of a trade-off between investment costs and the chances to win and stay in the contest, which depend on expected stage-1 investment by the opponent, as well as on the decision maker's own future stage-2 investment, and on the expected stage-2 investment by the future opponent.[5]

3.2. Experimental treatments, parameters, and hypotheses

Treatments and parameters. We implemented four different versions of the two-stage pairwise elimination contest in a between subject design. Rewards $\Delta_1 = 72$ and $\Delta_2 = 96$ were held constant across treatments. The framing of rewards differed across our baseline treatments SR and IR. Subjects in treatment SR – short for '*separated rewards*' – were told to receive the reward Δ_1 immediately after winning stage 1 and before the two stage-1 winners compete for the reward Δ_2 in stage 2. In contrast, in the '*integrated rewards*' treatment IR subjects were informed that rewards are awarded only after the realization of the stage-2 outcome. In particular, subjects were told that the stage-2 winner receives the sum $\Delta_1 + \Delta_2$ as a reward for winning both stages, while the stage-2 loser receives the reward Δ_1 for participating in stage 2 (i.e., for winning stage 1).

In addition, we conducted two control treatments SRc and IRc. The values of all rewards as well as the labels attached to these rewards in the control treatments SRc and IRc are identical to those in the baseline treatments SR and IR, respectively. The only difference is that the strategic interaction in stage 2 of the contest was replaced in the control treatments by a lottery that subjects either win or lose with equal probability, i.e., the complex strategic interaction in stage 2 was replaced by a simple lottery.

We conducted 5 sessions of each of the baseline treatments, and 4 sessions of each of the control treatments, i.e., we ran 18 sessions with 20 participants per session. The 360 subjects were students from the University of Innsbruck and recruited using ORSEE (Greiner, 2004). All sessions were computerized, using the software z-Tree (Fischbacher, 2007). The exchange rate was such that 200 experimental currency units corresponded to 1.00 Euro. Sessions lasted approximately 70 min (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).

Testable hypotheses. Fig. 2 displays the sequence of events in treatments SR and IR and illustrates that reward frames do not affect investment incentives of broadly bracketing decision makers. To illustrate this claim, consider a decision maker with utility function $U(\cdot)$ and an initial wealth endowment ω who participates in both framing variants of the contest and only cares about his final wealth. Independent of the reward frame, there are three possible outcomes: The decision maker may receive the sum $\Delta_1 + \Delta_2$ after winning both stages, or only the reward Δ_1 after winning stage 1 and losing stage 2, or no reward at all after losing stage 1. Given that final rewards and outcomes are identical, reward frames should leave investment incentives of broadly bracketing decision makers unaffected if the realization probabilities of these outcomes are independent of the reward frame, i.e., if reward frames leave (expected) stage-1 and stage-2 investment choices by the opponent unaffected.[6]

Hypothesis 0. (Equivalence): Reward frames leave stage-1 investment choices and stage-2 investment choices unaffected:

(*a*) $x_2(\mathtt{SR}) = x_2(\mathtt{IR})$

(*b*) $x_1(\mathtt{SR}) = x_1(\mathtt{IR})$.

Stage-1 investment choices might differ across treatments if decision makers bracket their choices narrowly, however. Consider first the integrated-reward frame in IR. The fact that the contest is presented as a sequence of a qualification-stage without rewards and a final-stage with rewards for both the winner and the loser is more likely to enforce broad bracketing and forward-looking behavior in stage 1. In particular, given that stage-1 winners receive no immediate reward, decision makers naturally take account of stage 2 and integrate rewards when choosing their stage-1 investment – the only reason for a non-zero investment choice in stage 1 is the prospect to receive a reward in stage 2. This is potentially different in SR, since the separate-reward frame allows decision makers to focus entirely on the immediate stage-1 reward when choosing stage-1 investment. In particular, separate rewards for winning stage 1 and stage 2 allow decision makers to break down the complex decision problem they are confronted with into two smaller components: Narrowly bracketing decision makers in SR may initially focus entirely on the immediate stage-1 reward when choosing stage-1 investment – ignoring the option value of receiving additional rewards in future interactions – and only subsequently choose stage-2 investment once they have won the stage-1 interaction. This implies that stage-1 investment should be lower in SR than in IR if decision makers bracket their choices narrowly in SR, while stage-2 investment choices should be unaffected by the reward frame.

Hypothesis 1. (Narrow bracketing): Reward frames affect stage-1 investment choices, but leave stage-2 investment unaffected:

(*a*) $x_2(\mathtt{SR}) = x_2(\mathtt{IR})$

(*b*) $x_1(\mathtt{SR}) < x_1(\mathtt{IR})$.

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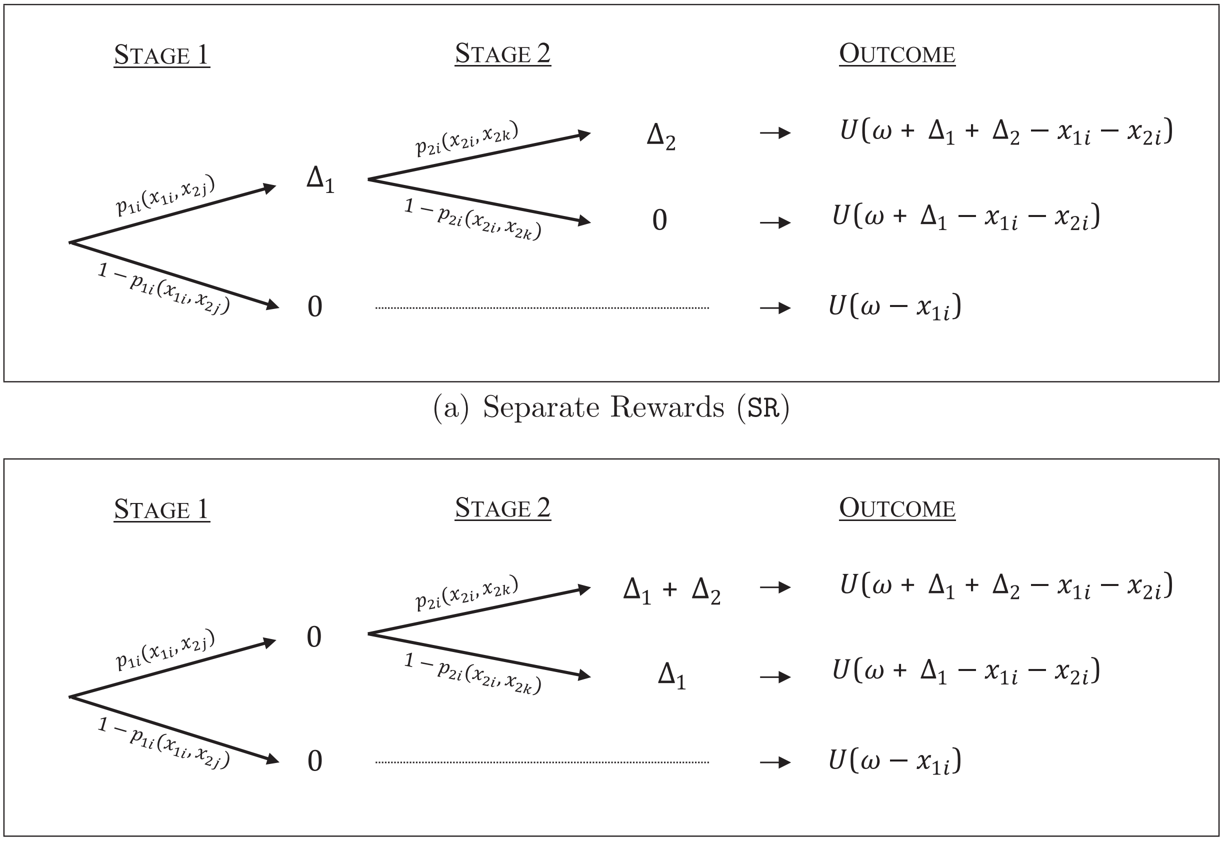
[5] This is illustrated in Appendix A by the corresponding predictions from subgame perfect Nash equilibria of the standard model of this setting.

[6] We assume that this is the case in what follows and get back to this potential issue in Section 5.1.



(a) Separate Rewards (SR)

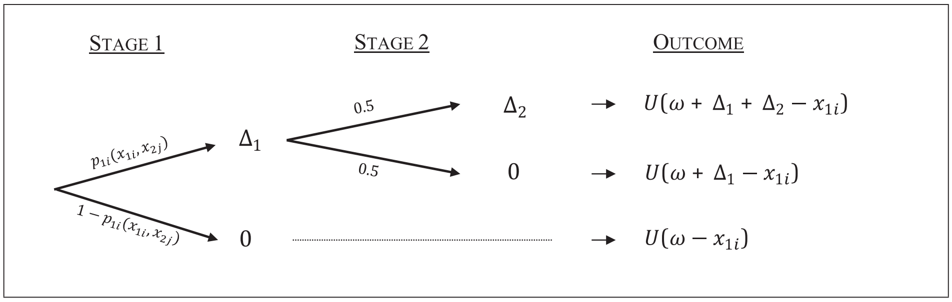
(b) Integrated Rewards (IR)

Fig. 2. Separate and integrated rewards (Baseline).

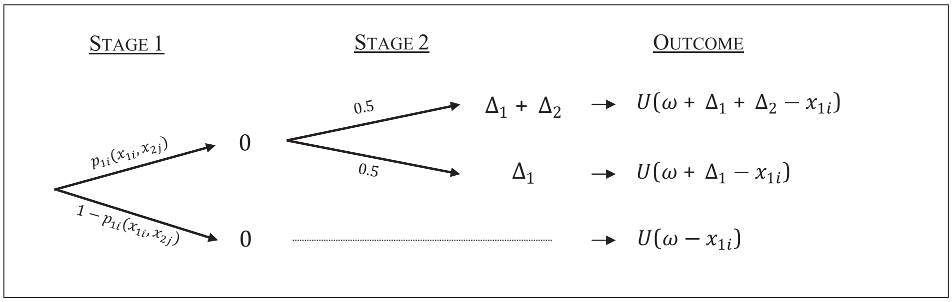
Narrow bracketing in SR facilitates the stage-1 investment decision but has the implication that decision makers who disregard the option value invest lower amounts in stage 1. In particular, narrowly bracketing decision makers avoid the problem of determining the option value of participation in stage 2, as assessing this value is fairly complicated due to the strategic interaction in stage 2 of the contest: the option value depends on the decision maker's intended own future stage-2 investment as well as on the future stage-2 investment by the opponent. Consequently, broadly bracketing decision makers face the complex problem to determine optimal stage-1 investment based on intended own stage-2 investment, on expected future investment by the (unknown) stage-2 opponent, and on expected stage-1 investment by the immediate stage-1 opponent. The stage-1 investment decision of narrowly bracketing decision makers in SR is much simpler, however; they only account for expected stage-1 investment by the immediate opponent.

We implement two control treatments SRc and IRc to test whether the complexity that decision makers are exposed to in stage 1 of the contest affects the prevalence of narrow bracketing. As depicted in Fig. 3, reward frames do not differ across baseline and control treatments in terms of rewards and the overall structure of the decision problem. In particular, the reward frames may still facilitate narrow bracketing in SRc relative to IRc. To vary the complexity of the decision problem, however, the strategic interaction in stage 2 of the contest is replaced by a lottery that subjects either win or lose with equal probability. This implies that the option value of participation in stage 2 is fixed and independent of future investment choices in the control treatments, i.e., there is no need for decision makers to think about intended own stage-2 investment, about expected stage-2 investment by the opponent, or about the resulting stage-2 winning probability when choosing stage-1 investment. Consequently, it is much easier to determine the option value of participation in stage 2 in the control treatments than in the baseline treatments, such that the benefit of narrow bracketing – the simplification of the stage-1 investment decision – is less important.[7] Stage-1 investment choices should thus not be affected by the reward frame in the control treatments – or at least to a lesser extent – if narrow bracketing is a means to reduce the complexity of a decision problem.

[7] Apart from that, one would expect that stage-1 investment choices are higher in any one of the two control treatments than in the respective baseline treatments, since costly stage-2 investments are absent in the control treatment. See Appendix A.2 for details.



(a) Separate Rewards control (SRc)



(b) Integrated Rewards control (IRc)

Fig. 3. Separate and integrated rewards (Control).

Hypothesis 2. (Equivalence – control treatment): The reward frame does not affect stage-1 investment choices in the control treatments, i.e.:

$$x_1(\texttt{SRc}) = x_1(\texttt{IRc}).$$

3.3. Experimental procedures

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/SRc or IR/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 and anonymously 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 in 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 et al. (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]

[8] A translated version of the instructions is provided in Appendix C in the Supplementary material. The original instructions (in German) are available from the authors upon request.

[9] In the Dohmen et al. (2010) procedure, each subject is exposed to a series of choices between a safe payment (which is varied systematically) 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 C in the Supplementary material.

[10] Subjects are, on average, weakly risk-averse, fairly good in mathematics, frequently majoring in economics, and able to correctly answer the control questions. Regarding differences across treatments, there are slightly more male students in the SR than in the IR treatment, but the difference across treatments is insignificant (Fisher ‘exact’ test: $p = 0.202$). 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 reward frames.

Table 1
Experimental results – baseline treatments SR and IR.

	#Obs.	SR	IR
Stage-1 investment (x_1^*)	100	**35.494**	**45.238**
		(1.874)	(2.957)
Stage-2 investment (x_2^*)	100	**43.589**	**45.976**
		(1.852)	(2.614)

Note: The numbers denote average investments of 100 subjects per treatment across 30 decision rounds (in experimental currency). Standard errors based on averages on the individual level are provided in parentheses.

Decision environment. The decision environment in the experiment was framed neutrally in that there was no reference of 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 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 of the baseline treatments and in stage 1 of the control treatments, respectively. In particular, a subject that reached stage 2 in the baseline treatments could use whatever remained of the endowment (after the costs for the number of balls acquired in 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 implied 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 across decision rounds, experimental subjects were told that the endowment could only be used in a given round.

4. Experimental results

4.1. Main results

Baseline treatments. Average investment levels by stage in the baseline treatments SR and IR across all 30 decision rounds are summarized in Table 1. Consider stage-2 investment first. Recall that stage-2 investment choices are predicted to be unaffected by the reward frame independently of choice bracketing and thus identical across treatments SR and IR if reward frames leave expected stage-2 investment choices unaffected. The averages of stage-2 investments across 100 subjects per treatment in Table 1 – 43.589 in SR vs. 45.976 in IR – indicate that observed behavior in the experiments is qualitatively consistent with this theoretical prediction.[11] When collapsing the data on the session level to account for interdependencies within the same session due to feedback and random rematching after each round, the non-parametric Mann–Whitney U-test (MWU-test) indicates that average stage-2 investments across all rounds are not statistically different across our main treatments (p-value $= 0.347$, $n_1 = n_2 = 5$). The finding that stage-2 investment choices do not differ across treatments continues to hold if we restrict attention to the first decision round where each subject rather than an entire session constitutes an independent observation (MWU-test: p-value $= 0.648$, $n_1 = n_2 = 100$).[12] In addition, evidence from random-effect panel regressions in columns (1)–(3) of Table 2 provides strong support to the view that stage-2 investment choices do not differ across treatments: the treatment effect is small and insignificant, no matter whether we consider session averages across all rounds in column (1), or session medians across all rounds in column (2), or session averages across rounds 16–30.

Considering stage-1 investment choices, recall from Hypotheses 0 and 1 that the reward frame should not affect stage-1 investment choices if decision makers bracket their choices broadly, while stage-1 investment is predicted to be lower in treatment SR than in treatment IR if decision makers bracket their choices narrowly. The averages of stage-1 choices across 100 subjects per treatment in Table 1 – 35.494 in treatment SR vs. 45.238 in treatment IR – are consistent with the view that decision makers bracket their choices narrowly rather than broadly in treatment SR.[13] Statistical inference shows that the observed treatment effect is not only economically sizeable, but also statistically significant. When collapsing the stage-1 data by treatment on the session level across all decision round, the MWU-test indicates that average stage-1 investments

[11] In line with numerous experimental studies of contests, the investment choices are substantially higher than the prediction of 24 units in both treatments in a subgame-perfect Nash equilibrium derived under the standard assumptions of homogeneous and risk neutral decision makers, see Appendix A for details. We return to this issue in Section 5.2.

[12] Panel (b) of Fig. B.1 in Appendix B of the Supplementary material plots average stage-2 investment choices by decision round and treatment shows that subjects invest lower amounts towards the end of the experiment than in initial rounds, both in IR and SR. The figure also reveals, however, that stage-2 investment choices are remarkably similar across treatments in all decision rounds.

[13] Again, in both treatments the investment is higher than the theoretical prediction of 24 units.

Table 2
Baseline treatments SR and IR – regression analysis.

	Stage-2 investment			Stage-1 investment		
	Mean (1)	Median (2)	Mean (3)	Mean (4)	Median (5)	Mean (6)
SR	−3.504	−1.973	−0.843	−9.744***	−3.277***	−5.235**
	(4.338)	(4.507)	(4.085)	(3.414)	(1.010)	(2.547)
Rounds	All	All	16–30	All	All	16–30
Round FE	Yes	Yes	Yes	Yes	Yes	Yes
Groups	10	10	10	10	10	10
Obs.	300	300	150	300	300	150

Note: Random-effects panel estimates with session-periods as independent observations, robust standard errors (in parentheses). SR is a dummy variable that equals 1 in treatment SR and 0 in treatment IR. Session averages for stage-2 investment are the outcome variable in columns (1) and (3), while median values are considered in column (2). $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3
Experimental results – control treatments SRc and IRc.

	#Obs.	SRc	IRc
Stage-1 investment (x_1^*)	80	**57.943**	**56.317**
		(2.999)	(2.704)

Note: The numbers denote average investments of 80 subjects per treatment across 30 decision rounds (in experimental currency). Standard errors based on averages on the individual level are provided in parentheses.

are significantly higher in IR than in SR (p-value of 0.047, $n_1 = n_2 = 5$). Restricting attention to the first decision round where each subject rather than an entire session constitutes an independent observation delivers a p-value of 0.033 for the MWU-test ($n_1 = n_2 = 100$). Results are similar when using random-effect panel regressions rather than non-parametric tests for inference: the treatment effect is sizeable and statistically significant, no matter whether we consider session averages across all rounds as shown in column (4), or session medians across all rounds as shown in column (5), or session averages across rounds 16–30 as shown in column (6) of Table 2. The effect appears to be somewhat weaker in the second half of the experiment, consistent with the view that narrow bracketing as a means to reduce complexity becomes less important as subjects gain experience.[14]

Result 1. Hypothesis 0 can be rejected in favor of Hypothesis 1. Decision makers bracket their choices narrowly in treatment SR: stage-2 investment does not differ across treatments, but stage-1 investment is significantly higher in treatment IR than in treatment SR.

Control treatments. Table 3 displays average investment levels by stage in the control treatments SRc and IRc across all 30 decision rounds. In line with Hypothesis 2, subjects invest similar amounts in stage 1 of the control treatments – 57.943 in treatment SRc vs. 56.317 in treatment IRc.[15] This suggests that decision makers account for the option value of participation in stage 2 in the control treatments, where this value is much easier to determine than in the baseline treatments. When collapsing the data by treatment on the session level across all decision rounds, the MWU-test indicates that average stage-1 investments are not significantly different across treatments (p-value $= 0.773$, $n_1 = n_2 = 4$). This continues to hold if we restrict attention to the first round only where the number of independent observations is larger (MWU-test: p-value $= 0.383$, $n_1 = n_2 = 80$), and if we use random-effect panel regressions for inference: the estimated treatment effect is close to zero and statistically insignificant, no matter whether we consider session averages across all rounds in column (1), or session medians across all rounds in column (2), or session averages across rounds 16–30 – see columns (1)–(3) of Table 4 for details.[16]

[14] For evidence on learning, see also Panel (a) of Fig. B.1 in Appendix B of the Supplementary material, which plots average stage-1 investment choices by decision round and treatment. The plot reveals that average stage-1 investment is higher in IR than in SR in all decision rounds, but that the difference across treatments decreases somewhat as subjects gain experience.

[15] A comparison of average stage-1 investment choices across baseline and control treatments for a given reward frame also indicates that participants invest higher amounts in the control treatments. Given that stage-2 investment costs are absent in the control treatments, this is qualitatively in line with theoretical equilibrium predictions, which predict investments of 30 in the control treatments, compared to 24 in the baseline treatments – see Appendix A.2 for details. It is noteworthy, however, that the over-investment relative to the theoretical prediction is similar across baseline ad control treatments, see Section 5.2 below.

[16] See also panel (c) of Fig. B.1 in Appendix B of the Supplementary material, which plots average stage-1 investment choices by decision round and treatment. The plot reveals that stage-1 investment choices are remarkably similar across treatments in all decision rounds.

Table 4
Control treatments SRc and IRc – regression analysis.

	Stage-1 investment		
	Mean (1)	Median (2)	Mean (3)
SRc	1.626	−2.392	0.441
	(5.961)	(6.286)	(4.399)
Rounds	All	All	16–30
Round FE	Yes	Yes	Yes
Groups	8	8	8
Obs.	240	240	120

Note: Random-effects panel estimates with session-periods as independent observations, robust standard errors (in parentheses). SRc is a dummy variable that equals 1 in treatment SRc and 0 in treatment IRc. Session averages for stage-1 investment are the outcome variable in columns (1) and (3), while median values are considered in column (2).

Result 2. Hypothesis 2 that reward frames leave stage-1 investment unaffected in the control treatments cannot be rejected: average stage-1 investment does not differ across treatments SRc and IRc.

4.2. Individual characteristics and narrow bracketing

Complexity, cognitive ability, and narrow bracketing. The evidence from baseline and control treatments is consistent with the hypothesis that decision makers use narrow bracketing as a means to reduce the complexity of the decision environments they face in the experiments. To further investigate whether the desire to simplify the stage-1 investment decision is responsible for narrow bracketing and the resulting treatment effect observed across the two baseline treatments, we conducted some additional analysis to test whether this effect is related to the cognitive ability of decision makers. If narrow bracketing is indeed a means to cope with the complexity of the decision environments in stage 1 of the experiments, decision makers with comparatively low cognitive abilities should be more inclined to bracket their choices narrowly than decision makers with comparatively high cognitive abilities. Following Abeler and Marklein (2017) as well as Hilgers and Wibral (2014), we use the self-reported high-school math grade in the final high school exam as a proxy for cognitive ability (see also Spinath et al., 2006) and generate a dummy variable "math good" that splits the sample in two subgroups of similar size.[17] In particular, we distinguish between subjects who report satisfactory or worse grades in math ("math good"$=0$), and subjects who received a good or very good grade in math ("math good"$=1$).

The regression output provided in columns (1) and (2) of Table 5 indicates that differences between the stage-1 investment choices across treatments IR and SR are systematically related to cognitive ability. In particular, the estimated coefficient for the dummy variable SR in column (1) shows that the average treatment effect is sizeable and statistically significant for subjects who are satisfactory or worse in math ($p<0.010$), while the treatment effect ($=$SR + SR x math good) is much smaller and insignificant for subjects who are good or very good in math (F-test, $p=0.335$).[18] The same pattern continues to hold if we restrict attention to the second half of the experiment in column (2): the treatment effect is close to zero and insignificant for subjects with good or very good math grades (F-test, $p=0.923$), but continues to be sizeable and statistically significant for subjects with satisfactory or worse grades in math ($p<0.050$).[19] At the same time, columns (1) and (2) of Table B.1 in the Supplementary material show that cognitive ability does not affect behavior differently across reward frames in the control treatment. The estimated coefficients and interaction terms are all close to zero and statistically insignificant.

One might worry that subjects with low self-reported math grade merely fail to understand the instructions provided for the decision environment they subsequently encounter in the experiment. Therefore, we investigate whether the treatment effect is related to the number of control questions that were answered incorrectly.[20] A greater number of mistakes

[17] The threshold for splitting subjects corresponds to a math grade 2.3 (where 1 is the best and 5 is the worst). This threshold almost coincides with the mean math grade of our subjects.

[18] Robust standard errors reported in Table 5 are based on individual-level random effects. Clustering standard errors on the session level leaves the subsequently reported p-values almost unaffected. Details available upon request.

[19] To some extent, this reflects heterogeneity in the effects of framing differences found by van der Heijden et al. (2012) for individuals with different levels of patience.

[20] Notice 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.

Table 5
Individual characteristics and narrow bracketing.

	Stage-1 investment					
	(1)	(2)	(3)	(4)	(5)	(6)
SR	−14.330***	−11.149**	−22.149***	−16.732**	−26.967***	−22.803**
	(5.456)	(5.610)	(7.344)	(7.262)	(9.134)	(9.304)
Math good	−8.484	−9.373	−3.491	−3.220	−8.071	−8.993
	(6.593)	(6.138)	(3.666)	(3.307)	(6.387)	(5.925)
Risk neutral	−0.610	−1.256	−9.232	−9.755	-9.114	−9.606
	(4.232)	(3.882)	(7.361)	(6.929)	(7.252)	(6.785)
SR x math good	9.555	11.845*	–	–	8.975	11.310*
	(7.624)	(6.930)			(7.346)	(6.639)
SR x risk neutral	–	–	19.544**	18.128**	19.241**	17.745**
	–	–	(8.282)	(7.799)	(8.122)	(7.581)
Round FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Rounds	All	16–30	All	16–30	All	16–30
Individuals	171	171	171	171	171	171
Obs.	5130	2565	5130	2565	5130	2565

Note: Random-effects panel estimates; robust standard errors in parentheses. SR is a dummy variable that equals 1 in treatment SR and 0 in treatment IR; 'math good' is a dummy variable that equals 1 for subjects who are good or very good in math, and 0 for subjects who are satisfactory or worse in math. Control variables include gender, the elicited certainty equivalent, and econ major. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

could be an indication for confusion or misunderstanding of the strategic situation.[21] Table B.2 in the Supplementary material 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 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, these findings lend support to the hypothesis that decision makers with cognitive capacity limitations employ narrow bracketing as a means to cope with the complexity of the stage-1 investment decision due to strategic interaction in stage 2 of the contest.

Small-stakes risk aversion and narrow bracketing. Risk attitudes that account for the curvature of the utility function might affect the level of stage-1 investment in all treatments.[22] However, risk attitudes should not differently affect stage-1 investment choices of broadly bracketing decision makers *across* reward frames in our baseline treatments: potential outcomes are identical across treatments – see Fig. 2 for details – and the same holds for realization probabilities if reward frames leave (expected) stage-1 and stage-2 investment choices by the opponent unaffected. At the same time, narrow bracketing as a means to facilitate the stage-1 investment decision does not depend on the curvature of the utility function. Omitting the option value of participation in stage 2 will reduce stage-1 investment choices, independent of whether decision makers are risk neutral, risk averse, or even risk loving.[23]

Elicited risk attitudes might be related to narrow bracketing and the observed stage-1 investment difference across the baseline treatments for a different reason, however. In an attempt to explain the so-called "calibration paradox", Rabin and Thaler (2001) hypothesize that narrow bracketing is responsible for risk averse behavior in lotteries involving small stakes.[24] In particular, they argue that risk aversion in small stakes lotteries is the consequence of narrow bracketing by individuals who make choices as if their wealth was zero and fail to integrate their overall wealth.

We subsequently investigate whether risk averse behavior in small-stakes lotteries is related to narrow bracketing in our baseline treatments. Specifically, we analyze whether the difference in stage-1 investment choices across the baseline treatments is related to elicited risk attitudes. If small-stakes risk aversion is indeed related to narrow bracketing, and if decision makers who bracket narrowly in one context do so in other contexts as well, we expect a positive correlation between the degree of risk aversion and the difference in stage-1 investment choices across the baseline treatments.

[21] The translated version of all control questions is provided in Appendix C in the Supplementary material. The original German version is available from the authors upon request.

[22] The level effect is ambiguous in contests (Konrad and Schlesinger, 1997). Whether investments increase or decrease in the degree of risk aversion depends on third-order risk attitudes (prudence) – see Treich (2010) for details.

[23] If anything, the stochastic option value is less valuable for risk averse than for risk neutral decision makers, such that the difference in stage-1 investment choices across baseline treatments due to narrow bracketing should be decreasing in the degree of risk aversion. Note, however, that we observe the opposite pattern in the experimental data, namely that the difference of stage-1 investment choices across baseline treatments is more (rather than less) pronounced for risk averse decision makers.

[24] The "calibration paradox" due to Rabin (2000) is based on the insight that risk averse behavior in lotteries involving small stakes compared to an individual's total lifetime wealth implies implausibly high degrees of risk aversion when extrapolated to risky choices involving larger stakes.

To test this hypothesis, we distinguish between subjects who are classified as being risk neutral or risk loving ("risk neutral"= 1), and subjects who are classified as being risk averse ("risk neutral"= 0).[25] Columns (3) and (4) of Table 5 show that stage-1 investment choices are significantly different across the baseline treatments for subjects who are classified as being risk averse, both across all decision rounds and in the second part of the experiment. In particular, investments by risk averse subjects are lower, which is consistent with lower incentives due to the omission of the option value. At the same time, stage-1 investment choices of subjects classified as being risk neutral (or risk loving) appear to be independent of the reward frame – the treatment effect (= SR + SR x risk neutral) is close to zero and insignificant (F-test, $p = 0.436$ and $p = 0.500$, respectively). Finally, we find no evidence that small stakes risk aversion affects behavior differently across reward frames in the control treatments SRc and IRc. The treatment effect is similar across subjects classified as being risk neutral and subjects classified as being risk averse, and not significantly different from zero in either case – see columns (3) and (4) of Table B.1 in the Supplementary material for details. Consequently, our data lend support to the joint hypothesis that small-stakes risk aversion is indeed related to narrow bracketing, and that decision makers who bracket narrowly in one context are likely to do so in other contexts, but only if the decision problem they are exposed to is sufficiently complex.

Robustness. A potential explanation for the previously reported finding that cognitive capacity and small-stakes risk aversion are related to narrow bracketing in our experiment could be that both aspects are correlated and measure the same personal trait.[26] The regression output in columns (5) and (6) of Table 5 shows that the estimates of coefficient and standard error of the interaction of the treatment dummy and the proxy for cognitive capacity is virtually unchanged when adding the interaction of the treatment dummy and the proxy for small-stakes risk aversion, and vice versa. If anything, the main effect is slightly higher in these specifications, but the total effect appears largely unaffected. This suggests that the last math grade in high school and small-stakes risk aversion measure distinct dimensions of cognitive ability if cognitive capacity limitations are the reason why participants fail to integrate their wealth in the risk elicitation task.

5. Potential concerns and alternative explanations

5.1. Reward frames and expected opponent investment

A potential concern with the results could be that the observed stage-1 investment difference across baseline treatments SR and IR is not caused by decision makers who employ narrow bracketing as a means to reduce the complexity of the decision problem they face, but instead by distorted beliefs regarding stage-1 and stage-2 investment by the opponent. In particular, the large variance in stage-1 and stage-2 investment choices might be an indication that subjects in the experiment are heterogeneous, such that different subjects might also hold different beliefs regarding their opponent's investment. Importantly, however, randomization across treatments SR and IR should account for heterogeneity in beliefs and the impact of beliefs on investment choices if beliefs are unrelated to the treatment variation. However, one might argue that reward frames affect beliefs regarding opponent investment in stage 1 and/or stage 2, and that differences in beliefs subsequently induce differences in investment choices.

Consider expected stage-2 investment choices by the opponent. To explain the observed stage-1 investment difference across baseline treatments through beliefs regarding stage-2 opponent investment, expected stage-2 investment choices would have to be systematically higher in SR than in IR. Intuitively, higher expected stage-2 investments by the opponent decrease the option value of participation in stage 2, such that stage-1 investments are ceteris paribus decreasing in expected stage-2 investment by the opponent. Our data reveal no systematic difference between stage-2 investment choices, however, and if anything, stage-2 investment choices tend to be higher in IR rather than in SR in the initial rounds. It is not clear why reward frames should affect beliefs regarding stage-2 investment by the opponent in the first place, however.[27]

This is different in stage 1 of the contest: decision makers who expect narrow bracketing by their opponents might rationally adjust own stage-1 investment accordingly. In particular, sophisticated decision makers in SR who anticipate that their stage-1 opponent might bracket narrowly might respond by investing less than they would in IR where narrow bracketing appears to be less prevalent.[28] However, this mechanism is unlikely to be responsible for the strong treatment effect we observe across baseline treatments in our data for two reasons. First, anticipating narrow bracketing by opponents requires a high degree of sophistication, but we find no evidence that participants with high cognitive capacity behave differently across treatments. Instead, participants with cognitive capacity constraints appear to be responsible for the difference in stage-1 investment choices across treatments SR and IR. Second, and in line with the previous argument, the effect on

[25] Dropping the (low number of) subjects that are classified as being risk loving and only comparing those subjects who exhibit risk neutral and risk averse behavior does not affect our findings. 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.

[26] This view would be consistent with Dohmen et al. (2010) who document a correlation between cognitive skills and risk attitudes as revealed in choices between safe payments and lotteries involving small stakes.

[27] If anything, the three digit prize awarded to the stage-2 winner in treatment IR might appear higher than the two digit prize awarded to the stage-2 winner in treatment SR. To avoid the belief that an opponent might be budget constrained in stage 2 due to high stage-1 investment, we chose an endowment that is higher than the highest prize awarded in the experiment.

[28] In line with this argument, Blume and Gneezy (2010) find that sophisticated decision makers anticipate that sophistication of other participants affects their choices in strategic interactions.

Table 6
Over-investment by treatment and stage.

	SR	IR	SRc	IRc
Stage 1	1.479[a]	1.885	1.931	1.877
Stage 2	1.816	1.916	–	–

Note: The numbers depict the ratio of observed average investment over the subgame perfect Nash equilibrium prediction, assuming homogeneity and risk neutrality. [a] Compared to a benchmark with option value for stage-2 participation of 0, the over-investment ratio is 1.972.

stage-1 investment due to anticipated narrow bracketing by the opponent should be very small. When considering a standard theoretical benchmark, the best-response function is almost flat at the point of intersection in equilibrium, such that a 50% reduction in expected opponent investment delivers less than a 10% reduction in optimal stage-1 investment.[29] Thus, even though we cannot rule out that beliefs of some participants regarding stage-1 investment by the opponent differ across treatments due to anticipated narrow bracketing by the opponent, the resulting effect of differences in beliefs are very unlikely to explain the stage-1 treatment effect we observe across the baseline treatments.[30]

5.2. Equilibrium predictions and over-investment

Observed investment choices are in line with the predictions that reward frames leave stage-2 investment in the baseline treatments and stage-1 investment in the control treatments unaffected, while stage-1 investment choices differ across baseline treatments. At the same time, a comparison of observed investment choices and subgame-perfect Nash equilibrium predictions derived under the standard assumptions that decision makers are homogeneous, broad bracketing, and risk neutral – and that this is common knowledge – indicates substantial degrees of over-investment.[31] In particular, Table 6 shows that the ratio of average investment choices in the experiment and the respective theoretical prediction by treatment and stage is substantially larger than one throughout.[32] Over-investment is rather the rule than the exception in experimental Tullock contests, however.[33] In particular, it is well known that over-investment is increasing in the endowment that participants receive in experiments. Sheremeta (2011) shows, for example, that over-investment can be reduced if the non-binding endowment participants receive decreases.[34] Moreover, Price and Sheremeta (2011, 2015) show that over-investment is related to whether the endowment is given as as lump sum in advance or period by period, and might be lower if participants earn their endowment in a real effort task prior to participating in a contest.[35] Apart from that, higher endowments increase the strategy space and thus the chance to make mistakes, which also contributes to over-investment according to Potters et al. (1998).

We explicitly chose an endowment that is as high as the total amount of prize money awarded in the experiment because this rules out that participants feel budget constrained at any point, even though this design choice might come at the price of over-investment in the experiment. Intuitively, participants who perceive the endowment as binding cannot bracket their choices narrowly, since stage-2 and stage-1 investment choices are then linked through the binding endowment, both in the integrated and in the separate reward frame. Apart from that, low endowments might also induce the belief that some opponents face a binding budget constraint in stage 2 of the contest, which might then affect stage-1 investment choices. Importantly, however, the endowment is held fixed across treatments, such that over-investment due to the comparatively high endowment is unlikely to differently affect behavior across treatments.[36] In particular, both the "play money" effect and the potential to make mistakes should increase over-investment to the same degree across treatments. In line with this argument, Table 6 shows that the degree of over-investment is similar across treatments and stages. The only exception is stage 1 of treatment SR. Over-investment in stage 1 of treatment SR is no longer different from the degree of over-investment observed in other treatments and stages, however, once one accounts for narrow bracketing by assuming that decision makers in stage-1 of treatment SR neglect the option value of participation in stage 2. In particular, when the option value of participation in stage 2 is entirely omitted, theory predicts that stage-1 investment should amount to 18

[29] See Appendix A.1 for details.

[30] In addition, the fact that treatment differences do not disappear in later rounds suggests that belief adaptation and learning do not eliminate the treatment effect either, providing little to no evidence for forward induction arguments along the lines of Blume and Gneezy (2010).

[31] The solution to this model is provided in Appendix A.1.

[32] All numbers are significantly larger than one at the 5% level according to a non-parametric Wilcoxon signed-rank test.

[33] See Sheremeta (2013) for an excellent survey of the existing literature, and Sheremeta (2015) for an insightful discussion of existing explanations for this phenomenon.

[34] Shupp et al. (2013) find that participants invest even less than the Nash equilibrium prediction if the endowment is non-binding, but relatively close to the equilibrium prediction.

[35] These findings suggest that participants tend to perceive the endowment as play money (Thaler and Johnson, 1990).

[36] We find no evidence for differences across treatments in the number of cases where participants invest their entire endowment: this happens in close to 0.3% of all decision rounds.

rather than 24 units. Compared to this benchmark, the over-dissipation ratio becomes 1.972 rather than 1.479, which is essentially in the same range as in the other treatment conditions in Table 6.

Another frequently mentioned explanation for over-investment in Tullock contests is that subjects experience a 'joy of winning', which amplifies the valuation of rewards awarded in contests and thereby leads to over-investment (Sheremeta, 2010). Even though non-monetary values of winning can arguably explain the extent of over-investment, joy of winning alone cannot explain the treatment differences, 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 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 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 incentives and thus investments when the immediate reward in 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 investments in both treatments in stage 2 (where rewards are unequal across treatments). More importantly, the finding in the control treatments – where the reward structure is identical to the baseline treatments – 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 treatment effects in investment behavior in the baseline and the control treatments based on such a mechanism.

6. Concluding remarks

The results of this paper provide novel evidence that supports the view that narrow bracketing is a means of decision makers to cope with complex decision problems. In particular, evidence from framing variation of rewards in experimental two-stage pairwise elimination contests indicates that decision makers neglect the option value of participation in future stages of the contest if the reward frame facilitates the separate consideration of stages, but not if the reward frame induces forward-looking behavior. At the same time, the data indicate that decision makers account for the option value of participation in the second stage if the determination of the option value is less complex, in particular, when complex strategic interactions in future stages of the contest are replaced by simple lotteries that facilitate the determination of the option value. These findings are consistent with the view that decision makers choose narrow bracketing as a strategy to cope with complexity, weighting potential benefits against potential costs.

The results provide some indication for the claim that decision makers with cognitive capacity limitations face a trade-off between mistakes due to cognitive overload when bracketing their choices broadly, and distorted stage-1 investment choices in case of narrow bracketing. In particular, the results show that decision makers with below average math grades invest *less* in stage-1 of treatment SR than decision makers with above average math grades, in line with the notion that decision makers with cognitive capacity limitations omit the option value of participation in stage 2. At the same time, we observe that decision makers with below average math grades invest *more* in stage-1 of treatment IR than decision makers with above average math grades, consistent with the view that decision makers with cognitive capacity limitations make mistakes due to cognitive overload and invest too much in stage 1. This pattern is consistent with the view that narrow bracketing might potentially help decision makers with cognitive capacity limitations to make better choices in complex environments, but the fact that all participants over-invest makes it hard to evaluate the impact of deviations on welfare. A thorough analysis of this question appears to be a promising avenue for future research.

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Appendix A. Theory: subgame perfect Nash equilibrium

The equilibrium concept in the two-stage pairwise elimination contest is subgame perfect Nash, i.e., we assume common knowledge of player types.[37] To facilitate the subsequent solution, we assume that participants are symmetric. This simplifying assumption is inconsequential for the qualitative predictions on which the experimental design is based if expected opponent types and the resulting beliefs are independent of the treatment variation.

[37] Analytical solutions of the perfect Bayesian Nash equilibrium in Tullock contests with imperfect information are only available for special cases – see Malueg and Yates (2004) or Ewerhart (2010), for example.

In what follows, we derive the equilibrium solution for baseline and control treatments, assuming that agents are either risk neutral or risk averse, respectively. In each case, we solve the game by means of backwards induction, i.e., we start by solving the stage-2 interaction before we consider the parallel stage-1 pairings.

A1. Baseline treatments

Risk neutrality. We solve the two-stage contest by backwards induction and start by solving the stage-2 interaction. The identity of agents who compete in stage 2 does not affect behavior due to homogeneity (and common knowledge of homogeneity). Therefore, it is without loss of generality to consider the interaction between agents i and j. The formal optimization problem for agent i reads

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

The contest satisfies the conditions for a unique symmetric equilibrium (see, e.g., Cornes and Hartley, 2012). Combining first-order optimality conditions delivers stage-2 equilibrium investment as

$$x_2^* \equiv x_{2i} = x_{2j} = \frac{\Delta_2}{4}. \tag{A.1}$$

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 in stage 1. Consequently, Π_2^* is the relevant option value for the stage-1 investment decision.

Consider the stage-1 investment decision next. As in stage 2, 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

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

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 in 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.

Risk aversion. The objective function of an agent i with utility function $u(\cdot)$ who competes with agent j in stage 1 and – upon winning stage 1 – meets agent k in stage 2 reads

$$\begin{aligned}\max_{x_{1i}\geq 0, x_{2i}\geq 0} EU_i(x_{1i}, x_{1j}, x_{2i}, x_{2k}) = &\frac{x_{1i}}{x_{1i}+x_{1j}}\cdot\frac{x_{2i}}{x_{2i}+x_{2k}}\cdot u(\omega+\Delta_2+\Delta_1-x_{2i}-x_{1i})\\ &+\frac{x_{1i}}{x_{1i}+x_{1j}}\cdot\left(1-\frac{x_{2i}}{x_{2i}+x_{2k}}\right)\cdot u(\omega+\Delta_1-x_{2i}-x_{1i})\\ &+\left(1-\frac{x_{1i}}{x_{1i}+x_{1j}}\right)\cdot u(\omega-x_{1i})\end{aligned}$$

The equilibrium concept is again subgame perfect Nash. Therefore, we start again by considering the derivative with respect to x_{i2}:

$$\begin{aligned}\frac{\partial EU_i(\cdot)}{\partial x_{2i}} = &\frac{x_{2k}}{(x_{2i}+x_{2k})^2}\cdot[u(\omega+\Delta_2+\Delta_1-x_{2i}-x_{1i}) - u(\omega+\Delta_1-x_{2i}-x_{1i})]\\ &-\frac{x_{2i}}{x_{2i}+x_{2k}}\cdot u'(\omega+\Delta_2+\Delta_1-x_{2i}-x_{1i})\\ &-\left(1-\frac{x_{2i}}{x_{2i}+x_{2k}}\right)\cdot u'(\omega+\Delta_1-x_{2i}-x_{1i}) \overset{!}{=} 0\end{aligned}$$

Again, the contest satisfies the conditions for a unique symmetric equilibrium (see, e.g., Cornes and Hartley, 2012), hence using symmetry $x_2^* = x_{2i}^* = x_{2k}^*$ and rearranging terms delivers

$$x_2^* = \frac{u(\omega+\Delta_2+\Delta_1-x_2^*-x_{1i}) - u(\omega+\Delta_1-x_2^*-x_{1i})}{2\cdot[u'(\omega+\Delta_2+\Delta_1-x_2^*-x_{1i}) + u'(\omega+\Delta_1-x_2^*-x_{1i})]}$$

As shown by Treich (2010), risk averse agents might invest more or less than risk neutral agents in stage 2, depending on third-order risk attitudes. In particular, only risk averse agents who are also prudent invest strictly less than risk neutral decision makers. Even though prudence appears to be the rule rather than the exception in reality, numerical calibrations (available from the authors upon request) show that the effect of risk aversion on investment choices is modest.

Consider next optimal behavior in stage 1 of the contest characterized by the derivative of the objective function with respect to x_{i1}:

$$\begin{aligned}\frac{\partial EU_i(\cdot)}{\partial x_{1i}} &= \frac{x_{1j}}{(x_{1i}+x_{1j})^2}\cdot\left[\frac{u(\omega+\Delta_2+\Delta_1-x_2^*-x_{1i})+u(\omega+\Delta_1-x_2^*-x_{1i})}{2}-u(\omega-x_{1i})\right]\\ &\quad -\frac{x_{1i}}{x_{1i}+x_{1j}}\cdot\left[\frac{u'(\omega+\Delta_2+\Delta_1-x_2^*-x_{1i})+u'(\omega+\Delta_1-x_2^*-x_{1i})}{2}\right]\\ &\quad -\left(1-\frac{x_{1i}}{x_{1i}+x_{1j}}\right)\cdot u'(\omega-x_{1i}) \overset{!}{=} 0\end{aligned}$$

Using symmetry $x_1^* = x_{1i}^* = x_{1j}^*$ and rearranging terms delivers

$$x_1^* = \frac{u(\omega+\Delta_2+\Delta_1-x_2^*-x_1^*)+u(\omega+\Delta_1-x_2^*-x_1^*)-2\cdot u(\omega-x_1^*)}{2\cdot[u'(\omega+\Delta_2+\Delta_1-x_2^*-x_1^*)+u'(\omega+\Delta_1-x_2^*-x_1^*)+u'(\omega-x_1^*)]}$$

Agents in stage 1 of the contest are not only exposed to the risk of winning or losing as in stage 2, but in addition they compete for a stochastic option value. As shown by Guigou et al. (2016), risk averse agents respond to uncertainty in the value of prizes in contests by investing less. Taken together, this implies that risk averse agents are event more likely to invest lower amounts in stage 1 of the contest than in stage 2. In particular, even imprudent agents might invest less (and not more) in stage 1 of the contest.

A2. Control treatments

Risk neutrality. Stage-2 investments are essentially fixed at zero in the control treatments. Reaching stage 2 thus has an expected value of $\Pi_2^* = 0.5\cdot\Delta_2$ for agents who compete in stage 1. Consequently, $\Pi_2^* = 0.5\cdot\Delta_2$ is the relevant option value for the stage-1 investment decision.

Consider the stage-1 investment choice next. 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 (and homogeneity is common knowledge). 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

$$x_1^* \equiv x_{1k}^* = x_{1l}^* = \frac{2\cdot\Delta_1+\Delta_2}{8}. \tag{A.3}$$

A comparison of (A.2) and (A.3) immediately reveals that equilibrium stage-1 investments are higher in the control treatments than in the benchmark treatment.

Risk aversion. The objective function of an agent i with utility function $u(\cdot)$ who competes with agent j in stage 1 and – upon winning stage 1 – meets agent k in stage 2 reads

$$\begin{aligned}\max_{x_{1i}\geq 0} EU_i(x_{1i},x_{1j}) &= \frac{x_{1i}}{x_{1i}+x_{1j}}\cdot\frac{u(\omega+\Delta_2+\Delta_1-x_{1i})}{2}\\ &\quad+\frac{x_{1i}}{x_{1i}+x_{1j}}\cdot\frac{u(\omega+\Delta_1-x_{1i})}{2}\\ &\quad+\left(1-\frac{x_{1i}}{x_{1i}+x_{1j}}\right)\cdot u(\omega-x_{1i}).\end{aligned}$$

The equilibrium concept is again subgame perfect Nash, but stage-2 investments are essentially fixed at zero. Therefore, we start by considering the derivative of the objective function with respect to x_{i1}:

Table A1
Theoretical predictions.

Treatment	SR	IR	SRc	IRc
Stage-1 investment (x_1)	24	24	30	30
Stage-2 investment (x_2)	24	24		
Prizes (Δ_1, Δ_2)	(72,96)	(72,96)	(72,96)	(72,96)

$$\begin{aligned}\frac{\partial EU_i(\cdot)}{\partial x_{1i}} &= \frac{x_{1j}}{(x_{1i}+x_{1j})^2}\cdot\left[\frac{u(\omega+\Delta_2+\Delta_1-x_{1i})+u(\omega+\Delta_1-x_{1i})}{2}-u(\omega-x_{1i})\right]\\ &\quad-\frac{x_{1i}}{x_{1i}+x_{1j}}\cdot\left[\frac{u'(\omega+\Delta_2+\Delta_1-x_{1i})+u'(\omega+\Delta_1-x_{1i})}{2}\right]\\ &\quad-\left(1-\frac{x_{1i}}{x_{1i}+x_{1j}}\right)\cdot u'(\omega-x_{1i}) \overset{!}{=} 0 \quad .\end{aligned}$$

Using symmetry $x_1^* = x_{1i}^* = x_{1j}^*$ and rearranging terms delivers

$$x_1^* = \frac{u(\omega+\Delta_2+\Delta_1-x_1^*)+u(\omega+\Delta_1-x_1^*)-2\cdot u(\omega-x_1^*)}{2\cdot[u'(\omega+\Delta_2+\Delta_1-x_1^*)+u'(\omega+\Delta_1-x_1^*)+u'(\omega-x_1^*)]}$$

A3. Quantitative predictions

Table A1 summarizes the predicted choices of a risk neutral decision maker in a standard rational-choice model for the parameters used in the implementation of the experiment.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.euroecorev.2017.07.001.

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