Behavior in Rent-Seeking Contests:
The Role of Beliefs, Bounded Rationality, Envy, and Group Identity

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Chapter 1: INTRODUCTION

Rent-seeking is omnipresent. Across different domains of economic and social activities, people compete against each other to obtain a favorable position that brings certain monetary or non-monetary advantages. However, these advantages are not generated in the course of competition, but are rather induced by the re-allocation of resources already present in the economic system. This means costly efforts of rent-seeking activities are economically not productive from a societal perspective.

Rent-seeking is not only often observed in markets (e.g., advertising campaigns), but also in politics (e.g., electoral campaigns, lobbying), in law practice (e.g., litigation) and in sports (see Szymanski 2003 for an extensive overview). Not all rent-seeking is legal (e.g., bribing and vote buying).

Moreover, rent-seeking competition has become more and more frequent in modern economies. It is often organized in forms of 'beauty contests'. In beauty contests, an organizer invites proposals that are costly to prepare for participants. In many industries, such as consulting, construction or architectural design, such procedures have become common. Similarly, in many prestigious sporting events, like the Olympic Games and the Soccer World Cup, the host is chosen through a beauty contest between cities or countries.

The social costs of rent-seeking are difficult to estimate. Scarce empirical literature is often based on only fragmentary data. Still, present analyses confirm that rent-seeking activities can be very costly (Angelopoulos et al. 2009) and can hamper economic growth (Murphy et al. 1993). One can argue that resources spent on rent-seeking activities are not necessarily wasted, *per se* – it is possible that they create new jobs, which may have some positive economic consequences. Still, it is very likely that this is not the most efficient way to spend resources.

Finally, rent-seeking produces inefficiencies in modern job markets. We live in "winner-take-all" societies (Frank and Cook 1995), in which many job markets are characterized by the following remuneration structure: A relatively small number of top performers are paid very well and get additional non-monetary benefits, such as fame or prestige. A large proportion of mediocre-performers are, however, paid poorly and work under precarious conditions. In such markets, a promise of big success attracts a lot of young, talented people who decide to receive an education in a certain domain. Since many of them are deemed to fail, they often end up in jobs below their qualifications and potential. Entertainment (film industry in particular), sports and the arts are the traditional examples of such job markets. A closer look at the higher education and research industries reveals that we face a very similar structure of incentives and a similar disparity between top-performers and almost top-performers in academia. This inefficiency of the misallocation of talents (Frank and Cook 1995, pp. 9-10) may be even more tremendous than the one described earlier. However, it is extremely difficult to estimate the magnitude of the phenomenon and the monetary consequences of wasted talents and skills.

1.1. The economic research on rent-seeking

Given how common, costly and inefficient rent-seeking behavior can be, there is no doubt that the topic deserves economists' attention and it is not surprising that it has been studied in economics for about fifty years now. Economic research on rent-seeking was pioneered by Tullock (1967), Krueger (1974) and Posner (1975). Tullock (1967) was the first who recognized that the endogeneity of political decisions on creating monopolies and tariffs increases social costs of such regulations beyond the Harberger's triangle because some resources are wasted in the contest between economic actors aiming at becoming the monopolist. Moreover, it became rapidly clear that similar situations are frequent in economic and social interactions and that the concept of rent-seeking has a much broader application than the context of striving for a monopolistic position (Congleton et al. 2008b, p. 1).

Economic research on rent-seeking behavior has been accelerated by the seminal work of Tullock (1980), who provided a simple analytical framework of rent-seeking contests.[1] First, it reflects a realistic assumption that the winner in such settings is selected by a combination of

[1] Note that the Tullock (1980) contest is used not only in rent-seeking settings. However, it is its most important application. Therefore, it is common to use the terms "Tullock contests" and "rent-seeking contests" interchangeably. So do I in this thesis.

merit and luck. Second, its analytical simplicity is a great asset that allows for a tractable game-theoretic analysis and numerous theoretical extensions. Tullock's (1980) framework provides researchers with neat and clear game-theoretic predictions. In most cases, a Tullock contest is characterized by a unique Nash equilibrium in pure strategies (Perez-Castrillo and Verdier 1992; Szidarovsky and Okuguchi 1997).

Empirical investigations of rent-seeking are hampered by the difficulty of obtaining reliable data. In many contexts mentioned above, no data is available to research, particularly due to its nontransparent nature – e.g., in lobbying, litigation or bribing. This is why economic experiments offered an appealing way of providing empirical data. The first experiments on rent-seeking were conducted in late 1980s (Millner and Pratt 1989, 1991). Since then, rent-seeking behavior has been studied in economic laboratories across the world. This led to extensive experimental literature on the topic (see recent surveys by Dechenaux et al. 2015 and Sheremeta 2013). Even though economic experiments have substantially advanced economists' understanding of rent-seeking, several robust results from the laboratories remain puzzling. *Overbidding* (i.e., subjects invest significantly more than the Nash equilibrium predicts) and *overspreading* (i.e., subjects frequently use almost the entire strategy space) have gained the most attention (Sheremeta 2013, 2014).

1.2. The scope of the thesis and its findings

This thesis consists of five studies on rent-seeking and contributes to the behavioral and experimental analyses of this phenomenon. Using experimental methods and insights from behavioral economics, I test theoretic predictions of standard economics for rent-seeking settings and complement standard models by behavioral extensions.

Chapter 2 (*Beliefs and Behavior in Tullock Contests*) is joint work with Bettina Rockenbach and is the first systematic analysis of beliefs and behaviors in experimental Tullock contests. In a series of experiments, we investigate how subjects build their beliefs in repeated contests, and, more importantly, how they respond to beliefs. We enrich the standard experimental design with the procedure of incentivized belief elicitation. This allows us to test the theoretical prediction in new dimensions. Our experimental data provides clean evidence for myopic belief formation and thus yields a strong justification for a common assumption in contest models. Moreover, we investigate how subjects respond to expected off-equilibrium strategies of the competitor. We find that *best-responding* is rare in general. Instead, subjects tend to match their belief with their own investment. This leads to a linear response function, which substantially differs from the game-theoretic prediction, and causes the well-documented phenomenon of overbidding. We consider several possible explanations for the revealed behavioral regularities. We show that neither limited computational abilities, nor random mistakes, nor inequity aversion drive the pattern. An analysis of behavior in asymmetric contests brings us to the conclusion that the desire to win is a dominant motive for “matching-behavior”.

Chapter 3 (*On the Reluctance to Play Best Responses in Tullock Contests*) is joint work with Bettina Rockenbach. We investigate the effect of the matching protocol in repeated experimental contests on investment behavior. We test whether the rarity of best responses in Tullock contests is due to the inter-temporal dynamics between partners in repeated contests. Since subjects’ computational limitations might impair the implementation of intended strategies, we also manipulate across treatments the saliency of best responses. We find that the matching protocol itself has no effect on the willingness to best-respond. This holds irrespective of whether best responses are made very salient to subjects or not. On the other hand, providing direct hints at best responses increases their occurrence, albeit to only about 20 percent.

Chapter 4 (*Pushing the Bad Away: Reverse Tullock Contests*), joint work with Bettina Rockenbach, is a study on behavior in reverse Tullock contests. While most research considers rent-seeking for achieving gains, we recognize that contests are often conducted for avoiding losses. We show that the equilibrium prediction under standard preferences is robust against such a variation. However, prospect theory (Kahneman and Tversky 1979) suggests that contests involving negative prizes may be fiercer than traditional contests with positive prizes. We test this hypothesis in a new experiment. We find that average investments in *reverse contests* are higher by 15 percent than in conventional contests. However, the effect is statistically not significant.

Chapter 5 (*Heterogeneous Effect of Group Identity in Collective Rent-Seeking*) investigates collective rent-seeking, in which groups instead of individuals compete against each other for a prize that is a public good for the winning team. The equilibrium analysis predicts that teams in such competition invest as much as individual players. However, Abbink et al. (2010) show that investments in collective contests are much higher than in individual contests. Moreover, the authors find that within groups, large differences in contributions are very persistent. In a new experiment, I replicate Abbink et al.’s results regarding both overbidding and behavioral heterogeneity. I explain the large and persistent within-group heterogeneity in behavior with the heterogeneous effect of group identity. Subjects identify themselves with their teams to very different extents. Those who report a strong attachment to their groups keep investing much, even though their team mates who do not report any attachment to the team consequently free-ride. This shows that group identity is an important driver of contest behavior. Moreover, it demonstrates that group identity may have detrimental consequences in rent-seeking settings, as it boosts efforts of a wasteful nature.

Chapter 6 (*Envy in Dynamic Contests*), which is joint work with Uta K. Schier, is a theoretical and experimental study on the role of envy in dynamic contests. First, we conduct a game-theoretic analysis of a set-up with two battles, in which only the winner of both battles is awarded a prize. A tie in wins (1:1) leads to the prize not being awarded. We demonstrate that the equilibrium predictions under standard preferences, does not hold for subjects that dislike lagging behind; that is, subjects who are inequity averse. We show theoretically that envious losers of the first battle do not give up in the second battle and try to prevent the competitor from winning the prize. We test the theoretical prediction in a laboratory experiment and find clear evidence for envy-driven behavior. We observe that the first-battle-losers frequently do not give up in the second battle. As a consequence, in 30 percent of the cases the prize is not awarded to any contestant. This suggests potential for large inefficiencies.

1.3. Scientific relevance and contribution of the thesis

This thesis provides new results and insights on rent-seeking behavior relevant from both (economic) methodological and political perspectives. Therewith it advances the economic analysis of rent-seeking in two important dimensions.

From the methodological point of view, it contributes to a better understanding of behavior in experimental contests. A profound understanding of motives, decision-processes and goals in the lab is crucial for researchers to be able to interpret observed behavior correctly. The thesis answers several questions important for researchers designing experiments on contest behavior, for example: *To what extent is the matching protocol important in repeated Tullock contests? Is the assumption of myopic beliefs in repeated contests justified? Does a computational tool help solve the problem of not following best responses?* These insights might be useful for future experimental studies on rent-seeking, given the increasing interest in experimental evidence on contest behavior.

From a policy perspective, the studies presented in this thesis help understand what drives competitive behavior in rent-seeking situations and, therefore, offer several implications for policy-makers designing contests. They provide at least partial answers to such questions as: *Why are tie-rules in dynamic contests important and should be chosen carefully? How large is the danger of collusion in rent-seeking contests? Are the contests organized to avoid bad outcomes different from contests organized to obtain a gain? Why can team contests between groups with a strong common identity be dangerous?* Moreover, I consider not only conventional (i.e., individual and static) contests but also richer and more complex settings. Therefore, the findings presented in the thesis better reflect frequent examples of dynamic contests and contests between groups.

Chapters 2 contributes to a vivid discussion in the recent literature on the reasons for overbidding in experimental contests – the research question that has dominated the experimental investigation of rent-seeking in the last years. Bettina Rockenbach and I demonstrate the crucial role of beliefs in explaining overbidding. Anticipation of opponents' aggressive behavior drives higher investments that ends up in well-documented overbidding. This result can be important for both researchers designing contests in the lab and policy-makers.

The data collected in the lab expands the research beyond theoretical considerations and proves that the standard game-theoretic analysis based on the *homo oeconomicus* paradigm requires behavioral supplements. On the one hand, I show that standard assumptions are not met: Subjects' rationality is bounded, they face difficulties with computing best responses, and they are inequity averse. On the other hand, standard analysis misses important non-monetary incentives, such as loss aversion, joy of winning, and group identity.

Chapter 2: BELIEFS AND BEHAVIOR IN TULLOCK CONTESTS

Joint work with Bettina Rockenbach

2.1. Introduction

Rent-seeking is frequent in politics, markets, sports, education, or in research.[1] It causes tremendous monetary and non-monetary costs and often leads to inefficiencies in resource allocations (e.g., Murphy et al. 1993, Stewart and Wu 1997, Angelopoulos et al. 2009). Tullock (1980) proposed a simple model of rent-seeking behavior that has become a standard analytical framework in economic research on contests.[2,3] Since observational field data on rent-seeking behavior are only rarely available, economists have turned to experimental methods to obtain controlled empirical evidence on rent-seeking behavior, extending the research beyond the theoretic considerations and models. More than 25 years of experimental investigation of rent-seeking (pioneered by Millner and Pratt 1989, 1991) has led to an extensive body of literature on experimental contests (see Dechenaux et al. 2015 for a comprehensive overview). In the face of a large number of studies, it is striking that some very robust phenomena of contest behavior observed in laboratories remain unexplained. The main, well-reported (and yet not fully

[1] Konrad (2009) discusses a wide range of applications in more detail.
[2] Earlier on, Tullock (1967) and Krueger (1974) already discussed economic consequences of rent-seeking.
[3] Throughout the paper, the term "contest" refers to Tullock contests. For differences between Tullock contests and other canonical models (tournaments or all-pay auctions), see Dechenaux et al. (2015).

understood) phenomenon is overbidding: Subjects invest systematically more than predicted by the theoretic equilibrium analysis (Sheremeta 2013). Researchers have been exploring several mechanisms that possibly drive this result. Among these, bounded rationality, utility of winning, other-regarding preferences, or probability distortions belong to the most widely considered. Yet in his survey Sheremeta (2013, p. 508) concludes that *"it remains an open question as to whether some of these factors are correlated as if so, which are the most important ones."*

In this chapter we advance our understanding of contestants' overbidding by providing the first study systematically focusing on contestants' beliefs about competitors' behavior and their responses to these beliefs. In a series of laboratory experiments on contest behavior with incentivized belief elicitation, we vary the salience of best-response behavior as well as the presence (and degree) of investment cost asymmetry between players.[4]

Our findings are extremely clear. With regard to belief formation, we provide strong evidence that contestants hold myopic (Cournot) beliefs, i.e., they expect the opponent to invest as much as in the previous period. This pattern has been an implicit or explicit assumption in several previous studies, e.g., Fallucchi et al. (2013), Lim et al. (2014), however, so far without any experimental support. Our main result, however, concerns behavioral responses to beliefs. In symmetric contests, we find that subjects predominantly invest as much as they believe the competitor does. This leads to systematic overbidding. We show that this is not an effect of limited computational abilities, as belief matching still prevails when subjects are equipped with a best-reply calculator. We can also exclude inequity aversion (Fehr and Schmidt 1999) in realized or in expected payoffs as an explanation for the observed behavior. Additionally, we show that (almost) linear response functions cannot be captured with statistical models involving noise, e.g., quantal response equilibrium (McKelvey and Palfrey 1995, Goeree et al. 2005). Finally, with a systematic analysis of contests with asymmetric investment costs, we show that belief matching is not driven by striving for equality in winning probabilities, but instead is in line with the desire to win. With an increasing cost

[4] A very recent study by Sheremeta (2015a) also employs incentivized belief elicitation. However, both the belief elicitation procedure and the focus of the paper are different from ours. We became aware of Sheremeta's study when completing our paper. Moreover, Herrmann and Orzen (2008) utilize the strategy method (Selten 1967) in experimental contests, which, however, is a substantially different approach from the direct response method in our experiment. See also discussion in Section 2.4.

asymmetry, low-cost players more and more heavily exploit their advantageous position by bidding excessively more than needed to achieve equal winning probabilities, whereas high cost players are more likely to surrender.

The remainder of the chapter is organized as follows. In Section 2.2, we present our research agenda. Section 2.3 describes our experimental game and design. In Section 2.4, we report our results on elicited beliefs and behavioral responses to beliefs. In Section 2.5, we study the impact of the bounded computational abilities in experimental contests, and in section 2.6, we explore behavior in asymmetric contests. Section 2.7 concludes.

2.2. Our research agenda

Scholars have investigated several reasons for overbidding. In general, one can classify the examined explanations into those modifying subjects' preferences and those allowing for subjects to make mistakes. Among the first category, social preferences (e.g., Herrmann and Orzen 2008), joy of winning (e.g., Sheremeta 2010) and evolutionary preferences (e.g., Hehenkamp et al. 2004) are most common. However, much attention is also devoted to the hypothesis that subjects make mistakes (see e.g., Potters et al. 1998). Sheremeta (2011) applies the Quantal Response Equilibrium (QRE) approach (McKelvey and Palfrey 1995) to fit the data from experimental contests and finds patterns in line with predictions of the QRE. Similarly, Lim et al. (2014) apply logit quantal responses to fit behavior in contest games with different numbers of players. The authors report that the average expenditure level does not depend on the group size. However, the expenditures are more dispersed in large groups. Lim et al. link their descriptive observations to parameter estimates of the QRE-model and conclude that for larger groups the parameters depart further from perfect rationality.

Our research agenda is closely related to the stream of literature striving to explain overbidding in experimental contests. We provide new insights into rent-seeking behavior by investigating how subjects form their beliefs about the competitors' behavior and how subjects respond to these beliefs. We can organize our research agenda into three steps. In the first step, we extend the standard Tullock contest to include belief elicitation. Two contestants, endowed with E tokens each, may buy tickets at a cost of 1 for a lottery with a winning prize of V. A contestant's winning probability equals the number of one's own tickets bought as a fraction of the total number of tickets bought by both contestants. In our experimental sessions, the same two contestants interact repeatedly over 20 periods. Our BASELINE treatment extends this standard design through incentivized belief elicitation: Contestants are asked to guess the other contestant's investment and earn more money when their guess is more accurate. We find that subjects predominantly hold myopic (Cournot) beliefs: They expect the opponent to invest as much as in the previous period. The main finding, however, lies in the pattern of responses to beliefs: Subjects predominantly invest the amount they believe their competitor invests. A linear reaction function is in stark contrast to the theoretic best-response function and results in overbidding (see Section 2.4).

In the second step (treatment C1), we investigate whether belief matching is caused by computational limitations. We modify our BASELINE treatment by providing subjects with a computational tool, which allows for unambiguously identifying best responses (see Section 2.5). Yet we observe no difference in behavior as compared to the BASELINE, either in the average investment levels or in the reaction functions. Thus, we can exclude limited computational abilities as a cause for belief-matching investments. We can also exclude that observed behavior is driven by inequity aversion (Fehr and Schmidt 1999) in realized or in expected payoffs, both in BASELINE and in C1.

In the third step of our agenda, we introduce investment cost asymmetry between contestants to investigate whether matching investments on beliefs is driven by striving for equality in investments or for equality in winning probabilities. While in treatment C1 both contestants receive 1 ticket per token invested, the asymmetric treatments feature a low-cost and a high-cost player. The low-cost player receives 3 (in treatment C1.5), 2 (in treatment C2) and 4 (in treatment C4) tickets per token, while the high-cost player receives 1 ticket per token invested in C2 and C4 and 2 tickets per token in treatment C1.5. Thus, we vary the level of asymmetry, from relatively low to high. We find that in the asymmetric contests low-cost players exploit their advantageous position by investing even more than they expect from the opponent, resulting in very high winning probabilities. In contrast, as cost asymmetry increases, high-cost players are more likely to give in and not participate in the contest (see Section 2.6).

Table 2.1 presents an overview of our experimental treatments and their main characteristics. In Section 2.3, we describe our experimental design in more detail.

Table 2.1: Overview over the experimental treatments.

Treat-ment	No. of subjects (indep. observ.)	Belief elicit.	Computa-tional tool	Investment costs
BASE-LINE	58 (29)	Yes	No	Symmetry: Both players receive 1 ticket per token
C1	56 (28)	Yes	Yes	Symmetry: Both players receive 1 ticket per token
C1.5	58 (29)	Yes	Yes	Low Asymmetry: Low-cost player receives 3 tickets and high-cost player receives 2 tickets per token
C2	60 (30)	Yes	Yes	Medium Asymmetry: Low-cost player receives 2 tickets and high-cost player receives 1 ticket per token
C4	60 (30)	Yes	Yes	High Asymmetry: Low-cost player receives 4 tickets and high-cost player receives 1 ticket per token

2.3. Experimental game, design and procedure

2.3.1. Experimental game

We design a laboratory environment that employs a two-player rent-seeking contest (Tullock 1980) for a monetary prize V. Assume that both contestants C_1 and C_2 have an endowment E and simultaneously invest in "lottery tickets" for the contest. For each unit invested, contestant C_i receives a_i lottery tickets[5], $i = 1, 2$. If C_i invests x_i and the competitor C_j invests x_j, player C_i wins the contest with probability:

$$p_i = \frac{a_i x_i}{a_i x_i + a_j x_j} \tag{2.1}$$

and has a payoff of:

$$\pi_i = \begin{cases} E & if \quad x_1 = x_2 = 0 \\ E - x_i & if \quad C_j\, won \\ E - x_i + V & if \quad C_i\, won \end{cases} \tag{2.2}$$

[5] Kimbrough et al. (2014, p. 98) refer to the different investment costs as the "conflict strengths."

Player C_i's best reply against contestant C_j's investment x_j is:

$$BR(x_j) = \frac{\sqrt{V a_i a_j x_j} - a_j x_j}{a_i} \tag{2.3}$$

The unique equilibrium is symmetric with investments:

$$x_i^* = x_j^* = \frac{a_i a_j V}{(a_i + a_j)^2} \tag{2.4}$$

Therefore, in the symmetric cost case ($a_i = a_j > 0$), the unique Nash equilibrium under money-maximizing preferences is that both players invest one quarter of the prize, i.e., $V/4$ (Szidarovsky and Okuguchi 1997).

2.3.2. Experimental implementation

In the experiment, we frame the game as a lottery. We choose an endowment of *E*=20 and a prize of *V*=20. Hence, in case of symmetric costs, in equilibrium both contestants invest 5. In addition to the decision on the investment in the lottery, subjects also have to guess what they expect the competitor to invest. We incentivize belief elicitation using a quadratic loss function.[6] Subjects can earn a bonus of up to 4 tokens in every round for their beliefs. The beliefs are rewarded according the following bonus function:

$$\text{Bonus} = \max\{0;\ 4 - 0.4(\text{Belief} - \text{Actual investment})^2\}. \tag{2.5}$$

Thereafter, subjects receive feedback about their own and their competitor's decisions, and the resulting probabilities of winning are provided in numbers and represented graphically (as lengths of a segment) on the screen.[7] All bought tickets are numerated and subjects are informed which numbers correspond to their lottery tickets. If no contestant buys any tickets, the lottery is not conducted and nobody wins the prize. Otherwise, the computer randomly draws one of the purchased tickets. The ticket holder wins the lottery and receives the prize. Both contestants are informed about the winner of the lottery and their payoff.

[6] For the advantages of the quadratic rules as compared to the linear ones, see e.g., Selten (1998) and Palfrey and Wang (2009). We are aware that there is no agreement in economic experimental methodology on whether to incentivize beliefs or not (see for example the recent survey by Schlag et al. 2015).
[7] For translation of experimental instructions, see appendix C.

Thus, the expected payoff (without a bonus for belief reports) of player C_i with an investment of x_i and a competitor's investment of x_j is given by:

$$\Pi_i = 20 - x_i + \frac{a_i x_i}{a_i x_i + a_j x_j} 20 \quad (2.6)$$

In the experiment, the stage game is repeated for 20 periods in partner matching.[8] Every period has the same timing and is payoff-relevant. The experimental tokens are exchanged into Euros at the rate 45 tokens=1 Euro.

2.3.3. Experimental procedure

We conducted the experiment in the spring and fall 2015 in the Cologne Laboratory for Economic Research (CLER), Germany. The participants were students with various majors and were recruited via ORSEE (Greiner 2015). In total, 292 students (59% female) participated in ten experimental sessions split equally between five treatments (see Table 2.1).[9] Each subject participated only in one treatment. Depending on the treatment, experimental sessions lasted between 60 and 90 minutes. On average, subjects earned 14.20 EUR. The experiment was programmed and conducted with the software z-Tree (Fischbacher 2007).

2.4. Beliefs and behavioral responses

Figure 2.1 shows the results from our BASELINE treatment (with $a_i = a_j = 1$). Average investments are 6.48, significantly more than the equilibrium investment of 5 (p=0.013).[10,11] The average magnitude of overbidding (29.7%) is comparable to the results from the meta-analysis by Sheremeta (2013). Another robust finding about

[8] The horizon of the game is standard; most previous experiments applied between 10 and 30 periods. Moreover, in the previous research, both partner and stranger matching have been frequently used (see e.g., Table 1 in Fallucchi et al. 2013 for an overview).

[9] We strived for 60 participants in each treatment (i.e., 300 in total). A lower number of subjects in some treatments is due to several no-shows.

[10] In all reported tests, we consider a pair of players over 20 periods as one independent observation.

[11] Throughout the paper, whenever we report significance levels in tests for differences, we refer to results from a non-parametric two-sided Fisher-Pitman permutation test (with 200,000 runs). Depending on the nature of the data, we apply a version for either independent samples or paired replications. Unlike the Wilcoxon signed-rank test (which is the most common alternative to the Fisher-Pitman test), the Fisher-Pitman test does not draw any conclusion about the underlying population. Therefore, it does not depend on assumptions about the population (e.g., subjects being randomly drawn and symmetric around the median). Moreover, the Wilcoxon test is based on ranks and therefore ignores a substantial part of the information in the sample data. The Pitman-Fisher permutation test uses the more powerful approach based on the original sample values without transformation (see Kaiser 2007; Selten et al. 2011). Unless explicitly noted, all usual significance thresholds hold also under the parametric test (t-test).

contest behavior is overspreading: Subjects not only invest "close" to the equilibrium prediction of 5 but also use the entire strategy space. This is also the case in our data (see Figure 2.10 in appendix A).

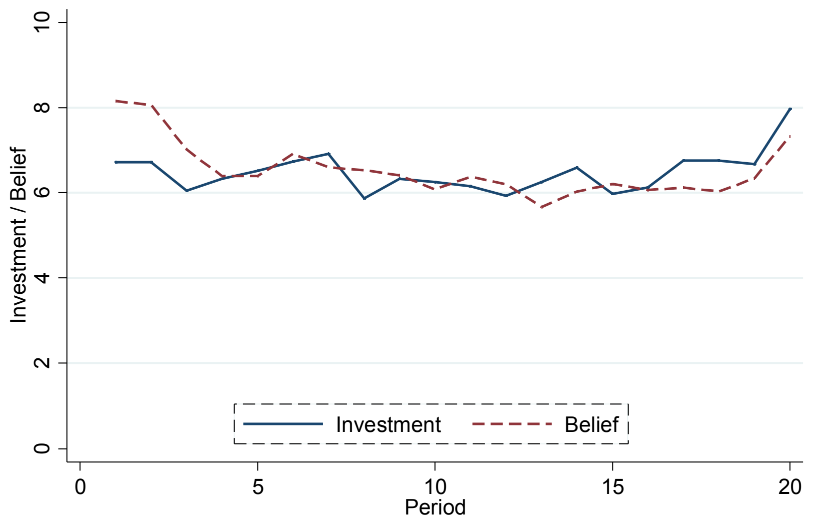


Figure 2.1: Average investments and beliefs (BASELINE treatment).

***Result 2.1**: In our BASELINE treatment, contestants' investments are in line with previous research and significantly higher than predicted in equilibrium.*

2.4.1. Accuracy of beliefs

More than three quarters (77.9%) of the reported beliefs were rewarded with a positive bonus, which means that the absolute inaccuracy was not larger than 3 tokens. More than one third (36.7%) of beliefs exactly matched the competitor's behavior.

Figure 2.2 (panel A) depicts the distribution of belief inaccuracy. Over time, subjects increase their performance in predicting competitor's behavior. The average inaccuracy is 3.13 in the initial five periods of the experiment and 1.81 in final five periods. The difference is highly significant ($p<0.01$).

2.4.2. Myopic beliefs

Several previous experimental studies on contest behavior analyze observed behavior under the assumption of myopic beliefs (e.g., Fallucchi et al. 2013, Lim et al. 2014). Our data allows us to identify to what extent this assumption is justified. We find clear evidence for myopic beliefs under partner matching. Figure 2.2 (panel B) shows that 44.2 percent of beliefs are perfectly myopic. In 68.9 percent of cases the difference between the reported belief and the myopic belief is not larger than 1. Moreover, the distribution of deviations from the myopic beliefs is symmetric. We can summarize this as:

Result 2.2: *Subjects' beliefs are highly accurate. They predominantly hold myopic beliefs or beliefs very close to myopic.*

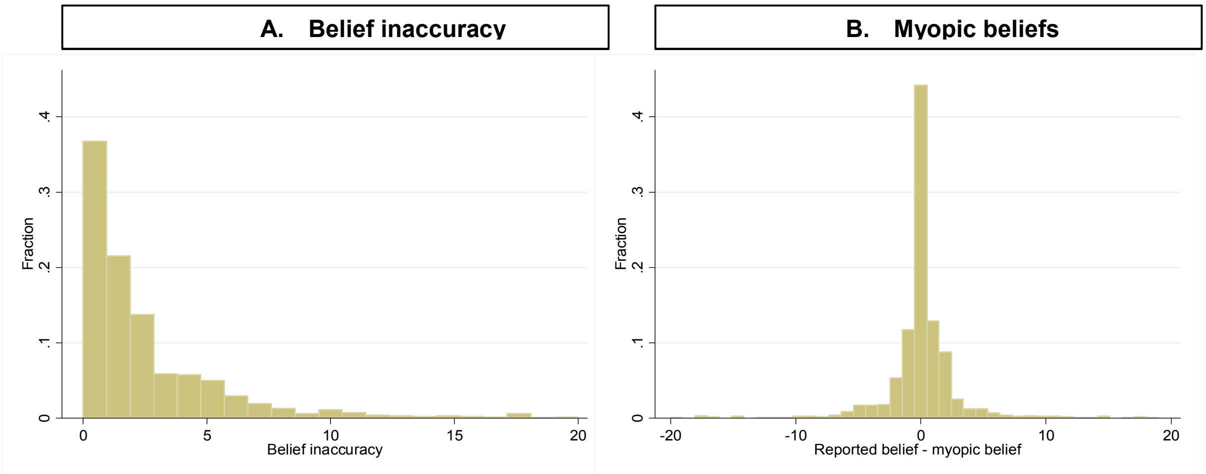


Figure 2.2: Beliefs in BASELINE treatment.

2.4.3. Behavioral response to beliefs

In the next step, we study how subjects behave when holding certain beliefs. We compare the theoretic prediction with the behavioral responses to beliefs observed in our experiment. Figure 2.3 depicts the average investments for a given belief about competitor's behavior and contrasts them with the theoretic prediction. Moreover, we plot the frequencies of reported beliefs.

The difference between the theoretic prediction and observed behavior is remarkable. Subjects substantially deviate from playing best responses. Instead, their behavior can be described with a linear reaction function. Subjects tend to invest as much as they expect from the competitor, which means that they match their investments with their beliefs. Whereas such behavior is justified in the symmetric equilibrium (both players invest 5 tokens), the theoretic best reply is never higher than 5 tokens and decreases for beliefs larger than 5. We find that subjects do not follow this rule. As a consequence, the discrepancy between theoretic prediction and observed behavior becomes larger the more aggressive the competitor is expected to be. The figure additionally shows that investment behavior cannot be described by playing a best reply under Fehr-Schmidt-preferences (1999), either in expected or in realized payoffs (for more details see appendix B).

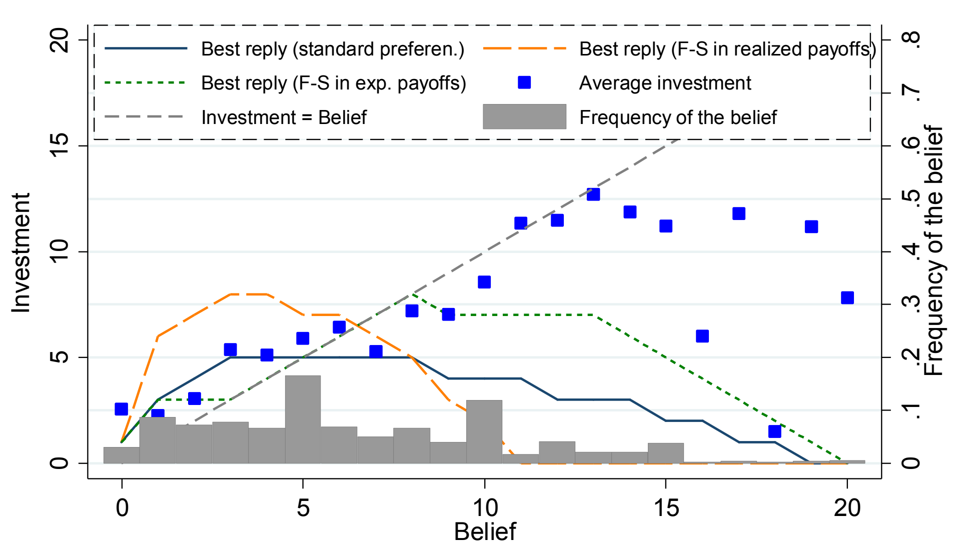


Figure 2.3: Average responses to beliefs (BASELINE treatment).

The matching pattern becomes even more apparent from the investment-belief-ratio, presented in Figure 2.4. In order to demonstrate that the results are not driven by the symmetric equilibrium nor are just an arithmetic phenomenon, we present the distribution for all beliefs (Figure 2.4, panel A) and separately the distribution only for beliefs larger than 5 (Figure 2.4, panel B). In both cases, the pattern is very similar; we observe a significant peak at the value of 1, where the investment exactly matches the belief. The significant role of the revealed heuristic is also confirmed in the regression analysis (see Table 2.3 in appendix A). We conclude the following:

Result 2.3: *Subjects display a pattern of a linear reaction function (matching behavior with beliefs), which is in stark contrast to theoretic best replies.*

The linear reaction function helps explaining why most previous studies on rent-seeking behavior report overbidding. It is likely that such results are driven by the responses to aggressive competitor's behavior (or beliefs of high competitor's investment).

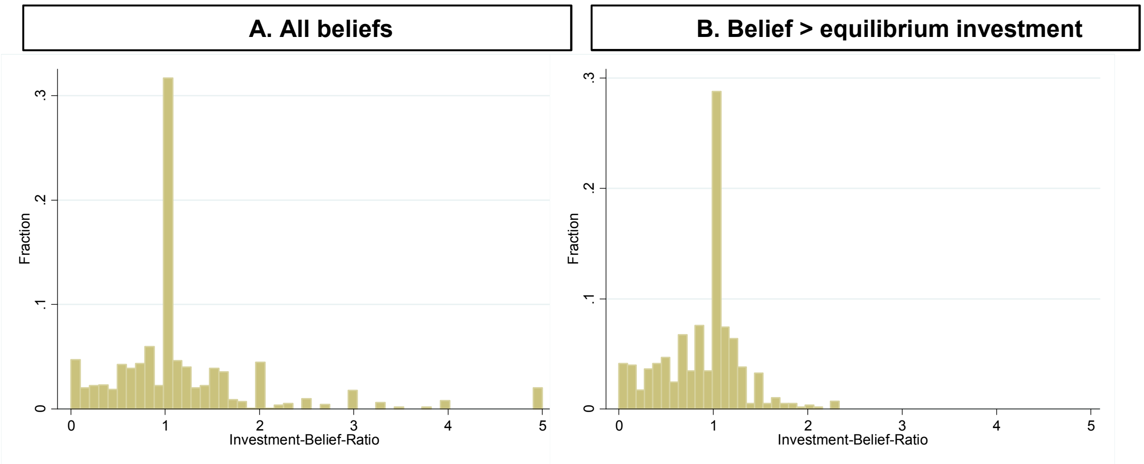


Figure 2.4: Investment-Belief-Ratio (BASELINE treatment).
Note: Investment-Belief-Ratio for beliefs different from 0 and censored at 5.

We are aware of one previous study that employs elicitation of response functions. Herrmann and Orzen (2008) use the strategy method (Selten 1967) in a similar fashion as Fischbacher et al. (2001) do for public good games. Our result of a linear response function is not quite in line with the evidence reported by Herrmann and Orzen (2008), but it also does not strictly contradict their findings. The authors observe that about 1/3 of subjects in the repeated interactions display an increasing response function, whereas almost ½ of subjects display a hump-shaped response curve (Herrman and Orzen 2008, Table 4). Moreover, even for increasing-types, the reaction curve is far from linear, especially for lower investments by the competitor (Herrmann and Orzen 2008, Figure 4). There are several possible design-related sources of these discrepancies with our results. The behavioral changes induced by the strategy method (instead of the direct responses in our experiment) are certainly one of to the most likely reasons (see Brandts and Charness 2011).

Why do contestants deviate from best replies and match beliefs? Is it possible that the observed matching pattern is induced by the procedure of belief elicitation and we face a problem of an inverse causality, in which the chosen action induces reported beliefs? We can address this concern by comparing our results to previous experiments from Abbink et al. (2010) and Ahn et al. (2011), which are very similar in design but did not elicit beliefs. Both experimental studies include treatments where two players compete repeatedly in partner matching for an exogenously given prize. The main difference to our design is the value of the prize and, therefore, the resulting strategy space. In both previous experiments, subjects invested an integer number between 0 and 1,000 to win a prize of 1,000 tokens. All other main elements are the same as in our design.[12] We compute average responses to beliefs under the assumption of myopic beliefs and present the results in a fashion analogous to Figure 2.3 in appendix A (Figures 2.16 & 2.17). Since in the previous experiments the investments were spread over a large strategy space, we aggregate them in 50-token increments in order to obtain reliable numbers of observations for computing average responses. We see that the matching pattern can also be found in the data collected by Abbink et al. (2010), as well as Ahn et al. (2011). This strongly speaks against the concern that the matching pattern is an artifact of our experimental procedure.

Another explanation for the observed matching pattern could stem from the repeated play. Investment matching could be used as a punitive action to penalize aggressive competitors in order to deter their investments in the future. However, such motivation should lead to more matching behavior in early rounds and less towards the end of the experiment. A panel probit regression provides evidence against this explanation (see Table 2.4, Panel A in appendix A). We do not observe less matching in the later rounds of the experiment than at the beginning. Moreover, a recent study by Schier and Waligora (see Chapter 6) reports a similar linear response function in one-shot Tullock contests, which also speaks against the role of dynamic interactions in the matching behavior.

2.4.4. Observed responses to beliefs in the light of QRE-approach

Previous studies used the quantal response equilibrium (QRE) model to explain experimental data on contests. QRE adds noise to the optimal behavior and helps to explain the over-dissipation of rents. We analyze how such an approach fits our results. As a statistical equilibrium concept, QRE relies on payoff perturbations and assumes that mistakes follow a random process. McKelvey and Palfrey (1995) build their approach on the assumption that better actions (in terms of payoffs) are more likely to be chosen than worse actions. Both Sheremeta (2011) and Lim et al. (2014) apply the QRE-model to their aggregated data and estimate the precision parameter λ. In contrast

[12] The only noteworthy difference from our design is the lump-sum endowment in Ahn et al.'s experiment, which is different from the per-period endowments in Abbink et al.'s as well as in our experiments.

to these studies, we do not conduct a complex econometric analysis, but rather apply the basic intuition of QRE to our experimental data.

One of the assumptions underlying QRE is that the probability of a mistake is reversely proportional to the foregone payoff. Hence, more costly mistakes are less likely. Figure 2.5 presents the distribution of investments in the BASELINE if the reported belief equals 10, for which the best response under standard preferences is 4. According to the QRE, smaller deviations from the best response should be more frequent than large (fairly costly) deviation. Even taking into account the distortive impact of prominent numbers (5, 10, 15), one cannot explain with the notion of payoff perturbations why a more costly mistake of investing 10 is three times more likely than a less costly mistake of investing 5. This clearly contradicts an explanation of the matching pattern drawn from QRE. We report similar regularities for other beliefs (see Figure 2.15 in appendix A).

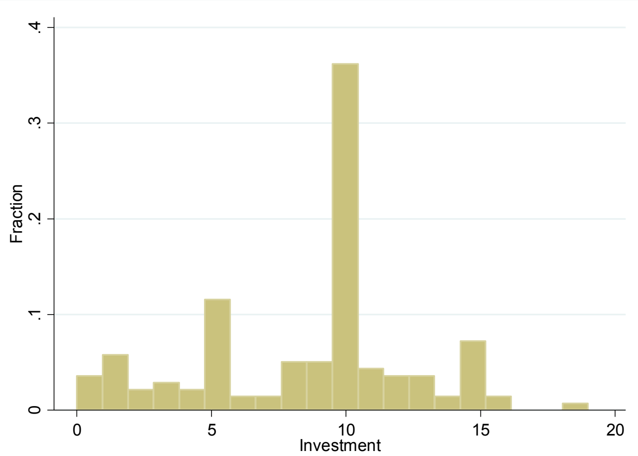


Figure 2.5: Distribution of investments if the reported belief equals 10 (n=138 from 36 subjects). BASELINE treatment.

2.5. The role of limited computational abilities

Tullock contests between two players with linear probabilities are characterized by a well-defined best-response function (see eq. 2.3). Its algebraic form and its shape are, however, neither trivial nor intuitive. In our experimental treatment C1, we test whether the matching pattern is induced by subjects' difficulties in computing probabilities and expected payoffs.[13] The only difference between C1 and BASELINE is the availability of a computational tool. At the decision stage, the computer mask includes a what-if calculator (see Figure 2.21 in appendix C). Subjects can enter the expected investment of the competitor, and the calculator displays expected winning probabilities and expected payoffs for every possible investment of the subject. Such a tool allows easily identification of the investment maximizing subject's expected payoff as well as the one maximizing the expected winning probability, given a certain belief on competitor's behavior. Moreover, subjects can compare their expected payoffs and winning probabilities with the expectations for the competitor in various scenarios. Subjects are allowed to use the calculator as many times as they want within the given decision time limit of 240 seconds in first ten rounds and 120 seconds in consecutive ten rounds.

Our experimental data show that the behavior in treatment C1 is – at the aggregate level – very similar to that in BASELINE. Neither investments (mean: 6.855) nor beliefs (mean: 7.268) are significantly different from the results reported in the previous section (p-values 0.631 and 0.368, respectively). Additionally, Figures 2.10 to 2.13 in appendix A illustrate that the behavior and reported beliefs in C1 are in several other dimensions very similar to those in BASELINE.

In the next step, we test whether subjects in treatment C1 are more likely to play best responses than in the BASELINE treatment. In BASELINE, in 15.43 percent of observations, subjects play exactly best responses; in C1 this proportion amounts to 15.00 percent. The difference is not significant ($p>0.9$). The same conclusion provides the panel data logit regression (see Table 2.4, Panel B in appendix A).

One obvious reason for no difference between treatments would be that subjects do not use the calculator. Therefore, we examine whether participants indeed use the tool we provided. Fifty-three out of 56 subjects (94.6 percent) use the calculator in at least one period. Figure 2.20 in appendix A depicts that most subjects make use of the tool at the beginning of the experiment. In periods 1-3, the average number of entries is 1.60, but it decreases rapidly, and by period 10 it drops to 0.18. The use of the calculator in the first periods is reflected in much longer decision times. Whereas in the BASELINE

[13] Note that we call this treatment C1 due to notation we use in asymmetric treatments reported in Section 2.6 (C stands for "calculator").

subjects in periods 1-3 make their decision on average within 12.18 sec., in C1 they need 52.34 sec. (see Figure 2.19 in appendix A). The difference is highly significant ($p<0.001$).

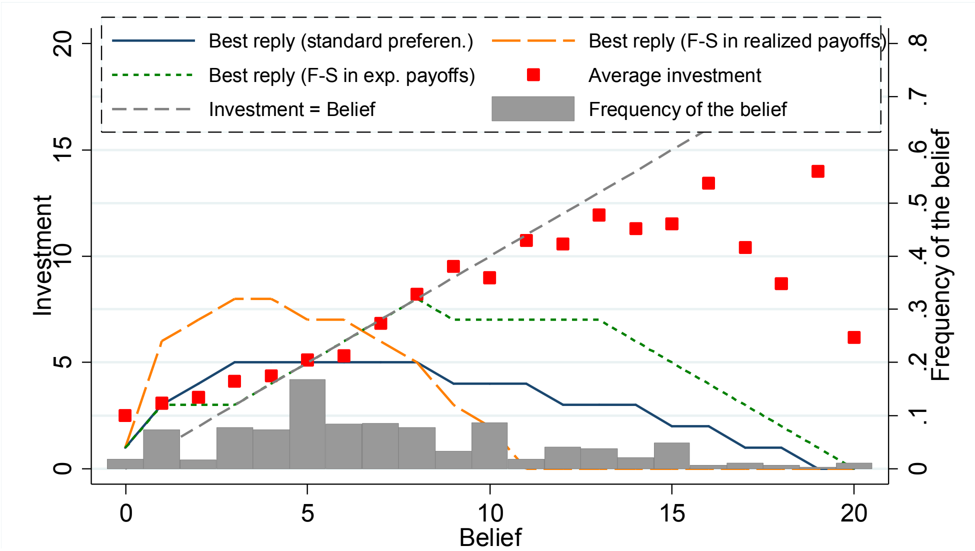


Figure 2.6: Average responses to beliefs (treatment C1).

Figure 2.6 demonstrates that the matching pattern in C1 is even slightly more pronounced than in the BASELINE. The regression analysis also confirms that the matching pattern is not induced by the limited computational abilities (Table 2.4, Panel A in appendix A). The figure additionally shows that, as in BASELINE, investment behavior cannot be described by best-reply behavior under Fehr-Schmidt-preferences (1999), either in expected or in realized payoffs (for more details see appendix B).

Result 2.4: *Subjects use the what-if calculator but still do not play best responses, in particular for high beliefs. Instead they tend to match their own behavior with beliefs. Thus, limited computational abilities do not seem to be the reason for the matching pattern in experimental contests.*

2.6. Beliefs and Behavior in Asymmetric Contests

In order to disentangle whether the observed investment behavior is motivated by matching the opponent's investment or matching the winning probability, we introduce cost asymmetry between contestants.[14] In three new treatments we manipulate the degree of asymmetry from relatively low to high. This provides richer insights into contest behavior and enhances our understanding of it.

In asymmetric treatments, we vary the number of tickets subjects can purchase per token ($a_i \geq 1$). The symmetric treatment C1, reported in the previous section, constitutes our benchmark. Consequently, the decision is framed across treatments as the number of tokens that a subject wants to spend on lottery tickets. Table 2.2 summarizes the ticket costs in the treatments. The treatment name corresponds to the ratio tickets per token ($a_{low-cost\ player}/a_{high-cost\ player}$). Moreover, using eq. (2.4) we can compute the theoretic benchmarks of Nash equilibrium.[15]

Table 2.2: Asymmetric treatments. Overview.

	Treatment	**C1**	**C1.5**	**C2**	**C4**
# tickets per token spent	low-cost type	1	3	2	4
	high-cost type	1	2	1	1
Nash equilibrium		(5, 5)	(5, 5)	(4, 4)	(3, 3)

2.6.1. Main behavioral regularities

In all asymmetric treatments, both low-cost and high-cost players invest significantly more than in the Nash equilibrium (all $p<0.05$). The detailed statistics and test results are presented in Table 2.5 in appendix A.

In order to recognize general behavioral patterns, we analyze in the first step the distributions of winning probabilities subjects believed they would achieve when making their decisions (see Figure 2.7). Expected winning probability results from own investments and reported beliefs. Inter-treatment and -type comparison allows us to identify three major behavioral regularities.

First, we find that under low cost asymmetry (treatment C1.5) both low-cost and high-cost types tend to strive for equal or similar winning probability as their competitor.

[14] With respect to asymmetric contests, our study is related to previous work by Anderson and Stafford (2003), Fonseca (2009), Anderson and Freeborn (2010) and Kimbrough et al. (2014).

[15] Note that we have to take the restriction on the strategy space into account; Subjects are allowed to invest integer numbers of tokens.

The exact matching is not as pronounced as in the symmetric treatment (C1); this is probably partially attributable to exact matching in probabilities not always being possible under the asymmetric cost due to restrictions on the strategy space. Nevertheless, a great mass of distribution lies around 0.5. This is no longer the case under a larger cost asymmetry.

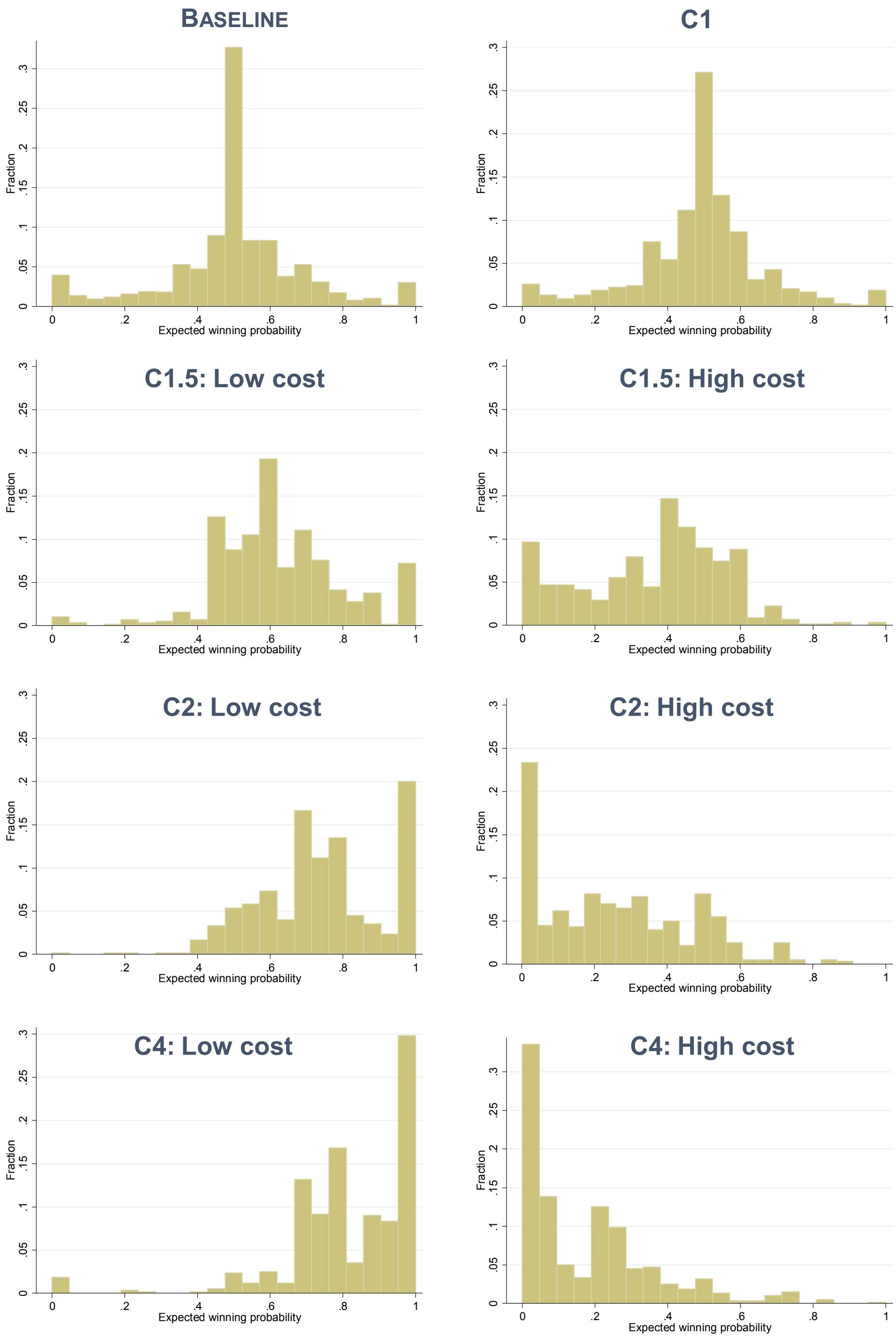


Figure 2.7: Expected winning probability. Empirical distributions by treatment and cost type.

Second, low-cost types strongly match in investments, which – combined with a cost advantage – allows them to achieve high winning probabilities of 60%, 67%, and 80% for treatments C1.5, C2, and C4, respectively.

Third, high-cost types are more likely to give in when being highly disadvantaged. While the fraction of contestants investing zero is negligible under cost symmetry (in BASELINE and C1), it increases with an increasing cost asymmetry between contestants. Under high cost asymmetry, about one third of high-cost contestants decide to "surrender" by not buying even a single lottery ticket (see Figure 2.7 right panels).

2.6.2. Matching pattern under cost asymmetry

To answer the question posed at the beginning of this Section, namely whether the investment behavior is driven by a desire for equal investments or equal winning probabilities, we compare how often subjects follow these behavioral rules. Since exact matching under cost asymmetry was not always possible, we slightly weaken the definition of matching and allow for a deviation from the described rules by at most one token. Figure 2.8 depicts the likelihood of following the matching rule across treatments.

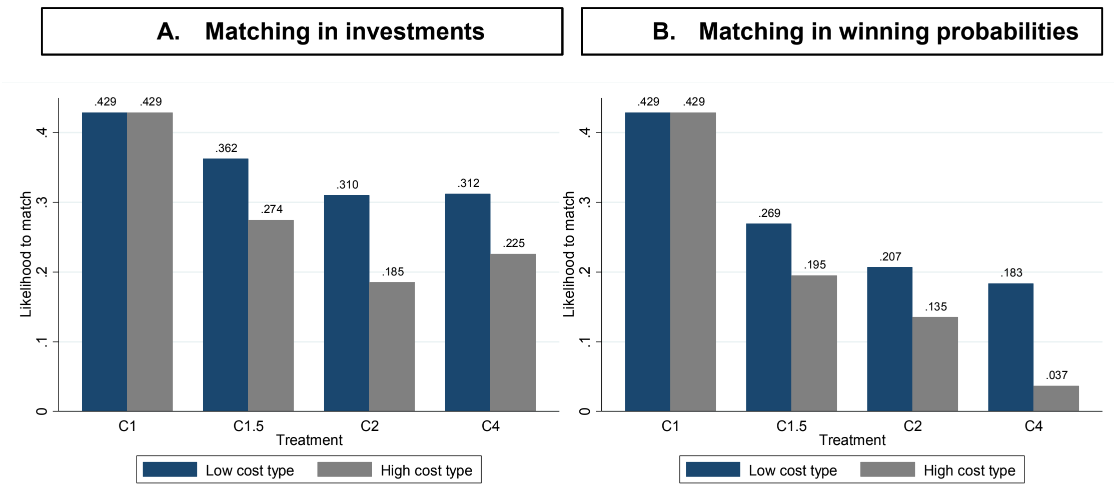


Figure 2.8: Likelihood to match in investments (panel A) and in winning probabilities (panel B).

The stronger the cost asymmetry, the lower the likelihood of low-cost players matching both in investments and in winning probabilities. However, the decreasing trend for the likelihood of matching winning probabilities is much stronger. This is confirmed by non-parametric analyses. While the Jonckheere-Terpstra test (JTT) for ordered alternatives indicates an only weakly significant negative trend for likelihoods to match investments (p=0.052), it detects highly significant negative trend for the likelihood to match in probabilities (p<0.001, JTT). Moreover, the same conclusion provides a parametric analysis reported in Table 2.6 panel A in appendix A, where a much stronger and more significant effect of cost asymmetry is found for the matching in winning probabilities.

For high cost players, both matching rules become less and less frequent under increasing cost asymmetry; JTT indicates in both cases a highly significant negative trend (p<0.001). Again, this conclusion is supported by the parametric regression analysis (see Table 2.6 panel B in appendix A). As the matching rules become less common under cost asymmetry, it is instructive to compare average behavior to the considered behavioral benchmarks. We normalize each decision with respect to predictions of matching in investments or matching in winning probabilities. A value of 100% means that the matching behavior is exactly followed, while values over (under) 100% indicate that a subject invests more (less) than the matching rule requires. The average values of normalized behavior are presented in Figure 2.9.[16]

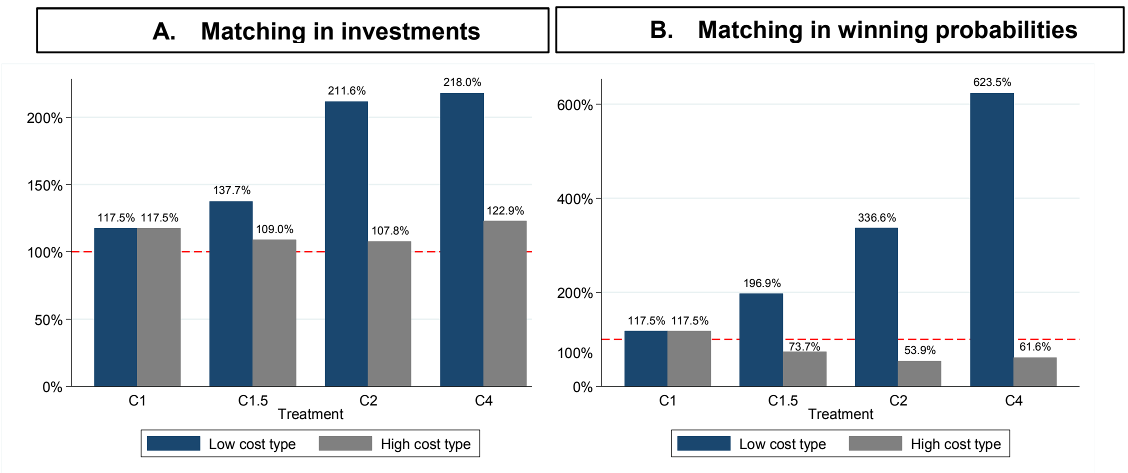


Figure 2.9: Average behavior normalized with respect to matching rules.

In the symmetric treatment C1, subjects invest on average only slightly more than required for investment matching. In asymmetric treatments, however, low-cost

[16] Note that if the reported belief is 0, the matching rule also predicts 0, which disables the normalization of the behavior. In these cases, we replace the benchmark of 0 with the benchmark of 1 and compare behavior with it.

players invest on average more than matching in investments would require; we observe a significant upward trend ($p<0.001$, JTT). Combined with the cost advantage effect, this gives them a rapidly increasing dominance in winning probabilities ($p<0.001$, JTT).

High-cost players, in contrast, continue matching in investments even when the asymmetry increases, such that we find no significant trend ($p=0.413$, JTT). Under the increasing disadvantageous cost asymmetry, this leads to a significant downward trend in probability matching ($p<0.001$, JTT).

We close our analysis of behavior in asymmetric contests with patterns of average responses to beliefs (see Figure 2.18 in appendix A). Here, again it becomes apparent that high-cost players tend to match in probabilities if the cost asymmetry is low (C1.5) and the expected opponent's investment is relatively small. Otherwise, average responses follow the matching investment rule.

We summarize our evidence on behavior in asymmetric contests in the following conclusion:

Result 2.5: *Subjects strive for equal winning probabilities only if the degree of asymmetry is low. Otherwise, low-cost players exploit their position to achieve high winning probabilities and often even deter the high-cost competitor from participating in the contest.*

2.7. Conclusion

In our study, we extend the standard experimental setting of rent-seeking contests from previous research to include incentivized belief elicitation, which allows us to gain new insights into contest behavior. We show that our experimental design is not very intrusive and allows us to report results consistent with previous findings. However, it enriches our understanding of behavior in experimental contests in two significant dimensions. First, we find evidence for myopic (Cournot) beliefs; in repeated interactions under partner matching, subjects usually expect the competitor to behave exactly or similarly as in the last previous period.

Moreover, the belief elicitation allows us to examine the empirical response function and to compare the behavioral responses with theoretic predictions. In stark contrast to the game-theoretic best-reply function, which is parabolic with its maximum in the Nash equilibrium, we find a nearly linear response pattern. Subjects tend to match the beliefs about competitor's behavior with their own behavior. Such regularity helps to explain why most previous experiments on Tullock contests report overbidding. While the best-reply function predicts a gradual surrendering for high beliefs, subjects tend to reciprocate aggressive behavior, which leads to costly rent-dissipation and results in well-documented overbidding.

This result has political and economic implications because it reveals that rent-seeking contests can easily intensify, which results in dissipation of huge amounts of resources. When expecting a competitor to behave aggressively, people usually respond with similarly aggressive behavior. This is in line with the notion of the proverb existing in several languages: "*attack is the best form of defense.*" However, this is in stark contrast to predictions of the game-theoretic analysis. Having found such a systematic and clear deviation from the best-reply function, it is not surprising that we do not observe Nash equilibrium behavior in rent-seeking experiments.

The results can be interpreted in a broader context of competitive settings. Our study demonstrates how dangerous, costly and welfare-decreasing contests can be. People only rarely recognize that giving up is a better strategy than taking part in a very fierce competition. They are usually willing to make costly sacrifices to have a chance of winning against determined competitors. A prominent example is the educational and professional choices to enter a highly discriminating job market, e.g., for professional actors/actresses, musicians or scientists.

To better understand the behavioral pattern of matching, we introduce cost asymmetry between contestants in a new series of experimental treatments. This allows us to better comprehend motives of matching and investigate whether behavioral patterns are robust against asymmetries. We find evidence that subjects strive for an equal (or at least similar) winning probability if the degree of asymmetry is small. However, under a larger cost asymmetry, low-cost players tend to exploit their favorable position by investing excessively more than necessary to achieve equal winning probabilities and dominate the contest by successfully deterring high-cost players. Observed linearity of the response function seems motivated by the desire to win.

2.8. Appendix A. Additional tables and figures

Table 2.3: Adjustment model (panel regression).

Dependent variable: Inv*(t)* - Inv*(t-1)*	**BASELINE**	**C1**	**Baseline & C1 (pooled)**
Best response*(t)* - Inv*(t-1)*	0.233***	0.241***	0.233***
	(0.067)	(0.045)	(0.066)
Matching*(t)* - Inv*(t-1)*	0.361***	0.422***	0.361***
	(0.080)	(0.049)	(0.079)
Calculator			-0.153
			(0.236)
Calculator x [Best-response*(t)* - Inv*(t-1)*]			0.008
			(0.080)
Calculator x [Matching*(t)* - Inv*(t-1)*]			0.061
			(0.093)
Constant	0.584***	0.430***	0.584***
	(0.179)	(0.157)	(0.177)
Number of observations	1102	1064	2166
Number of clusters	29	28	57
Wald Chi-squared	87.88***	136.16***	245.76***

Notes: Panel regression with random subject effects. In parentheses, robust standard errors clustered at pairs of players. Significance levels *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Comments:

Our approach is analogous to those adopted by Huck et al. (1999) as well as Fallucchi et al. (2013). We investigate which rule (playing best-responses vs. matching) more strongly drives behavior (measured with period-by-period adjustments). For details on the approach see the cited studies.

We find that in both symmetric treatments (BASELINE & C1) the matching rule has a stronger effect on the behavior than best replies. Moreover, there is no significant treatment effect of the what-if-calculator.

Table 2.4: Panel probit regression for the event of matching behavior with beliefs (Panel A) and playing best responses (Panel B).

A. Dependent variable: 1[Investment=Belief]	BASELINE & C1 (pooled)	BASELINE & C1 (pooled)
Period	0.016*	0.023**
	(0.008)	(0.011)
Belief		-0.031
		(0.029)
1[Matching in equilibrium]		0.355***
		(0.119)
1[Calculator provided]		-0.118
		(0.342)
Period x 1[Calculator provided]		-0.022
		(0.015)
Belief x 1[Calculator provided]		-0.005
		(0.035)
Constant	-0.947***	-0.796***
	(0.100)	(0.275)
Number of observations	2280	2280
Number of clusters	114	114
Wald Chi-squared	8.51***	40.21***

B. Dependent variable: 1[Investment=Best-reply]	BASELINE & C1 (pooled)	BASELINE & C1 (pooled)
Period		-0.014
		(0.011)
Belief		-0.068**
		(0.032)
1[Calculator provided]	0.113	-0.124
	(0.229)	(0.384)
Period x 1[Calculator provided]		0.043**
		(0.017)
Belief x 1[Calculator provided]		-0.033
		(0.046)
Constant	-1.384***	-0.806***
	(0.194)	(0.277)
Number of observations	2280	2280
Number of clusters	114	114
Wald Chi-squared	0.4	56.79***

Notes: Panel probit regression with random subject effects. In parentheses, robust standard errors clustered at pairs of players. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

Table 2.5: Summary statistics across treatments and types.

Treatment	Type	Cost: Ticktes pro token	Equilibrium prediction	Average investment	Standard deviation	H0: Investment = NE
BASELINE		1	5	6.483	4.391	p=0.013
C1		1	5	6.855	4.612	p=0.001
C1.5	Low-cost	2	5	7.007	4.168	p=0.001
C1.5	High-cost	3	5	6.540	4.781	p=0.013
C2	Low-cost	2	4	6.529	3.750	p<0.001
C2	High-cost	1	4	4.970	4.900	p=0.032
C4	Low-cost	4	3	5.553	3.979	p<0.001
C4	High-cost	1	3	4.788	5.512	p=0.004

Notes: p-values from the Fisher-Pitman permutation tests (two-sided) at the level of independent observations.

Table 2.6: Behavior in asymmetric treatments (panel probit regression).

Dependent variable:	**A. Low cost types**		
	1[surrender]	**1[Match investments]**	**1[Match probabilities]**
Cost asymmetry	0.026	-0.121*	-0.315***
	(0.166)	(0.064)	(0.085)
Constant	-3.424***	-0.182	-0.137
	(0.704)	(0.154)	(0.199)
Observations	2900	2900	2900
Clusters	117	117	117
Wald Chi-squared	0.03	4.03**	15.34***

Dependent variable:	**B. High cost types**		
	1[surrender]	**1[Match investments]**	**1[Match probabilities]**
Cost asymmetry	0.717***	-0.224***	-0.629***
	(0.130)	(0.071)	(0.082)
Constant	-3.797***	-0.206	0.174
	(0.398)	(0.163)	(0.181)
Observations	2899	2899	2899
Clusters	117	117	117
Wald Chi-squared	28.02***	12.30***	65.43***

Notes: Panel probit regression with random subject effects. In parentheses, robust standard errors clustered on pairs. Significance levels *** p<0.01, ** p<0.05, * p<0.1.

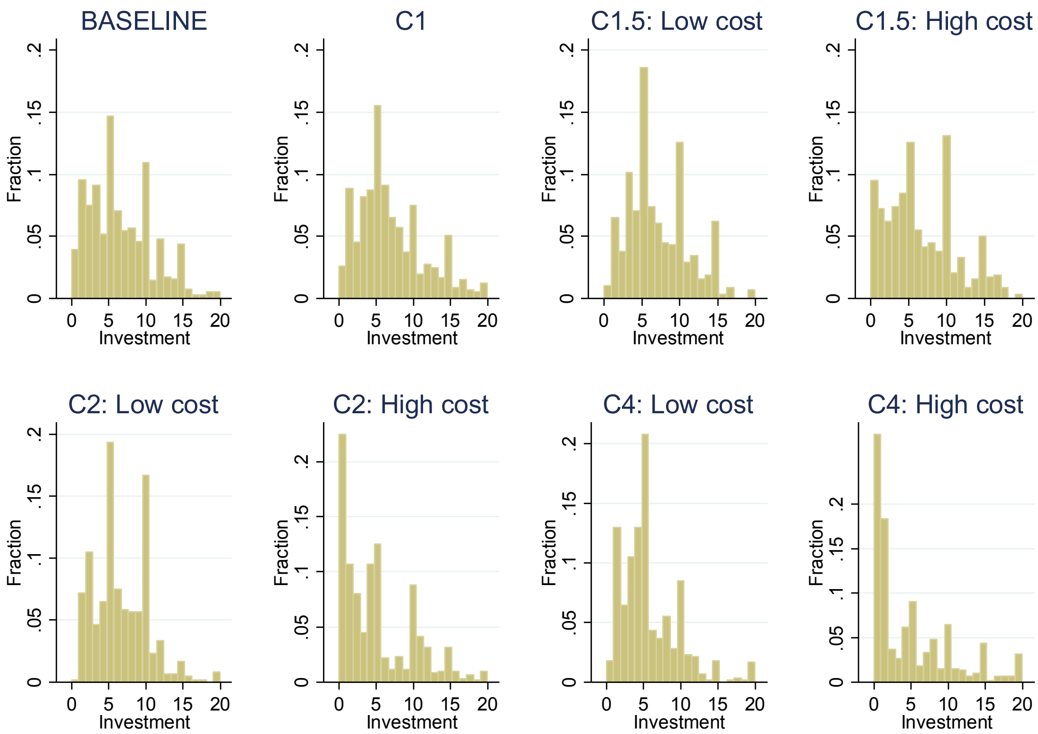


Figure 2.10: Distribution of investments over treatments.

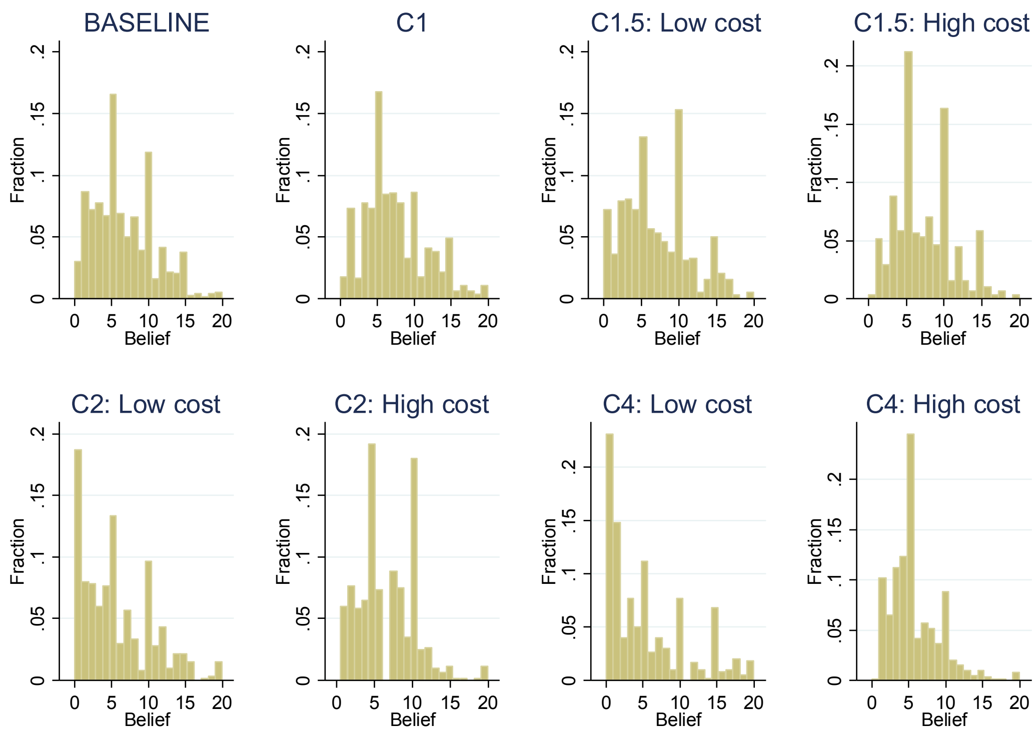


Figure 2.11: Distribution of beliefs over treatments.

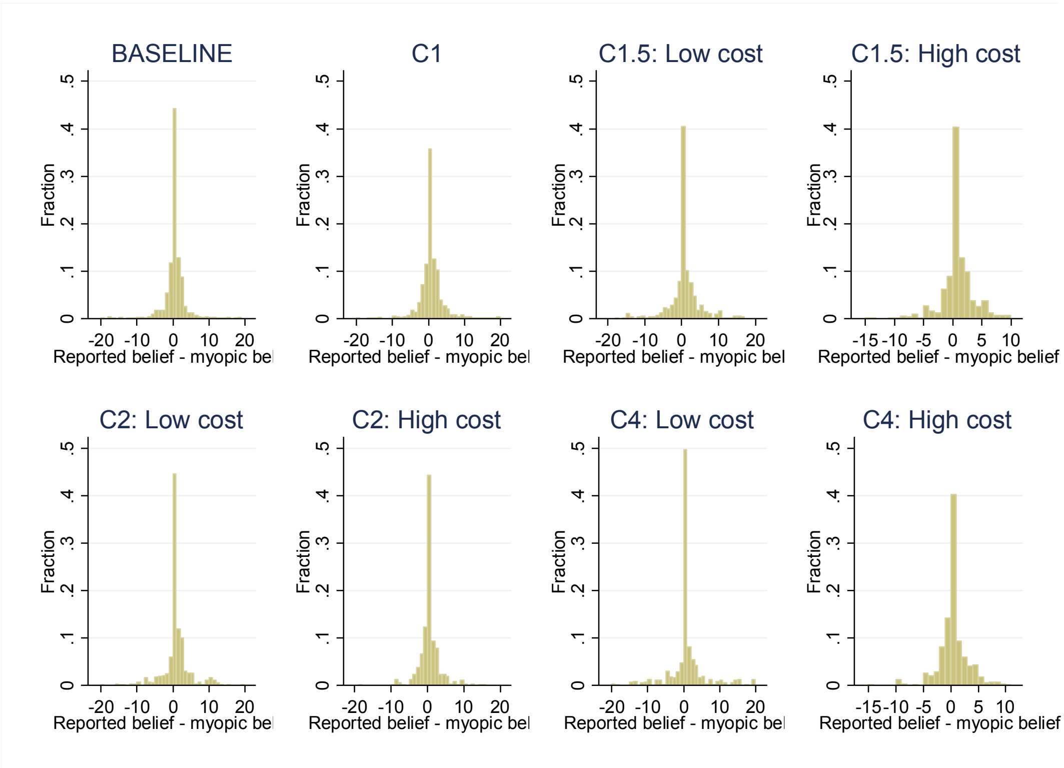


Figure 2.12: Myopic beliefs over treatments.

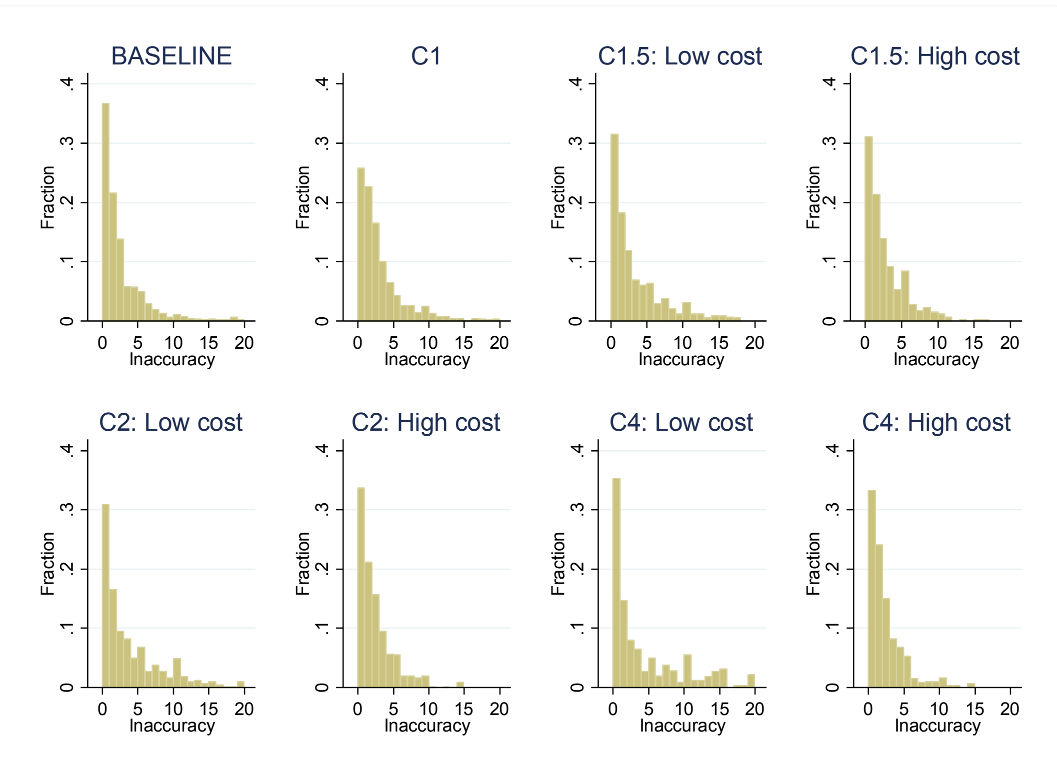


Figure 2.13: Belief inaccuracy over treatments.

Note: Inaccuracy defined as the absolute value of the difference between the belief and the actual competitor's behavior.

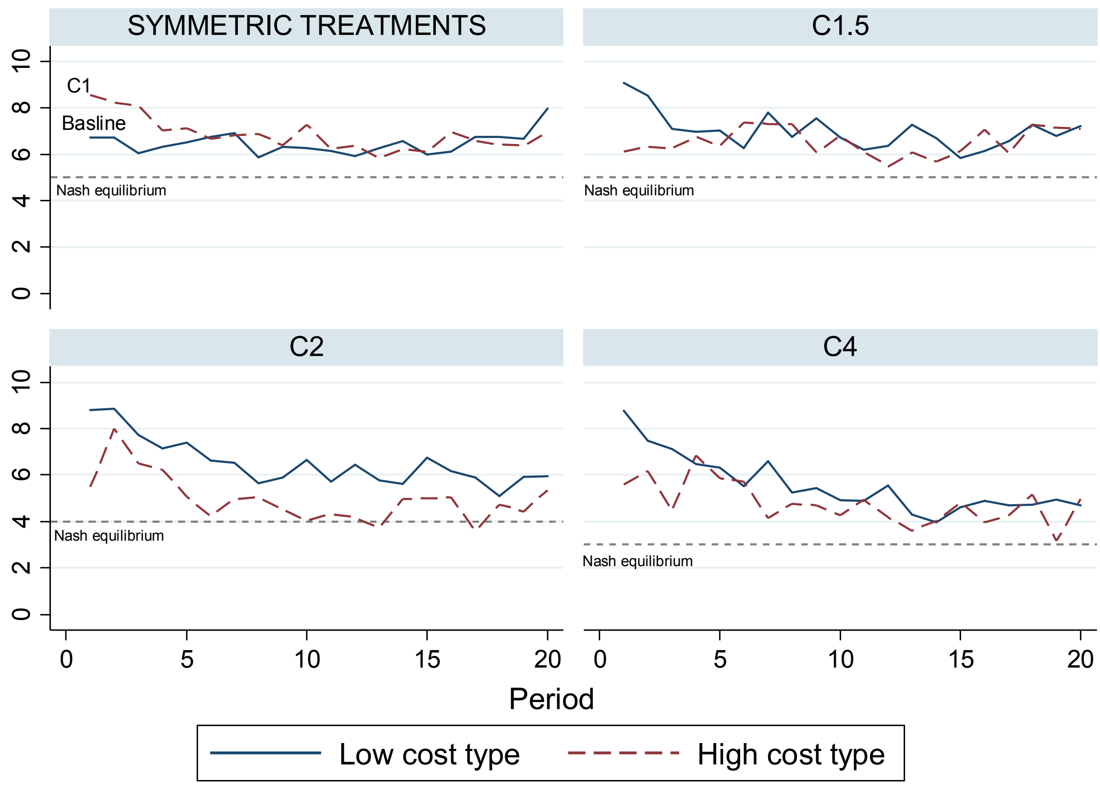


Figure 2.14: Average investments over treatments.

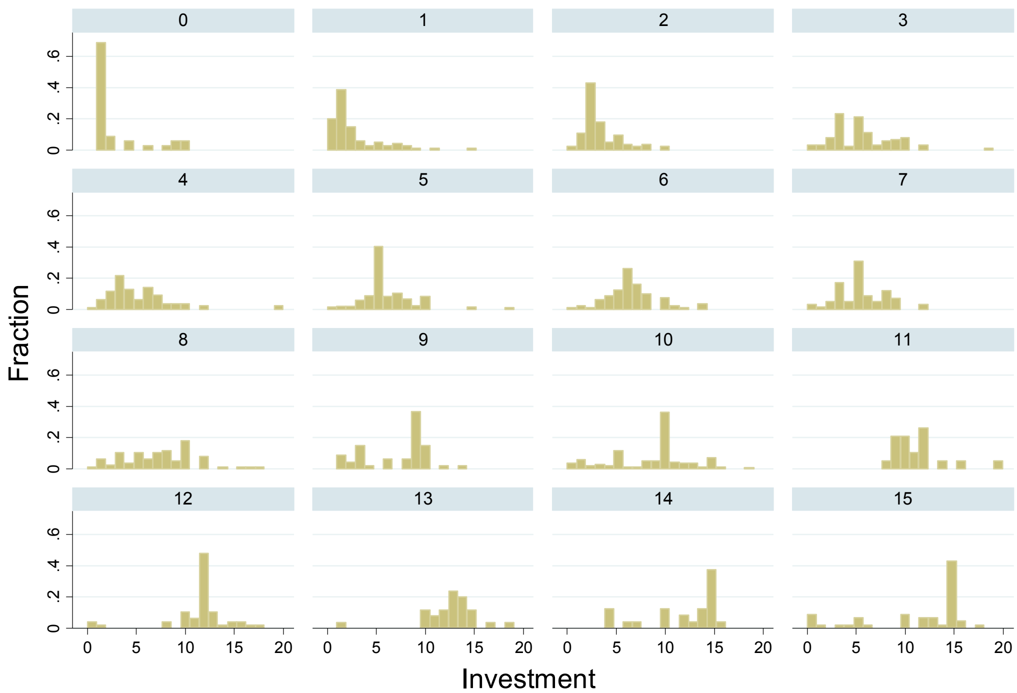


Figure 2.15: Distribution of investments for a certain belief (BASELINE treatment).

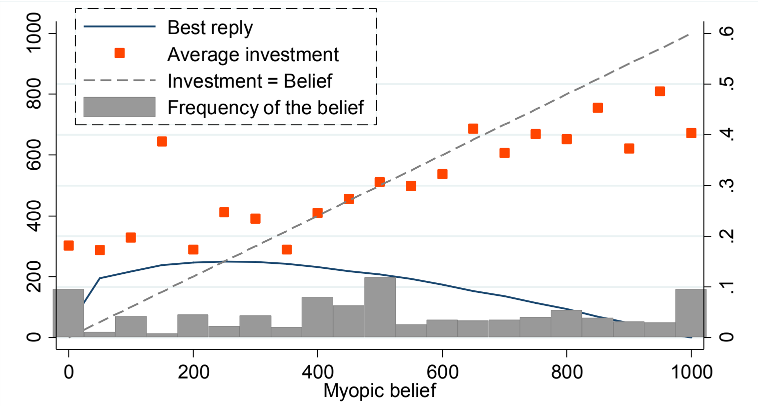


Figure 2.16: Average responses to myopic beliefs in the experiment by Abbink et al. (2010).

Note: We use the data from 1:1-treatment (28 subjects in partner matching, i.e., 14 independent observations, over 20 periods). The data is publicly available under: https://www.aeaweb.org/articles.php?doi=10.1257/aer.100.1.420 [last access: July 3, 2015]

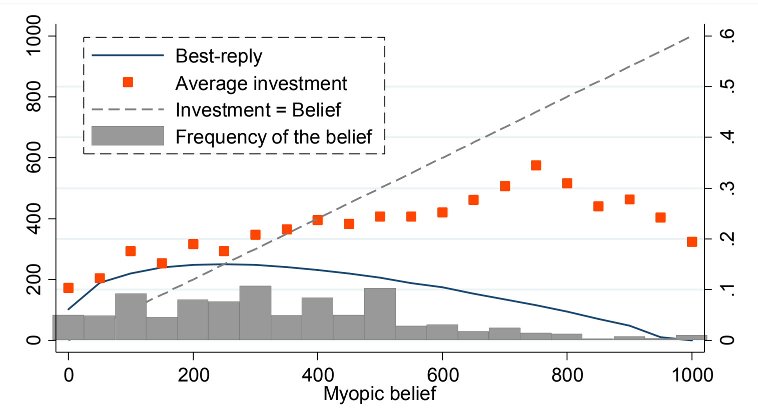


Figure 2.17: Average responses to myopic beliefs in the experiment by Ahn et al. (2011).

Note: We use pooled data from the first part in treatments baseline, AG, and SG. The data was kindly provided by Tim Salmon.

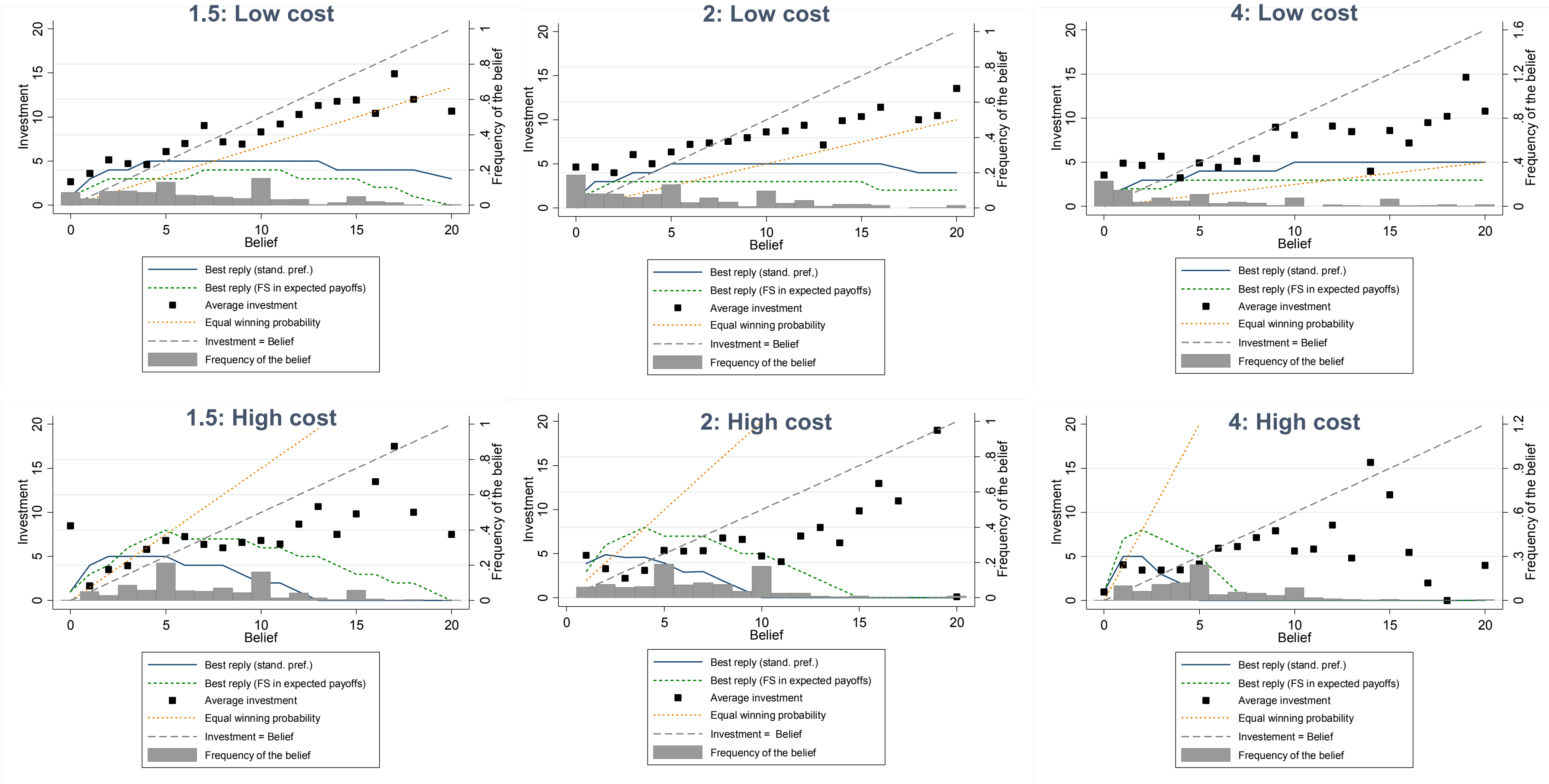


Figure 2.18: Average responses to beliefs. Asymmetric treatments.

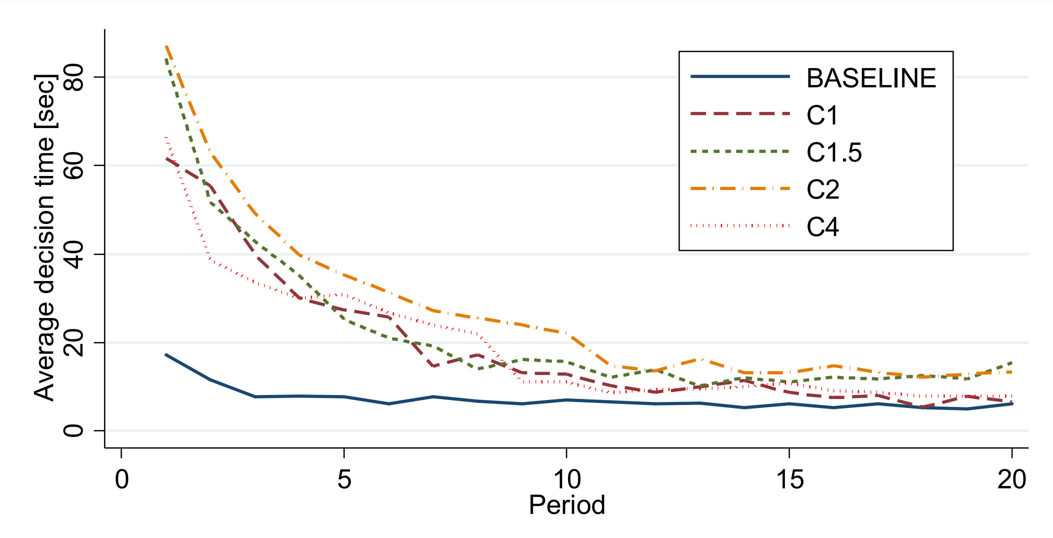


Figure 2.19: Average decision times over treatments.

Note: In all treatments, subjects had max. 240 seconds of time for decision in periods 1-10 and 120 seconds in periods 11-20.

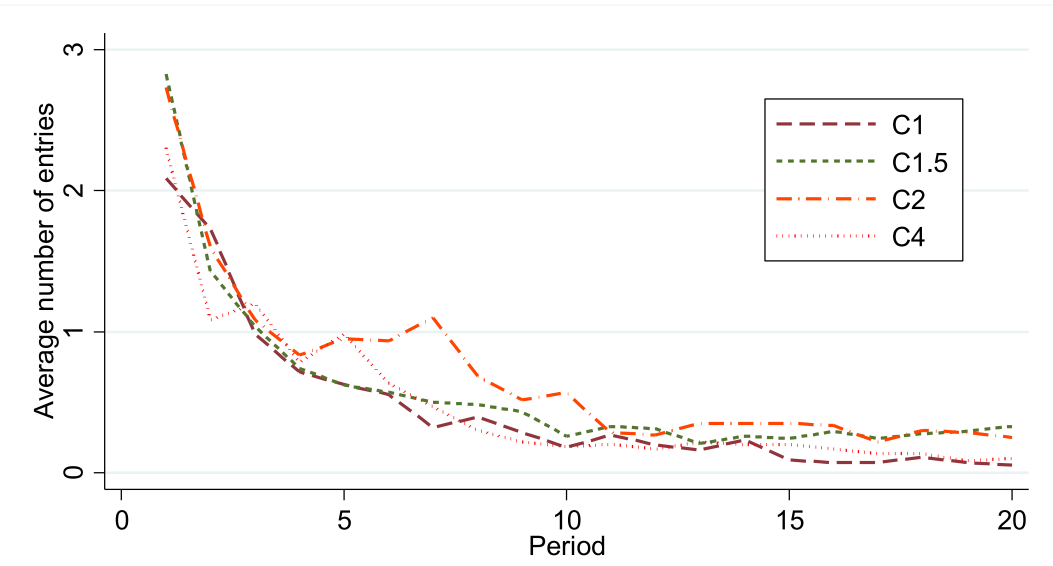


Figure 2.20: Average number of entries in the what-if-calculator.

2.9. Appendix B. Contest behavior under Fehr-Schmidt preferences (1999)

We investigate the effect of inequity aversion on behavior in Tullock (1980) contests and utilize the standard modeling approach by Fehr and Schmidt (1999). Player i's utility depends on both her own monetary payoff and the equity of payoffs between players.

$$U_i = \pi_i - \alpha_i \max\{0;\, \pi_j - \pi_i\} - \beta_i \max\{0;\, \pi_i - \pi_j\}$$

As noted by Herrmann and Orzen (2008), it is not straightforward how inequity aversion works in the studied setting. In general, one can consider two modeling approaches: Players suffer from inequity in realized (ex-post) payoffs, or they dislike inequity in expected terms. Since both alternatives can be rationalized, we leave open which approach is more accurate and consider both. For the sake of simplicity, we assume that the inequity parameters (α, β) are symmetric between two contestants and common knowledge.

First, following Herrman and Orzen (2008) we assume that players are inequity averse in realized payoffs. Therefore, given our experiment parameters E and V in the symmetric treatments BASELINE and C1, contestant C_i's utility is expressed with:

$$E[U_i] = 20 - x_i + \frac{x_i}{x_i + x_j}\Big(20 - \beta\big(20 - x_i + x_j\big)\Big) - \alpha\,\frac{x_j}{x_i + x_j}\big(20 - x_j + x_i\big)$$

Alternatively, if subjects display inequity aversion with respect to expected payoffs, C_i's utility is expressed with:

$$E[U_i] = 20 - x_i + \frac{x_i}{x_i + x_j}20 - \alpha \max\left\{0;\, \frac{x_j - x_i}{x_i + x_j}20 + x_i - x_j\right\}$$
$$- \beta \max\left\{0;\, \frac{x_i - x_j}{x_i + x_j}20 - x_i + x_j\right\}$$

Taking into account the estimates of the parameters from various experiments considered in Fehr and Schmidt (1999) as well as those reported by Blanco et al. (2011), we assume in our analysis the following, modest values of the inequity aversion parameters: $\alpha = 1;\ \beta = 0.25$.

We use the above utility specifications and assumed parameter values to derive best replies under the restriction of limited strategy space (integer numbers between 0 and 20). The results are plotted in Figures 2.3 and 2.6.

2.10. Appendix C. Decision screens and experimental instructions

Figure 2.21: Decision screen in treatment C1 (also part of the written instructions to subjects). Translation from German.

Figure 2.22: Feedback screen in treatment C1. Translation from German.

Figure 2.23: Contest result screen in treatment C1. Translation from German.

Instructions in treatment C1 (translation from German)

General information

Welcome to our experiment! It is important that you carefully read and understand the following instructions. If you have a question, please raise your hand. We will then come to you and answer it. Communication with other participants before and during the experiment is prohibited. If you violate this rule, you will have to leave the experiment and will not receive any payment.

You can earn money in this experiment. You will receive 2.50 EUR for your participation. You can earn additional money during the experiment. The amount of money you earn depends on your decisions and decisions of other participants in the experiment. Your earnings are denoted in tokens. These will be converted in EUR and paid out in cash at the end of the experiment. The exchange rate is:

45 tokens = 1 EUR.

The experiment consists of several rounds. Tokens you earn in each round are added to your tokens account. Your payoff is the sum of the tokens you have earned in all rounds of the experiment. No participant will receive information about your payoff in the experiment or your identity.

Course of the experiment

In today's experiment you will interact with another participant. In a minute you will be randomly matched with an opponent. In every pair there will be a player A (marked blue) and a player B (marked red). The roles are also randomly assigned. The pairings as well as the roles remain unchanged for the entire experiment. Information about your role will be displayed on the screen before the first round begins.

The experiment consists of **20 rounds**. All rounds proceed in the same way. In each round, you and your opponent will compete in a lottery for a prize.

At the beginning of each round, you receive from us **20 tokens**. Then, you can decide how many tokens you want to spend on lottery tickets. For one token you can purchase one ticket (1 token = 1 ticket). You can purchase as many tickets as you want, but you are not allowed to exceed your budget. Tokens that you do not spend on lottery tickets are added to your account.

The prize that you can win in the lottery in each round is **20 tokens**.

Your chance of winning depends only on how many tickets you have bought and how many your opponent has bought. The more tickets you have bought, the more likely it is that you win. Another way around, the more tickets your opponent has bought, the less likely it is that you win. The probabilities with which you win the prize are equal to the number of your tickets divided by the number of all tickets bought. This means it is computed according to the following rule:

$$\text{Your probability of winning} = \frac{\text{Number of your tickets}}{\text{Number of your tickets} + \text{Number of opponent's tickets}}$$

Your earnings in a single round are as follows:

Your earnings if you win = 20 – your investment in the tickets + 20

Your earnings if you lose = 20 – your investment in the tickets

If only one player has bought tickets, she/he wins with certainty. If neither of the players has bought any tickets, the lottery does not take place and nobody wins the prize.

Each round consists of three steps:

Step 1

In the first step, you decide how many tickets you would like to purchase. At the same time, your opponent makes the same decision.

While you make your decision, you can use a what-if-calculator. You can insert a hypothetical investment of by your opponent, and the calculator computes the winning probabilities and expected payoffs for you and your opponent, subject to your investment.

The expected payoff is computed in the following way:

$$\text{Expected payoff} = \text{Probability that you win} \times \text{Payoff if you win} + \text{Probability that you lose} \times \text{Payoff if you lose}$$

You can use the calculator as often as you want. Please just insert a value in the field the opponent's investment and click on "Compute" (see red field (1) in Figure 1).

The example in Figure 1 shows the what-if-calculator for a hypothetical investment of 10. You can see in the table that your probability of winning is 16.67%, and your expected payoff equals 21.33 if you invest 2 tokens. If you invest 19 tokens, is your winning probability is 65.52% and your expected payoff 14.10.

In every round you should insert within the given time your final decision in the fields on the right-hand side of the screen and confirm with the button "OK" (see red field (2) in figure 1). You are asked not only about your investment, but also what investment you expect from your opponent. For this you can earn a bonus of maximal **4 tokens**. The amount of the bonus depends on how good your prediction was. The smaller the mistake in your prediction, the higher your bonus. The bonus is computed in the following way.

$$\text{Bonus} = 4 - 0.4 * \text{Mistake}^2$$

The mistake is the difference between your prediction and actual opponent's investment:

$$\text{Mistake} = |\text{Your prediction} - \text{actual opponent's investment}|$$

If the mistake is larger than 3, the bonus is 0. This means there is no negative bonus; you cannot lose any tokens for your prediction.

Time for your decision in every round is limited. In the first ten rounds you have 4 minutes time, in the next 10 rounds 2 minutes.

Step 2

In the second step, you get feedback on the opponent's decision, in other words, how many tickets she/he has bought. Probabilities of winning are also computed and displayed. All tickets bought are numerated. You get information on which numbers correspond to your tickets and which to tickets of your opponent. In order to make it clearer, this is also displayed graphically.

Each ticket is equally likely to be drawn.

You also receive feedback on how good your prediction about your opponent's investment was and how large your bonus is.

Step 3

In the last step, the winning ticket is drawn. The computer draws one of bought tickets. The number of the winning ticket and the winner are displayed on the screen, as are your earnings in the current round.

[Subjects received also a screen shot of the decision stage mask – see Figure 21. The only difference is that we marked decision fields with red frames – as described in the instructions above.]

Chapter 3: ON THE RELUCTANCE TO PLAY BEST RESPONSES IN TULLOCK CONTESTS

Joint work with Bettina Rockenbach

3.1. Introduction

Despite a long tradition of experimental investigation of rent-seeking contests (Tullock 1980), behavior observed in the lab is still not fully comprehended. As summarized by Sheremeta (2013, 2014), several robust findings remain puzzling. Most attention has been devoted to *overbidding*, whereby subjects in experimental contests invest on average more than the Nash equilibrium predicts and equilibrium behavior is rarely observed in the lab. One could argue that equilibrium behavior is not supported by the theory if the opponents play off-equilibrium strategies. In a recent study, Rockenbach and Waligora (see Chapter 2) conducted a systematic analysis of beliefs and behavior in experimental Tullock contests to answer the question whether best responses to off-equilibrium-behavior are more frequently played. They show that the average response function observed in the lab is almost linear, which is very different from the theoretic prediction. The observed pattern is induced by frequent belief-matching behavior (i.e., subjects investing as much as they expect from the opponent) rather than best-responding. This suggests that best responses are rare in general. Furthermore, they do not become more frequent over time in repeated contests.

In the current study, we strive to understand why best responses in experimental contests are rare and investigate the effect of the matching protocol on investment behavior in repeated Tullock contests. Partner matching is a common design feature in experiments on contest behavior. However, both theoretical analyses and experimental evidence from previous studies prompt us to expect that repeated interactions with the same competitor make subjects follow other goals than maximizing expected payoffs from the current period. Subjects might attempt to deter the opponent from the competition (Selten 1978), they might strive for collusion (Andreoni and Croson 2008; Huck et al. 2004), or they can influence opponents' future behavior in other ways, e.g., through punishment (Fehr and Gächter 2000) or reputation building (Kreps et al. 1982; Bohnet and Huck 2004).

Hence, different matching protocols may induce different goals and strategies. However, in the context of a rent-seeking contest, subjects may face computational difficulties that hamper the implementation of the intended behavior. For instance, the best reply function in symmetric two-player contests is neither trivial nor intuitive. Therefore, subjects might not best-respond simply because they cannot compute best responses. In order to control for potential computational limitations, in our study we not only manipulate the matching protocol, but also the saliency level of best responses across experimental conditions.

The results we report here are clear. In our experiment, the matching protocol has no significant effect on average investments or the response function. Therefore, we find no evidence of strategic non-best-responding. This result holds regardless of providing hints at what the best response is. On the other hand, we show that making best responses extremely salient increases the willingness to play them. The frequency of best response behavior rises from 3 to 22 percent. This means that limited computational abilities are at least partially responsible for the rarity of best responses in experimental contests.

The remainder of the chapter is organized as follows. In Section 2, we formulate our research questions and review related literature. In Section 3, we briefly sketch the theoretical framework. Section 4 describes our experimental design and the research hypotheses. We present the results in Section 5, before concluding in Section 6.

3.2. Research questions and related literature

Our study contributes to research on rent-seeking behavior and has both political and methodological implications. We investigate whether different matching protocols in repeated contests induce different strategies and behavioral patterns. In repeated interactions between partners – relying on the argument of backward induction – the unique equilibrium prediction of a stage game holds if the number of repetitions is finite and known to all players. However, experimental results have shown that actual behavior often differs.

The deviation from the equilibrium prediction may have various causes, like information asymmetry or uncertainty about the rationality of the competitor (see Kreps and Wilson 1982; Milgrom and Roberts 1982; Kreps et al. 1982) or deterrence strategies (Selten 1978): Subjects bid very aggressively in the initial round of the contest to deter the opponent from the competition in the later rounds. Moreover, experimental evidence suggests that the matching protocol is likely to influence the willingness to cooperate despite the horizon of the game being finite. Similar patterns have been shown for instance in experiments on public goods provision (see Andreoni and Croson 2008 for an overview).

The question about the role of the matching protocol in bidding behavior is relevant for both researchers designing experimental contests and contest designers outside of economic laboratories. If repeated interactions between contestants lead to systematic behavioral distortions, designers of both experimental and outside-the-lab contests shall be more careful about who to let compete with whom in repeated contests and whether to provide information about the identity of other competitors. In experimental contests, both random stranger and partner matching have been very common. However, the evidence on the effects is mixed. In a meta-analysis, Sheremeta (2013) compares behavior observed across 30 independent studies finding no systematic effect of the matching protocol. By contrast, Baik et al. (2016) who recently investigated the effect of the matching protocol and group size in experimental Tullock contests report significantly more overbidding in two-player contests under stranger protocol than among partners. Not only do we contribute to the discussion on the effect of the matching protocol on average bidding behavior, but we also extend this question by a new aspect, investigating how the matching protocol alters choices of strategies, particularly considering the willingness to play best responses to reported beliefs.

The implementation of intended strategies induced with different matching protocols might be impaired by computational limitations that subjects face during the experiment. This is likely the case because the environment of a Tullock contest can be cognitively demanding for subjects in the lab. Two previous studies provide clues on the relation between the complexity of the contest environment and behavior. In a cleverly designed experiment, Masiliunas et al. (2014) show that making contests cognitively simpler for subjects increases the explanatory power of the Nash equilibrium. The authors thus conclude that "*bounded rationality rather than preference heterogeneity is the reason for the typically large behavioral variation in experimental Tullock contests*" (Masiliunas et al. 2014, p. 21). However, Masiliunas et al. manipulate the ease of formulating best responses differently than we do. Rather than providing subjects with a computational tool, the authors replace the lottery contest with a share contest, in which a prize is divided and distributed to players according to their shares in the total investment. Such a variation does not change the equilibrium prediction as an expected value of the prize is simply replaced with a realized value of the prize. The authors argue that the process of identifying best responses is nevertheless facilitated due to the lesser uncertainty of outcomes. Masiliunas et al. show that simply replacing the lottery contest with a share contest is insufficient to induce a higher frequency of best responses and further simplifications are required (see Masiliunas et al. 2014, Result 2).

Such a conclusion is additionally supported by a study by Chowdhury et al. (2014). The authors also manipulate features of contest design, keeping the equilibrium prediction unchanged. Similar to Masiuliunas et al., Chowdhury et al. find that if the prize sharing rule is the only manipulation, the magnitude of overbidding does not change.[1] Accordingly, both studies show that one can induce behavior closer to Nash equilibrium when manipulating the contest in at least two dimensions, whereby the share rule is one of them. Still, this finding does not answer the question of why best responses

[1] Fallucchi et al. (2013) also compare behavior in experimental contests under both the share and the lottery rule. However, the focus of their analysis is different and the authors do not report whether the difference in average investments between the corresponding treatments is statistically significant. Moreover, Shupp et al. (2013) compare behavior across different prize allocation rules. However, the authors observe *underbidding* (i.e., investments significantly smaller than equilibrium prediction), which is in stark contrast to rich evidence on overbidding in experimental contests.

are so rare in the original form of the contest. The result of more frequent best responses in other contests does not explain the rarity of best responses in conventional Tullock contests.[2] Rather than manipulating the contest rules, we investigate behavior in the original form of the Tullock contest and provide subjects with direct information concerning what the best response is. This is what makes our study complementary to those reported above.

3.3. The theoretical framework

We consider rent-seeking contests (in a framework due to Tullock 1980) between two ex-ante symmetric risk-neutral expected-payoff maximizing players (called A and B). Both contestants are budget-constrained with the initial budget of E and compete for a single and non-divisible monetary prize V. In the course of the contest, both players simultaneously spend monetary resources, which determine their winning probabilities. While investments made by both contestants are sunk, the outcome of the competition is stochastic. Assume that player A invests x_A and player B invests x_B. Subsequently, the winning probabilities are as follows:

$$p_A = \frac{x_A}{x_A + x_B} \ ; \quad p_B = 1 - p_A \tag{3.1}$$

Therefore, the expected payoff of player $i \in \{A, B\}$ is:

$$E[\pi_i] = E - x_i + p_i V \tag{3.2}$$

One can define this situation as a static game of complete information. It has a unique pure strategy Nash equilibrium, in which both players invest V/4 (Szidarovsky and Okuguchi 1997). Moreover, the best response function is at its maximum in equilibrium and all investments higher than the equilibrium level are strictly dominated.

3.4. The experiment

3.4.1. Experimental implementation

The Tullock contest between two subjects is framed in the experiment as a lottery for the prize of V=100 tokens. The stage game proceeds always in three steps. First,

[2] See Selten (1990, p. 651) for a similar argument in the discussion on the chain-store-paradox, as well as Chowdhury et al.'s (2014, p. 233) conclusion from their experiment.

subjects are endowed with E=100 tokens, whereby they can decide how many they want to spend on lottery tickets. For one token they purchase one ticket. The strategy space is restricted to integer numbers. At the same time, subjects report what investment they expect from the competitor. Belief elicitation is incentivized, whereby subjects are paid according to a quadratic loss function between 20 and 0.[3,4] In the second step, subjects receive feedback about their opponent's investment and resulting winning probabilities. In step three, the computer randomly draws one of the tickets. The owner of the drawn ticket receives the prize and subjects are informed about their payoffs from the game. The stage game is repeated for 30 periods. All periods have the same timing and are payoff-relevant. The experimental tokens are converted into Euros at the exchange rate 270 tokens=1 EUR.

3.4.2. Experimental treatments

We employ a 2x2 factorial design. The treatment variables are the matching protocol and the saliency of best responses. In one dimension, we implement either a partner matching or a random stranger matching in sub-groups of six subjects. In another dimension, we vary the presence of the computational tool in the decision stage. In the CALCULATOR treatments, the computer displays several different investment proposals, given the subject's reported belief: 1) investment maximizing the expected payoff; 2) investment leading to the same expected payoff as the opponent; and 3) investment maximizing the winning probability (see Figure 3.1). Each proposal is placed in a separate box, including information on the winning probability as well as payoffs in the case of winning and losing. Subjects are also free not to choose any of the displayed proposals but rather insert any investment between zero and 100 (see most right box in Figure 3.1). In BASE treatments, subjects follow the same two-step decision process (state beliefs and then own investment), albeit without being exposed to any investment proposals.

[3] The applied function is: Bonus for belief $= \max\{0; 20 - 0.5 \cdot (\text{Belief} - \text{Actual investment})^2\}$.

[4] Procedure of incentivizing beliefs always raises the question about possible hedging strategies (see e.g., Blanco et al. 2010). Whereas we cannot completely rule out such behavior, we do not recognize any behavioral patterns indicating hedging. Moreover, monetary incentives for belief elicitation were relatively low in comparison to stakes in the main task. In our experiment, subjects earned on average 122.6 tokens per period, out of which only 6.3 tokens were bonus for belief. Therefore, the bonus amounted to only 5.2% of the total profit, which demonstrates that hedging strategies could not be very successful.

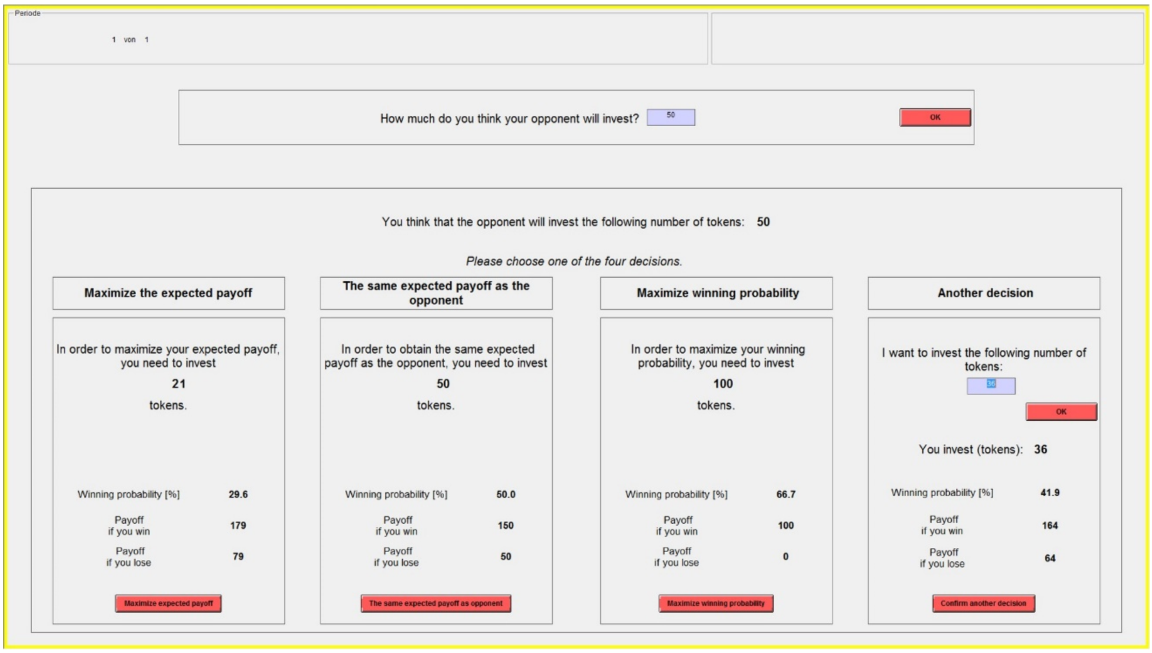


Figure 3.1: Decision screen in PARTNER_CALCULATOR treatment. Translation from German.

Choosing the first proposal – investments maximizing the expected payoff – corresponds to playing a best response to the state belief. Nonetheless, other proposals may also be appealing, particularly in repeated play with partners. Rockenbach and Waligora (see Chapter 2) show that belief-matching (proposal 2) is a common strategy under partner matching. It may be motivated by the unwillingness to give up against an aggressive opponent or by inequity aversion. Proposal 3 can be interpreted as an implementation of a deterrence or punishment strategy: very aggressive behavior to deter the opponent from the contest or punish the opponent for previous play. Moreover, such a richer set of alternatives also mitigates the experimenter demand effect (Zizzo 2010), as subjects are not exposed to a single proposal. Similar computational tools that provide information on best responses have been frequently used in experimental studies on behavior in Cournot oligopolies (Huck et al. 1999; Raab and Schipper 2009).[5]

Two treatment variables interacting with each other lead to the following four treatments, as summarized in Table 3.1.

[5] See Table 1 in Requate and Waichman (2011) for an overview.

Table 3.1: Experimental treatments.

		Matching protocol	
		Partner	**Stranger**
Computational tool	**No**	PARTNER_BASE (58/29)	STRANGER_BASE (60/10)
	Yes	PARTNER_CALCULATOR (58/29)	STRANGER_CALCULATOR (60/10)

Note: In parentheses: (number of subjects / numbers of independent observations).

3.4.3. Hypotheses

We design the experiment to test several hypotheses concerning the effects of the matching protocol and the presence of a computational tool. The null hypotheses describe the behavior of *homo oeconomicus* in games with common knowledge of rationality. Based upon previously described considerations, we also formulate behavioral alternative hypotheses.

To investigate the joint effect of the matching protocol, we compare behavior in the two BASE treatments. The game-theoretic prediction does not change under the assumption of complete information and common rationality. However, if we relax these assumptions repeated interactions with the same opponent might induce intertemporal dynamics and strategies different from best-responding, i.e., belief-matching or aggressive deterrence strategies. The partners can also tacitly collude. Although two players cannot share the prize among themselves in a single period, they can do so in repeated interactions by agreeing that alternately one of them does not participate in the lottery in every second period. Alternatively, players may agree to make the lowest positive investment (i.e., 1 token) in every period and thus they both always have a 50 percent winning chance.

Hypothesis 3.1 (effect of the matching protocol): PARTNER_ BASE vs STRANGER_BASE

H0: Behavior in PARTNER_BASE and STRANGER_BASE treatments is not systematically different.

HA: In STRANGER_BASE, best responses are more frequent than in PARTNER_BASE treatment.

The inference about the effect of the matching protocol from the comparison between the BASE treatments might be confounded with the effect of limited computational abilities. Subjects might strive for best-responding under the STRANGER condition, although they may be hampered by difficulties in computing it. Therefore, we also test the matching protocol effect in CALCULATOR treatments, in which we provide direct hints at best responses. Computational abilities no longer hinder best-responding and we can disentangle the two effects. Therefore, hypothesis 3.2 is very similar to hypothesis 3.1, although it tests the effect of the matching protocol under a high saliency of best responses.

Hypothesis 3.2 (effect of the matching protocol): PARTNER_CALCULATOR vs STRANGER_CALCULATOR

H0: Behavior in PARTNER_CALCULATOR and STRANGER_CALCULATOR treatments is not systematically different.

HA: In STRANGER_CALCULATOR, best responses are more frequent than in PARTNER_CALCULATOR treatment.

A comparison of the treatment effects that we investigate in hypotheses 3.1 and 3.2 provides additional, complementing evidence on the possible interplay between the matching protocol and limited computational abilities. Applying the difference-in-differences-approach, we test whether the effects of the matching protocol are different under low and high saliency of best responses.

Hypothesis 3.3 (Difference-in-differences)

H0: The matching protocol effect is the same under low (BASE) and high (CALCULATOR) saliency of best responses.

HA: The effect of the matching protocol is stronger in the CALCULATOR condition than in the BASE condition.

Finally, under the stranger matching protocol subjects do not have intertemporal incentives to deviate from best responses in the current period. Deterrence strategies, collusion, punitive behavior, etc. are not possible. Therefore, a comparison of STRANGER treatments allows us to measure the effect of limited computational abilities. *Homo* *oeconomicus* does not face any difficulties with identifying best responses. However, it is likely that subjects in the lab often cannot compute the optimal response by themselves. Hence, we expect that the computational tool will increase the frequency of best responses.

Hypothesis 3.4 (the effect of computational abilities): STRANGER_BASE vs STRANGER_CALCULATOR

H0: In STRANGER_CALCULATOR, best responses are as frequent as in STRANGER_BASE.

HA: In STRANGER_CALCULATOR, best responses are more frequent than in STRANGER_BASE.

3.4.4. Procedure

We conducted the experiment in April and May 2016 at the Cologne Laboratory for Economic Research (CLER), Germany. 236 subjects (undergraduate and graduate students with various majors; 58% female) were recruited with ORSEE (Greiner 2015) and always participated in only one session. The experiment was computerized using z-Tree (Fischbacher 2007). Experimental sessions lasted between 75 and 90 minutes and average earnings amounted to 20.4 EUR, including a 4 EUR show-up fee.[6]

3.5. Results

3.5.1. Overbidding and overspreading

In PARTNER_BASE treatment, as our control condition, we replicate results from previous experiments on Tullock contests (e.g., BASELINE treatment in Chapter 2). In PARTNER_BASE, subjects invest on average 34.7 tokens, which is more than the Nash equilibrium of 25. The difference is statistically significant (p=0.023).[7,8] The average magnitude of overbidding (38.9%) is comparable to previous studies. Similarly, we find *overspreading*, whereby subjects use the entire strategy space, although prominent

[6] After the main part of the experiment, subjects also participated in a short incentivized task eliciting their risk-preferences.

[7] Throughout the paper, we report p-values from non-parametric (two-sided) Fisher-Pitman permutation tests. Hereafter: FP test.

[8] In treatments with the partner matching protocol, a pair of players over 30 periods constitutes one independent observations, while in treatments with stranger matching it is a group of six players over 30 periods.

numbers (multiples of 5) are more frequently chosen than others (see Figure 3.6 in Appendix A). We do not observe any significant time trend, nor learning towards Nash equilibrium. Furthermore, we find no noteworthy differences with respect to myopia and the precision of reported beliefs. The empirical response function closely resembles that one reported in Chapter 2 (see Figure 3.8, panel A in Appendix A). We conclude:

Result 3.1: *PARTNER_BASE treatment replicates results reported in previous studies.*

3.5.2. The effect of the matching protocol

In order to test hypothesis 3.1, we compare behavior in the BASE treatments, in which subjects are not equipped with the computational tool. Figure 3.2 reveals that the bidding behavior is similar. Average investments amount to 34.7 and 38.0 tokens in PARTNER_BASE and STRANGER_BASE treatment, respectively. The difference is statistically insignificant (p=0.652, FP test). A parametric regression analysis comes to the same conclusion (see regression [I] in Table 3.7, Appendix A). This result is consistent with Sheremeta's (2013) meta-analysis. We also compare behavioral responses to reported beliefs. Figure 3.8 (panels A and B) in Appendix A reveals that the patterns are similar.

Table 3.2 summarizes strategies applied in BASE treatments. We observe that the structures are quite similar in several dimensions. In both treatments, only about 2.5 percent of investments correspond to best responses. Furthermore, the fractions of belief-matching and striving for maximal winning probabilities are not strongly different. On the other hand, we observe that collusion between contestants is more likely to emerge under partner matching, as we find more of such behavior (15.1% vs 0.3%).[9] When tested non-parametrically, the difference is insignificant (p=0.121, FP test), given that all collusive behavior is concentrated in a small number of player-pairs (5 out of 29). However, the parametric regression analysis detects, a significant effect of the matching protocol on the likelihood of collusion (see regression [III] in Table 3.4). We do not find any evidence of deterrence strategies, whereby high investments are not more frequent between partners than strangers. Accordingly, we can conclude:

[9] As described in Section 4.3, we classify the following cases as collusion: Investment=1 & Belief=0; Investment=0 & Belief=1; Investment=Belief=1.

***Result 3.2**: We fail to reject the null hypothesis 3.1. In STRANGER_BASE, best responses are not more frequent than in PARTNER_BASE.*

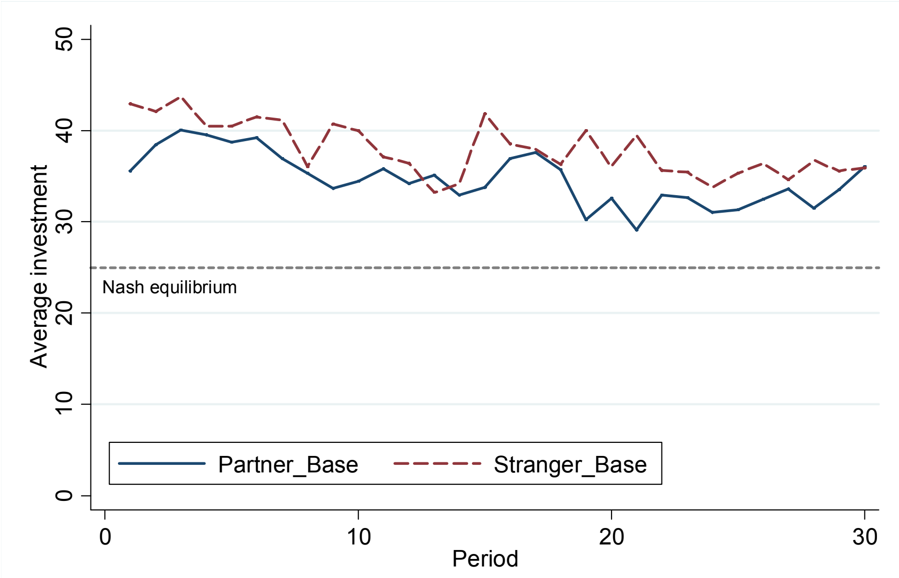


Figure 3.2: Average investments over time. BASE treatments.

We conduct a similar analysis on the effect of the matching protocol when providing subjects with direct hints at best responses and compare behavior in the CALCULATOR treatments. Average investments in PARTNER_CALCULATOR and STRANGER_CALCULATOR are 28.4 and 33.6, respectively. The difference is not significant (p=0.180, FP test). Figure 3.4 and Table 3.3 provide a more detailed picture of the treatment effect. Table 3 shows that the fraction of best responses is very similar in both treatments and amounts to about 20 percent (p=0.995, FP test). We find somehow more belief-matching in the PARTNER condition than in the STRANGER condition, although the difference is statistically insignificant (p=0.125, FP test). Furthermore, the difference in the frequency of the collusive behavior is not significant (p=0.533, FP test). In general, the overview of the treatments effects in the CALCULATOR treatments resembles the previous analysis for the BASE condition.

***Result 3.3**: We fail to reject the null hypothesis 3.2. In STRANGER_CALCULATOR, best responses are not more frequent than in PARTNER_CALCULATOR.*

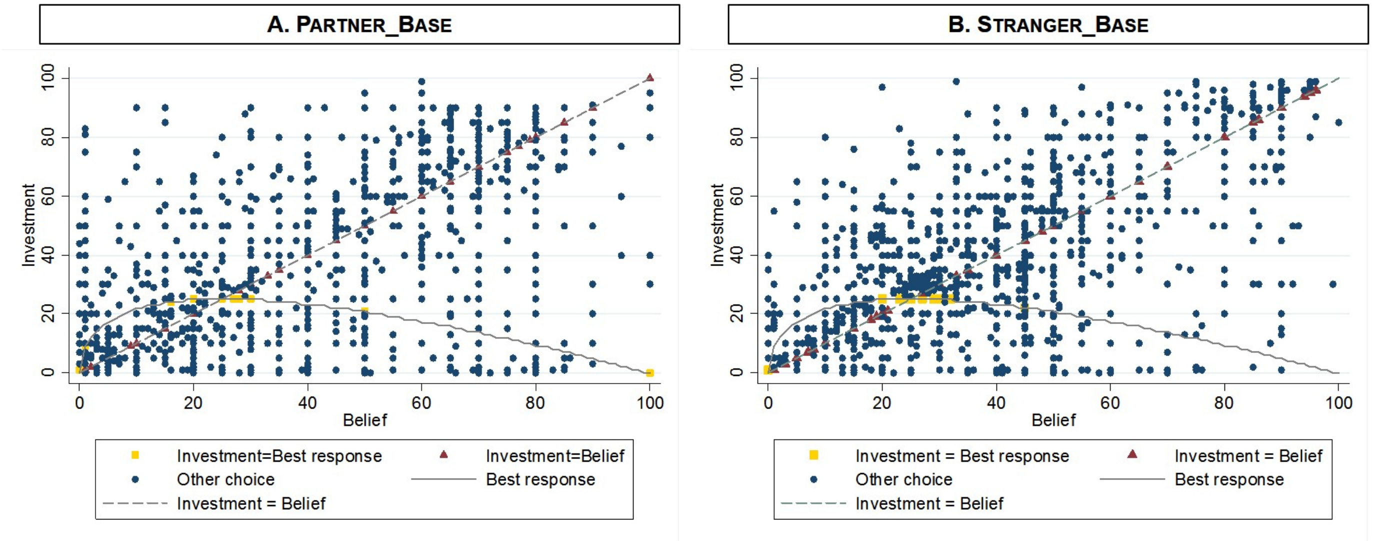


Figure 3.3: Beliefs and investments in BASE treatments.

Table 3.2: Structure of behavioral responses to beliefs in BASE treatments.

A. PARTNER: Decision	**Fraction**
Best response *	2.8%
Matching **	12.2%
Collusion	15.1%
Max winning	0.7%
Other decision	69.2%

* Except for Belief=0 & Investment=1.

** Out of Nash equilibrium and no collusion.

B. STRANGER: Decision	**Fraction**
Best response *	2.4%
Matching **	10.7%
Collusion	0.3%
Max winning	0.3%
Other decision	86.2%

* Except for Belief=0 & Investment=1.

** Out of Nash equilibrium and no collusion.

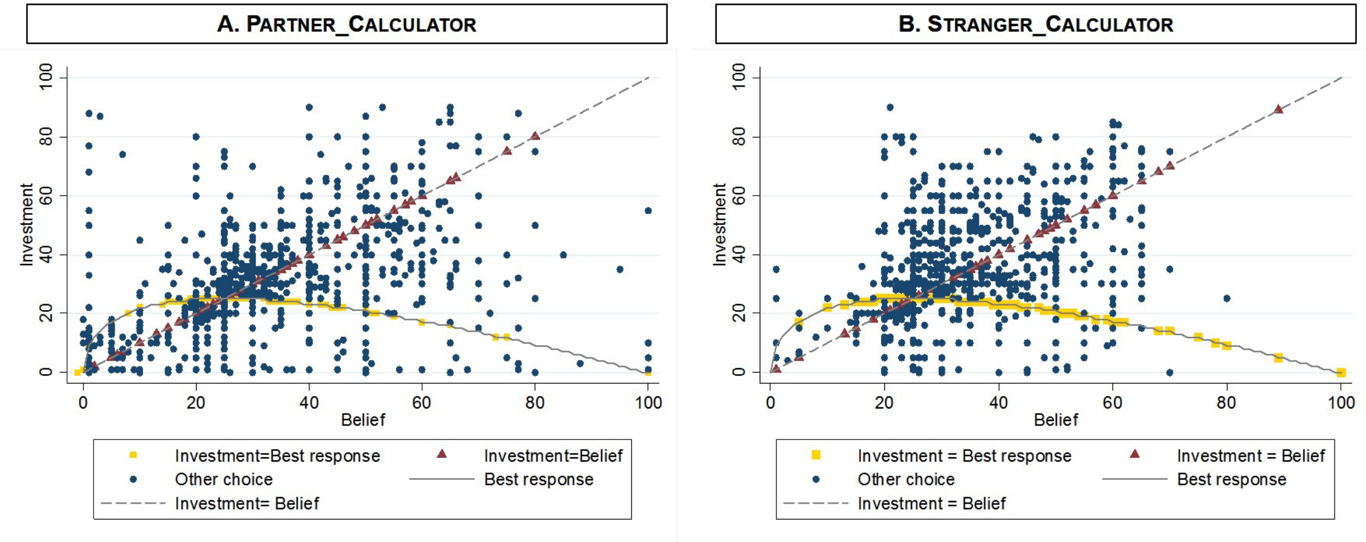


Figure 3.4: Beliefs and investments in CALCULATOR treatments.

Table 3.3: Structure of behavioral responses to beliefs in CALCULATOR treatments.

A. PARTNER: Decision	**Fraction**
Best response *	20.9%
Matching **	15.5%
Collusion	7.1%
Max winning	0.7%
Other decision	55.8%

* Except for Belief=0 & Investment=1.

** Out of Nash equilibrium and no collusion.

B. STRANGER: Decision	**Fraction**
Best response *	22.1%
Matching **	9.6%
Collusion	0.2%
Max winning	1.7%
Other decision	66.4%

* Except for Belief=0 & Investment=1.

** Out of Nash equilibrium and no collusion.

3.5.3. Difference-in-differences

The results 3.2 and 3.3 in the previous section suggest that the matching protocol effects are qualitatively similar across conditions of a low or high saliency of best responses. We complement this conclusion with a quantitative analysis. The parametric probit regressions of the likelihood to follow different strategies confirm that the effect of the matching protocol on all of the considered strategies (best-responding, belief-matching, collusion) is comparable under the BASE and CALCULATOR conditions. In all three regressions presented in Table 3.4, the interaction dummy is far from being significant. Similarly, Table 3.7 in Appendix A (regression [III]) also reveals that when measured with the average investments, there is no significant difference-in-differences. This means that the effects of the matching protocol are not only quantitatively similar across different informational conditions (BASE vs CALCULATOR), but also the magnitudes of these effects are similar. This speaks against any interplay between the effects of matching protocol and the saliency of best responses.

Table 3.4: Treatment effects on the frequency of following different strategies. Panel probit regression.

Dependent variable:	**1[Investment = best response] [I]**	**1[Investment = belief] [II]**	**1[Investment = collusive behavior] [III]**
1[Stranger protocol]	-0.274	-0.101	-4.378**
	(0.330)	(0.397)	(2.125)
1[Calculator]	1.423***	0.192	-0.771
	(0.211)	(0.173)	(1.760)
1[Stranger] x 1[Calculator]	0.221	-0.283	0.023
	(0.383)	(0.221)	(2.861)
Constant	-2.393***	-1.381***	-4.256***
	(0.166)	(0.322)	(1.286)
Number of observations	7080	7080	7080
Number of clusters	78	78	78
Wald Chi-squared	105.17***	8.75**	79.09***

Notes: Panel probit regression with random subject effects. In parentheses, robust standard errors clustered at independent observations. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

***Result 3.4**: We fail to reject the null hypothesis 3.3. The matching protocol induces similar effects under a low (BASE) and high (CALCULATOR) saliency of best responses.*

3.5.4. The effect of limited computational abilities

We compare behavior in two STRANGER treatments to test whether providing direct hints at best responses helps to follow this strategy. Panels B of Tables 3.2 and 3.3 demonstrate that the fraction of best responses indeed increases from 2.4% to 22.1%. The difference is highly significant ($p<0.001$, FP test). Although the effect is large – best responses become nine times more likely – the magnitude is lower than one could expect. In STRANGER_CALCULATOR, despite being provided with information concerning the best response, in more than three-quarters of cases subjects decided not to best-respond. Therefore, computational limitations are one important obstacles in playing best responses, albeit probably not the only one.

***Result 3.5**: We reject the null hypothesis 3.4. In STRANGER_CALCULATOR, best responses are significantly more frequent than in STRANGER_BASE, whereby their fraction rises from 2.4% to 22.1%.*

Additionally, the probit regression [I] in Table 3.4 confirms that the computational tool significantly increases best-responding. The effect is not only present in the STRANGER condition, but also among partners. The scales of the effect under both conditions are strikingly similar (see panels A of Tables 3.2 and 3.3).

We also investigate whether a lower-than-expected fraction of best responses in CALCULATOR treatments is due to the fact that subjects need time to recognize which strategy to follow. Figure 3.9 in Appendix A depicts the structure of decisions in treatments with the computational tool over time. It becomes clear that the share of best responses remains at about 20% throughout the course of the experiment. We cannot identify any time trends.

Finally, we analyze how the increased fractions of best responses impact on the response function to beliefs in CALCULATOR treatments. Panels C and D of Figure 3.8 in Appendix A shows that the empirical response function remains linear, albeit with a slightly lower slope than 45 degrees. The effect of more common best responses is too weak to induce a response function resembling the theoretic prediction.

Table 3.5: Adjustment models: regression analysis of period-by-period adjustments.

A. Base treatments Dependent variable Inv(t) - Inv(t-1)	**Partner _Base [I]**	**Partner _Base [II]**	**Stranger _Base [I]**	**Stranger _Base [II]**
Best response*(t)* - Inv*(t-1)*	0.424***	0.181***	0.145*	0.149**
	(0.109)	(0.049)	(0.082)	(0.060)
Matching*(t)* - Inv*(t-1)*	0.434***	0.389***	0.326***	0.327***
	(0.058)	(0.083)	(0.057)	(0.062)
Max_win_prob*(t)* - Inv*(t-1)*	-0.263***		0.005	
	(0.084)		(0.050)	
Constant	23.874***	2.874***	1.830	2.214**
	(7.074)	(1.004)	(3.762)	(1.029)
Number of observations	1682	1682	1740	1740
Number of clusters	29	29	10	10
Wald Chi-squared	128.13***	46.84***	117.43***	98.29***

B. Calculator treatments Dependent variable Inv(t) - Inv(t-1)	**Partner_ Calculator [I]**	**Partner_ Calculator [II]**	**Stranger _Calculator [I]**	**Stranger _Calculator [II]**
Best response*(t)* - Inv*(t-1)*	0.332***	0.323***	-0.051	0.222***
	(0.047)	(0.046)	(0.207)	(0.037)
Matching*(t)* - Inv*(t-1)*	0.403***	0.402***	0.243***	0.287***
	(0.084)	(0.080)	(0.068)	(0.074)
Max_win_prob*(t)* - Inv*(t-1)*	-0.010		0.321	
	(0.034)		(0.240)	
Constant	2.480	1.691**	-21.871	2.302***
	(2.363)	(0.736)	(17.929)	(0.580)
Number of observations	1682	1682	1740	1740
Number of clusters	29	29	10	10
Wald Chi-squared	275.19***	171.93***	45.81***	47.56***

Notes: Panel regressions with random subject effects. In parentheses, robust standard errors clustered at independent observations. Due to a relatively small number of clusters in STRANGER treatments, as a robustness check for those treatments we also estimate regressions with cluster-bootstrapped standard errors (see Cameron et al. 2008). The inference does not change qualitatively. In particular, insignificant regressors are also insignificant with the bootstrapping method. Significance levels: *** p<0.01; ** p<0.05; * p<0.1.

3.5.5. What if not best responses?

We find that in CALCULATOR treatments best responses are not played very often. Therefore, the question arises: *what do subjects play instead?* As is typical of experiments on rent-seeking behavior, we deal with a large behavioral variation. Nevertheless, we attempt to organize the data to identify average tendencies. We use the adjustment models that follow the approach by Huck et al. (1999) as well as Fallucchi et al. (2013). We investigate which of the three strategies proposed in CALCULATOR treatment most strongly drives period-by-period adjustments. The results are reported in Table 3.5. In regression specifications [I] we use all three displayed strategies. It becomes apparent that maximizing the expected winning probability (i.e., investing the entire endowment) is either not significant or has a negative effect, which is difficult to interpret. Hence, we also estimate alternative specifications [II], using only two other behavioral rules as predictors (best responses and matching beliefs) and interpret these results. In all treatments, both behavioral rules have a significant impact on the observed investments. Moreover, in all treatments the absolute magnitude of matching beliefs is larger than the absolute magnitude of playing best response. However, the parametric comparison of regression parameters reveals that the differences are statistically insignificant.[27] Therefore, we can conclude that both playing best responses and matching beliefs drive contest behavior in our experiment significantly and to a similar extend. On average subjects tend to choose investments in between best-reply and matching beliefs.

3.6. Discussion and conclusion

We conduct a new series of experiments on rent-seeking in symmetric two-player contests to test the effect of the matching protocol on investment behavior in repeated interactions. Much previous work in behavioral and experimental economics leads us to expect a behavioral effect of the matching protocol. Surprisingly, our results provide clear evidence that the matching protocol has no effect on average bidding behavior, nor the willingness to play best responses. Accordingly, we can reject strategic motives induced by repeated interactions with the same opponent as an explanation for subjects

[27] Only in PARTNER_BASE treatment is the difference significant at 10 percent.

not best-responding in experimental contests. Manipulation of the matching protocol triggers only one noteworthy behavioral difference, whereby we observe somehow more collusive behavior among partners rather than strangers. However, the scale of collusion remains relatively small and it does not induce differences detectable at the level of average investments or the empirical response function.

Our results confirm the conclusion following from Sheremeta's (2013) meta-analysis and complement mixed previous evidence on this question. The study provides guidelines for both researchers investigating contest behavior in the lab as well as contest designers. On the one hand, we show that the effect of the matching protocol in rent-seeking experiments is not too large and often negligible. On the other hand, we demonstrate that the danger of collusive behavior between contestants facing each other repeatedly is only moderate. We observe little collusion between two players, which leads us to expect that such behavior would be even more unlikely if we increased the number of contestants (Huck et al. 2004). Moreover, the result of no strategic motives behind non-best-responding is robust against our experimental variation of a low or high saliency of best responses.

We report evidence that limited computational abilities prevent subjects from best-responding. We find that making best responses extremely salient increases subjects' willingness to play them. However, the effect is smaller than one could expect (the frequency of best responses increases to 20 percent). This result holds to the almost exact extent and magnitude under both tested matching protocols. Moreover, our experimental design in which we provide subjects with three possible investments and not only with a hint at best responses allows us to argue that the effect is not driven by the experimenter demand effect. When exposed to three proposals, we would expect subjects to feel obliged to follow them to a similar extent. However, we find that the presence of the tool does not raise the likelihood of matching beliefs or maximizing the winning probability. This suggests that the increased willingness to follow best responses in CALCULATOR treatments is not due to the experimenter demand effect; instead, bounded rationality and severe difficulties in identifying best responses seem to be the most likely reasons. In this sense, our results complement conclusions from Masiliunas et al.'s (2014) work.

3.7. Appendix A. Additional tables and figures

Table 3.6: Summary statistics of investment behavior across treatments.

Treatment	Average investment	Standard deviation	H0: Investment = Nash equilibrium
PARTNER_BASE	34.724	28.885	p=0.023
STRANGER_BASE	38.014	24.523	p=0.008
PARTNER_CALCULATOR	28.421	18.347	p=0.127
STRANGER_CALCULATOR	33.613	18.467	p=0.002

Note: In the last column, we test the null hypothesis of the investments being at the Nash equilibrium level. We report p-values from a two-sided Fisher-Pitman permutation test conducted at the level of independent observations.

Table 3.7: Treatment effects on bidding behavior. Panel regression analysis.

Dependent variable: Investment	BASE treatments [I]	CALCULATOR treatments [II]	all treatments [III]
1[Stranger protocol]	3.291	5.192*	3.291
	(6.032)	(2.836)	(5.993)
1[Calculator]			-6.302
			(4.532)
1[Stranger protocol x 1[Calculator]			1.901
			(6.623)
Constant	34.724***	28.421***	34.724***
	(4.018)	(2.159)	(3.992)
Number of observations	3540	3540	7080
Number of clusters	39	39	78
Wald Chi-squared	0.30	3.35*	5.79

Notes: Panel regression with random subject effects. In parentheses, robust standard errors clustered at independent observations. Significance levels: *** p<0.01; ** p<0.05; * p<0.1.

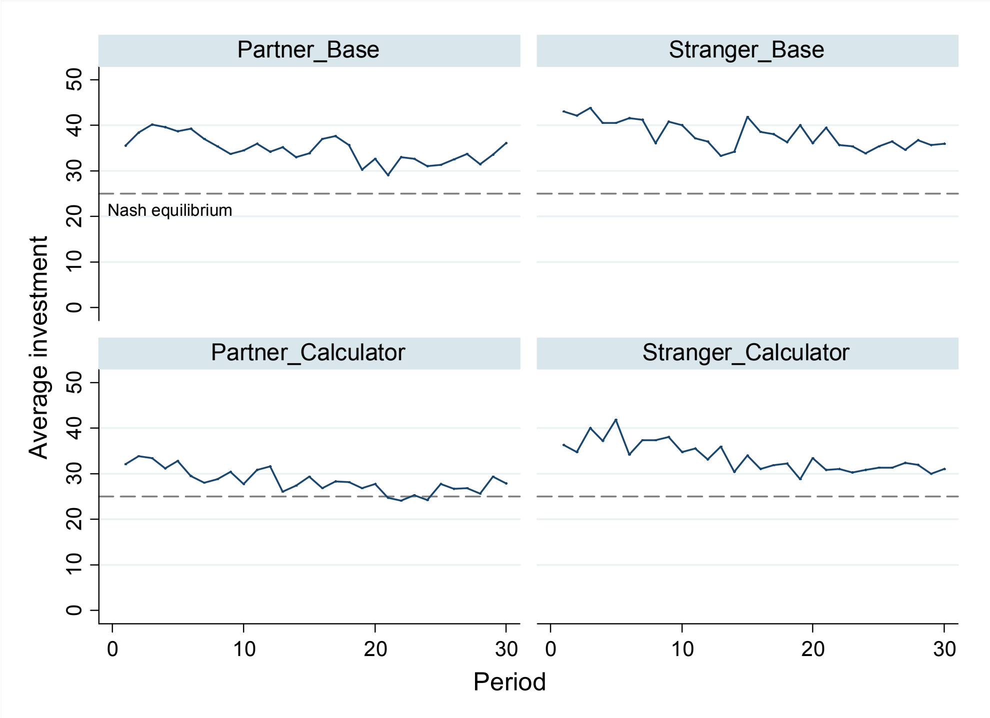


Figure 3.5: Average investments over time, plotted by treatment.

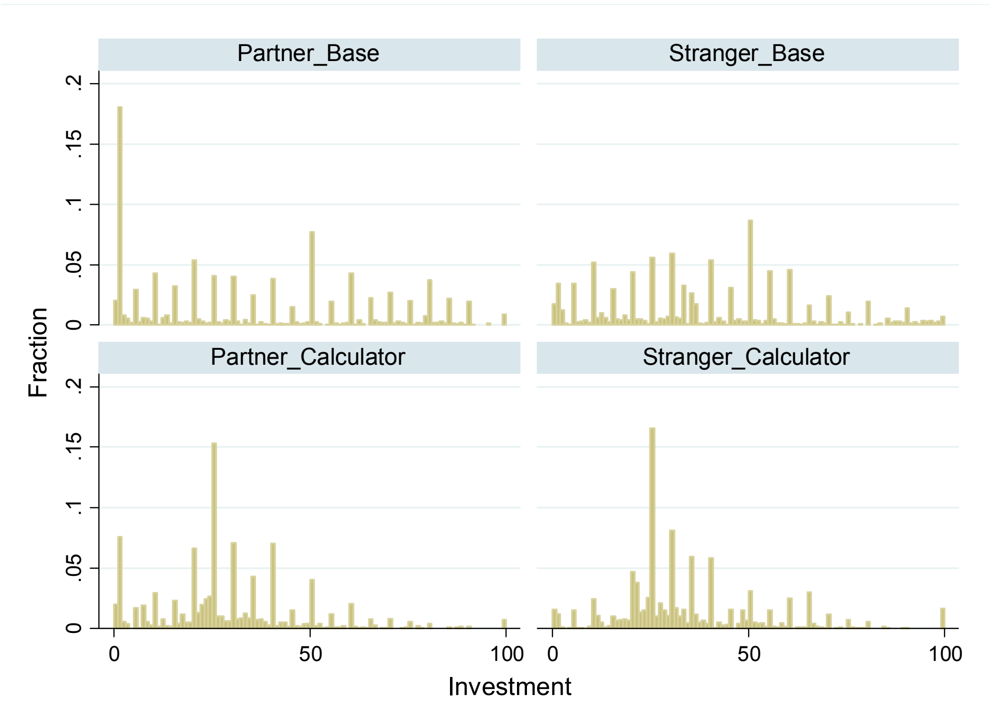


Figure 3.6: Distribution of investments across treatments.

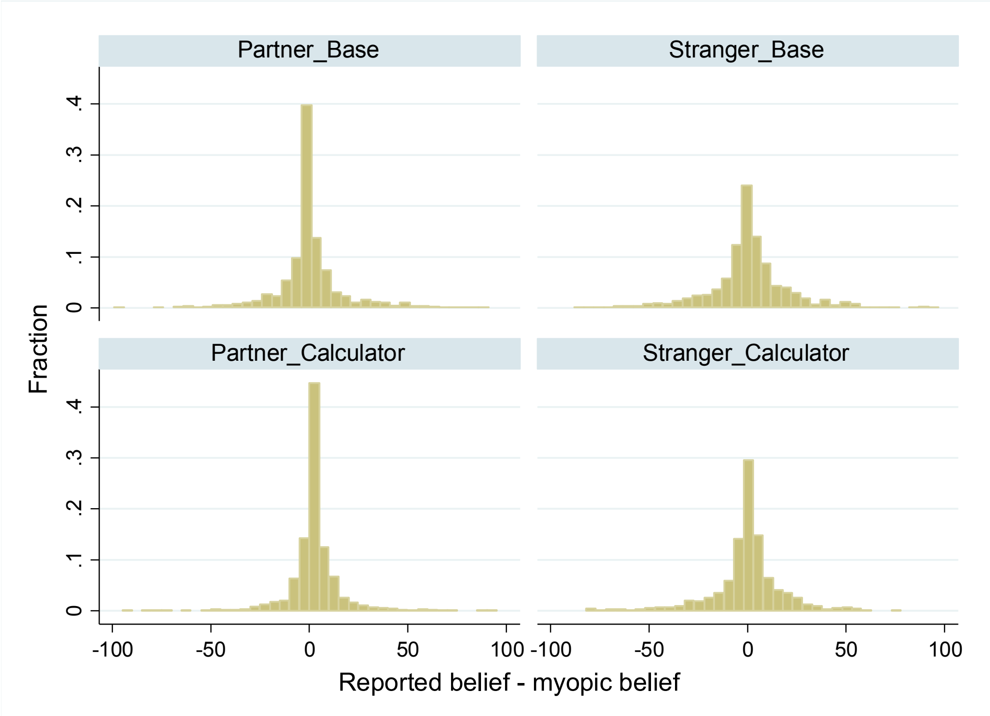


Figure 3.7: Myopic beliefs across treatments.

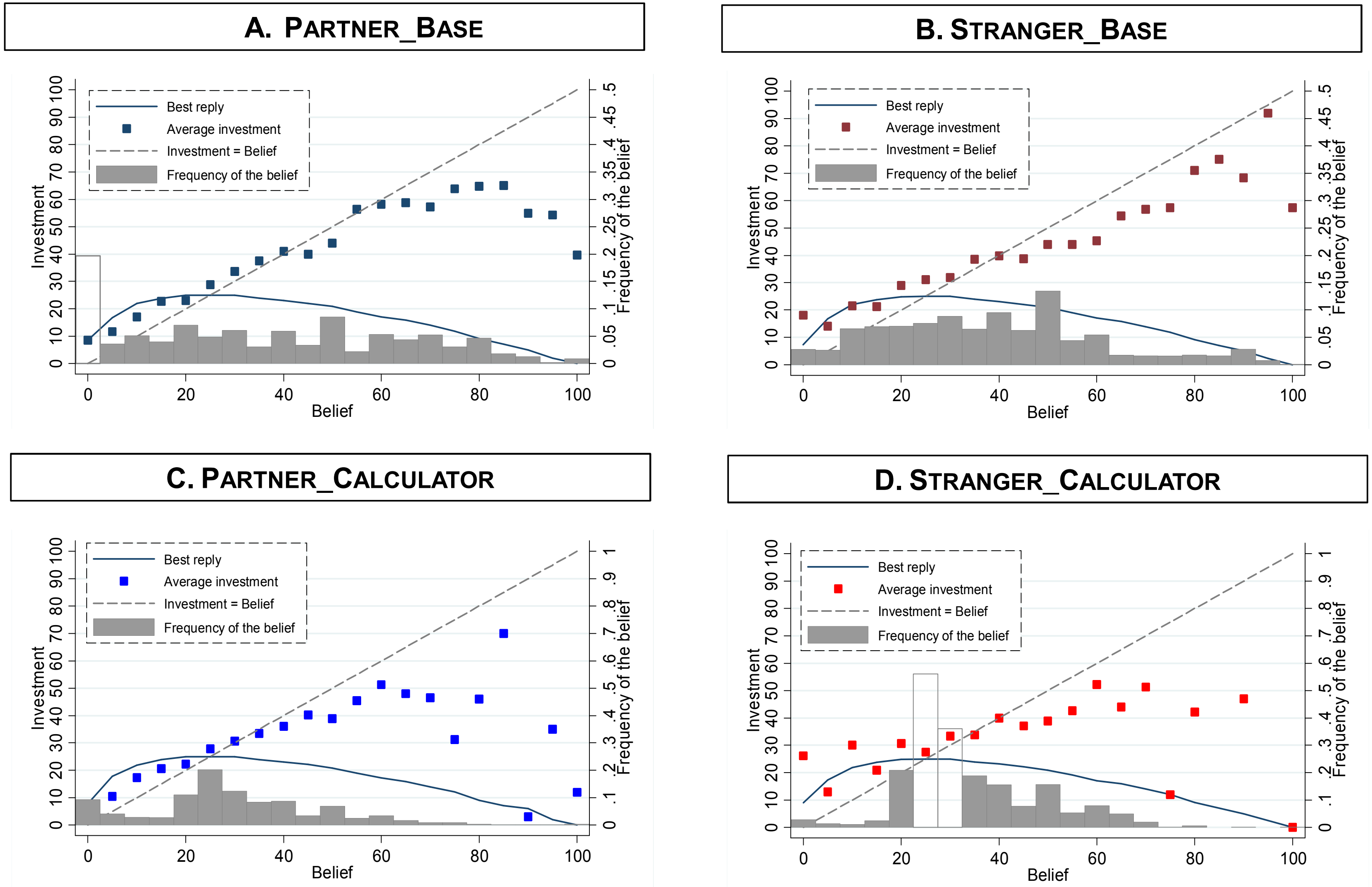


Figure 3.8: Average responses to beliefs across treatments.

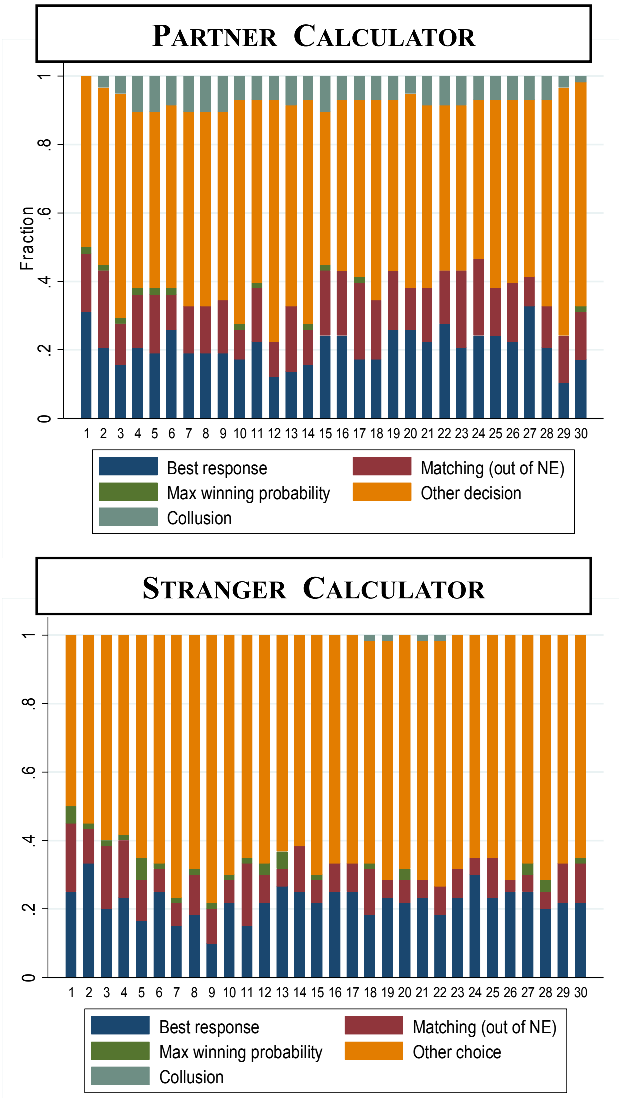


Figure 3.9: Structure of decisions in CALCULATOR treatments over time.

3.8. Appendix B. Decision screens and experimental instructions: PARTNER_CALCULATOR treatment

Note: See Figure 3.1 in the main text for the decision screen.

Figure 3.10: Feedback screen in PARTNER_CALCULATOR treatment. Translation from German.

Figure 3.11: Contest result screen in PARTNER_CALCULATOR treatment. Translation from German.

Instructions in PARTNER_CALCULATOR treatment (translation from German)

General information

Welcome to our experiment! It is important that you carefully read and understand the following instructions. If you have a question, please raise your hand. We will then come to you and answer it. Communication with other participants before and during the experiment is prohibited. If you violate this rule, you will have to leave the experiment and will not receive any payment.

You can earn money in this experiment. You will receive 4 EUR for your participation. You can earn additional money during the experiment. The amount of money that you earn depends on your decisions and those of other participants in the experiment. Your earnings are denoted in tokens. These will be converted in EUR and paid out in cash at the end of the experiment. The exchange rate is:

270 tokens = 1 EUR.

The experiment comprises several rounds. Tokens that you earn in each round are added to your tokens account. Your payoff is the sum of the tokens that you have earned in all rounds of the experiment. No participant will receive information about your payoff in the experiment or your identity.

Course of the experiment

In today's experiment you will interact with another participant. You will shortly be randomly matched with an opponent. In every pair there will be a player A (marked blue) and a player B (marked red). The roles are also randomly assigned. The pairings as well as the roles remain unchanged for the entire experiment. Information about your role will be displayed on the screen before the first round begins.

The experiment comprises **30 rounds**. All rounds proceed in the same way. In each round, you and your opponent will compete in a lottery for a prize.

At the beginning of each round, you receive from us **100 tokens**. Then, you can decide how many tokens you want to spend on lottery tickets. For one token, you can purchase one ticket (1 token = 1 ticket). You can purchase as many tickets as you want, but you are not allowed to exceed your budget. Tokens that you do not spend on lottery tickets are added to your account.

The prize that you can win in the lottery in each round is **100 tokens**.

Your chance of winning only depends on how many tickets you have bought and how many your opponent has bought. The more tickets you have bought, the more likely it is that you win. The more tickets your opponent has bought, the less likely it is that you win. The probabilities with which you win the prize are equal to the number of your tickets divided by the number of all tickets bought. This means that it is computed according to the following rule:

$$\text{Your probability of winning} = \frac{\text{Number of your tickets}}{\text{Number of your tickets} + \text{Number of opponent's tickets}}$$

Your earnings in a single round are as follows:

Your earnings if you win = 100 – your investment in the tickets + 100

Your earnings if you lose = 100 – your investment in the tickets

If only one player has bought tickets, she/he wins with certainty. If neither of the players has bought any tickets, the lottery does not take place and nobody wins the prize.

Each round comprises three steps:

Step 1 (Decision)

In the first step, you decide how many tickets you would like to purchase. At the same time, your opponent makes the same decision.

Figure 1 shows the computer screen in step 1. The decision process takes place in two sub-steps.

Sub-step 1 a (Your prediction about the opponent's investment)

First, you are asked what investment you expect from your opponent (see field (a) in Figure 1). Please insert your prediction and click "OK".

For your prediction, you can earn a bonus of maximal **20 tokens**. The amount of the bonus depends on how good your prediction was. The smaller the mistake in your prediction, the higher your bonus.

The mistake is the difference between your prediction and actual opponent's investment:

$$\text{Mistake} = |\text{Your prediction} - \text{actual opponent's investment}|$$

The following table shows how large the bonus is:

Mistake	Bonus
0	20
1	19,5
2	18
3	15,5
4	12
5	7,5
6	2
larger than 6	0

There is no negative bonus, whereby you cannot lose any tokens for your prediction.

Sub-step 1b (Your investment)

After you confirmed your prediction, field (b) appears on the screen (see Figure 1), where you are expected to insert your decision on your investment. The example in Figure 1 shows the screen if a player predicted that his/her competitor would invest 50 tokens.

Based upon your prediction is sub-step 1a, the computer calculates several proposals of how much you could invest.

In every box 1-3 one proposal appears. The winning probability and your payoff if you win or lose after having followed the proposals are also displayed.

Box 1 shows how much you need to invest in order to maximize your expected payoff, given your prediction about the opponent's investment.

The expected payoff is computed in the following way:

$$\text{Expected payoff} = \text{Probability that you win} \times \text{Payoff if you win} + \text{Probability that you lose} \times \text{Payoff if you lose}$$

Box 2 shows how much you need to invest to obtain the same expected payoff as your opponent (given your prediction about the opponent's investment).

Box 3 shows how much you need to invest to maximize your winning probability.

In order to follow one of the proposals, simply click the red button at the bottom of the respective box.

You are not obligated to follow any of the proposals; you may also invest any other number of tokens. You can do this using box 4. Please insert there your investment and confirm with the OK button. The corresponding winning probability and payoffs will be computed and appear on the screen. You can repeat this as many times as you want. You need to confirm your final decision with the "Confirm another decision" button.

The time for your decision in every round is limited. In the first ten rounds, you have 90 seconds time, in the next 20 rounds, 60 seconds. If you do not insert any prediction and decision within the given time, your investment in the round will be zero and you will receive no bonus.

Step 2 (Feedback)

In the second step, you receive feedback on the opponent's decision; in other words, how many tickets she/he has bought. The probabilities of winning are also computed and displayed. All tickets bought are numerated. You receive information on which numbers correspond to your tickets and which to those of your opponent. In order to make it clearer, this is also displayed graphically.

Each ticket is equally likely to be drawn.

You also receive feedback concerning how good your prediction about your opponent's investment was and how large your bonus is.

Step 3 (Lottery drawing)

In the last step, the winning ticket is drawn. The computer draws one of bought tickets. The number of the winning ticket and the winner are displayed on the screen, as are your earnings in the current round.

[Subjects also received a screen shot of the decision stage mask, see Figure 3.1. The only difference is that we marked decision fields with red frames, as described in the instructions above.]

Chapter 4: Pushing the Bad Away: Reverse Tullock Contests

Joint work with Bettina Rockenbach

4.1. Introduction

Situations of contest are ubiquitous. Under a number of circumstances, people compete against each other to obtain some profits or rents. As a result, a stream of literature on rent-seeking behavior has arisen (see e.g., Congleton et al. 2008a). Since field data on rent-seeking is hardly available, most economic research relies on experimental data (see e.g., Dechenaux et al. 2015). However, people not only compete against each other to achieve gains. We can think of numerous examples in which people exert efforts to avoid something bad coming or to avoid a loss. For instance, imagine that in the face of decreasing numbers of children in society at least one of the primary schools in a city needs to be closed. To avoid long travel times, different neighborhoods will start lobbying against closing their school. Similarly, imagine that two potential locations for a waste disposal have been identified and the government needs to decide which one to choose. Again, it is very likely that local communities will be ready and determined to spend resources to avoid their community being chosen. Even though rent-seeking contests for avoiding negative rents are common in practice, such settings have not been studied in the literature to date. We strive to close this gap by investigating whether contests to avoid the bad induce different behavior than 'conventional' contests for achieving gains. In particular, we address the question whether contests for avoiding losses are fiercer than those for achieving gains.

In our theoretical analysis, we introduce negative prizes into the framework of Tullock (1980) contests (henceforth called ‘reverse Tullock contests’) and show that such a variation does not alter the game-theoretic prediction under standard preferences. Nonetheless, prospect theory (Kahneman and Tversky 1979) predicts that behavior in reverse contests should be more aggressive because losses induce larger changes in utilities than corresponding gains. This suggests that contests for pushing the bad away lead to an even larger dissipation of resources than settings traditionally considered in the empirical and experimental research.

Our data show that reverse Tullock contests generate on average 15 percent higher investments than conventional contests, although this effect is statistically insignificant. Thus, it seems that the insights on behavior in Tullock contests for achieving gains may serve as appropriate predictors for contests to avoid losses.

4.2. The theoretical framework of reverse Tullock contests

We consider Tullock (1980) contests between two symmetric players (called A and B). In the *conventional contests*, players with the initial wealth *E* compete for a single non-dividable prize *V*. Both players spend resources that determine their winning probabilities. If player A invests x_A and player B invests x_B, the winning probabilities are as follows:

$$p_A = \frac{x_A}{x_A + x_B}\,; \qquad p_B = 1 - p_A \tag{4.1}$$

Therefore, th expected payoff for player $i \in \{A, B\}$ is:

$$E[\pi_i] = E - x_i + p_i V \tag{4.2}$$

In the *reverse contest*, contestants enjoy higher initial wealth (E+V). However, the prize is negative ($-V$). Investments in the contest increase the probability of avoiding the negative prize (i.e., not being drawn as a recipient of the prize). Thus, player i's probability of receiving the prize is equal to $(1 - p_i)$. Hence, the expected payoff for player *i* is:

$$E[\pi_i] = E + V - x_i - (1 - p_i)V \tag{4.3}$$

This can be re-written as:

$$E[\pi_i] = E - x_i + p_i V \qquad (4.4)$$

Equations 4.2 and 4.4 show that - in expected terms - the contests are equivalent (see also Figure 4.1). Neither the best response function nor the equilibrium prediction change. In both conventional and reverse Tullock contests, the Nash equilibrium under standard preferences prescribes individual investments equal to $x_A^{NE} = x_B^{NE} = \frac{V}{4}$ (Szidarovsky and Okuguchi 1997).

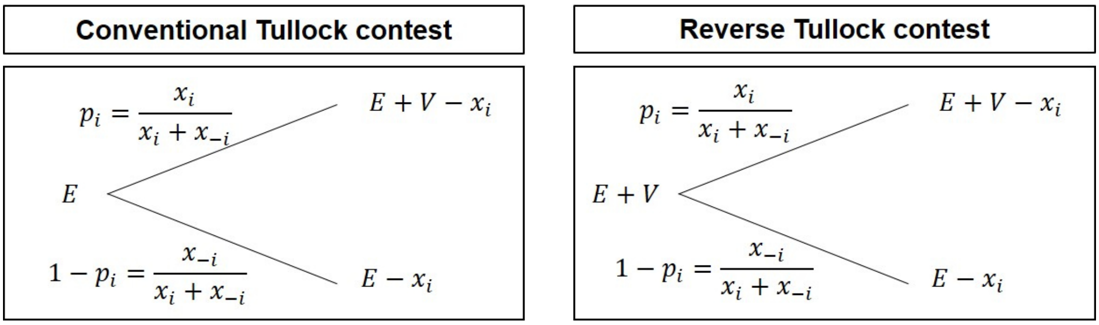


Figure 4.1: The equivalence of the contests.

Although the two considered contests are equivalent from the theoretical perspective and characterized by the same equilibrium prediction, there are reasons to expect different behavior across the two settings. Prospect theory (Kahneman and Tversky 1979, 1992) claims that people care more about losses than about gains; they are *loss-averse*. If the perceived value of a loss is larger than the perceived value of a monetarily equivalent gain, players competing to avoid a loss should be willing to invest more than contestants competing for a gain. This is because the equilibrium analysis predicts that a higher (perceived) stake of the contest drives more aggressive bidding behavior. Thus, loss aversion predicts that contests for avoiding losses are fiercer than those for achieving gains.[1]

[1] Loss aversion has been studied experimentally in a number of domains. Among others, the impact of loss aversion has been tested in a competitive environment other than Tullock contests. For instance, Delgado et al. (2008) show that framing an auction such that the losses become more prominent increases bidding prices.

4.3. Related literature

Even though rent-seeking contests for avoiding losses are common in practice, such settings have not been studied in the experimental literature to date.[2] Exploiting the difference between positive and negative domains has been particularly prominent in experimental literature on public goods provision. Andreoni (1995) shows that people are more willing to cooperate if the provision of a public good is framed positively. Similarly, Sonnemans et al. (1998) investigate whether behavior is different if cooperation leads to the provision of a public good versus the prevention of a public bad, finding that cooperative behavior is more common in a positive domain (public good provision).

Our project is also related to a recent study by Hong et al. (2015), whose field experiment investigates the effects of framing monetary incentives as losses or gains on workers' productivity in a team contest. The authors show that although the framing effect measured with the average productivity is statistically insignificant teams competing to avoid a loss are significantly more likely to win the contest. There are several major differences between Hong et al.'s study and ours. Unlike the authors, who test team behavior in tournament-styled contests, we investigate individual investments in Tullock contests. Moreover, in our study the prize is symmetrically framed as either achieving a gain or avoiding a loss for both contestants, whereas in Hong et al.'s study the framing was asymmetric (one team competed for a reward, another for avoiding a punishment).

4.4. The experiment

4.4.1. The experimental design

In the experiment, the contest between two players is framed as a lottery with a positive or negative prize V. In the conventional contest, both players are endowed with E=20 tokens and compete for a positive prize V=20 tokens. In the reverse contest players with the initial wealth of $E+V$=40 tokens compete against each other to avoid being awarded a negative prize ($V= -20$ tokens). Players buy lottery tickets for one token each.

[2] During the 'Contest: Theory and Evidence' conference in May 2016 in Norwich we became aware of a similar research project in progress: "Property Rights and Loss Aversion in Contests" by Chowdhury and Ramalingam.

The contest is repeated for 20 periods in partner matching. Each period proceeds in the same way and is payoff-relevant. First, subjects decide how many tokens from their endowment they want to spend on lottery tickets.[3] At the same time, they report which investment they expect from the opponent. The guessing is incentivized, whereby subjects are paid according to a quadratic loss function between four and zero tokens.[4] On the decision stage, subjects are also provided with a what-if calculator, which computes winning probabilities and expected payoffs, given their belief about the opponent's investment (see Figure 4.6 in Appendix B). In the second step, subjects receive feedback on the opponent's decision, the resulting winning probabilities and the amount of bonus for the reported belief. In the third step, the computer draws one of the bought tickets and the winner achieves the gain (or avoids the loss). Both contestants receive information on their earnings in the current period. If both players do not buy any lottery tickets, the lottery is not conducted and nobody achieves a gain (or both players experience a loss).

4.4.2. Treatments and hypothesis

We study two different treatments: Treatment T0 is a conventional Tullock contest and treatment T1 is a reverse Tullock contest, i.e., the winner of the lottery avoids the negative prize.[5] The treatments' characteristics are summarized in Table 4.1.

[3] Although endowments are different across treatments, we keep the strategy space unchanged. In both treatments, subjects are allowed to invest at most 20 tokens. This also prevents making losses in T1.

[4] The applied function is: $\max\{0;\, 4 - 0.4(Belief - actual\ investment)^2\}$. See Selten (1998) and Palfrey and Wang (2009) for the advantages of the quadratic rules against the linear ones. We are aware of the current discussion in the literature on whether and how to incentivize beliefs (e.g., Schlag et al. 2015).

[5] Note that one could implement the "avoiding the bad" condition in at least one alternative way. Rather than lobbying for oneself to avoid the bad, contestants could lobby for the opponent to receive the bad. In the experimental implementation, players do not buy lottery tickets for themselves, but rather for the opponent to increase the opponent's chance of being drawn and hence receiving the negative prize. However, such an implementation has the drawback that in comparison to T0 it not only differs in the rent dimension. Additionally, in such an implementation, contestants do not want to win the contest, but rather they want the opponent to win. It has been shown that in Tullock contests, players experience some non-monetary utility from the mere act of winning (Sheremeta 2010, 2013). This *joy of winning* is present in T0 and T1 alike. However, if contestants want the other player to win, they potentially lack the *joy of winning* and we may observe two counteracting effects as compared to T0: *loss aversion* drives higher investments, whereas reduced *joy of winning* lowers investments. For these reasons, we decided to predominantly focus on the implementation of the Tullock contest for avoiding the bad as in treatment T1. Nonetheless, we conducted the alternative implementation of buying lottery tickets for the opponent as a treatment T2. For details, see Appendix A.

Table 4.1: Characteristics of the experimental treatments.

Treatment	Endowment	Prize	Contest winner…
T0	20	20	receives the positive prize
T1	40	-20	avoids the negative prize

As outlined in Section 4.2, we expect subjects to invest more aggressively in T1 than in T0.

Hypothesis: *Reverse Tullock contests in treatment T1 generate higher investments than conventional Tullock contests in T0.*

4.4.3. Sample Size

We use power calculations to determine the appropriate sample size for our study. Convention prescribes that an effect should be detectable at the 5 percent significance level with the power of 80 percent (i.e., 8 out of 10 times). To calculate the appropriate sample size we have to quantify the expected magnitude of the treatment effect and its variance. Since there are no previous studies on reverse Tullock contests, we try to come as close as possible by estimating the expected magnitude of the treatment effect from previous experiments using a positive and negative frame, albeit in a different context (public goods setting). Andreoni (1995) finds that the treatment effect between positive and negative framing of the public good provision is 50 percent. The author reports that the average cooperation rate increases from 16 percent in the negative frame to 34 percent in the positive frame (Andreoni 1995, pp. 7-8). Sonnemans et al. (1998) find that framing the game as public goods provision induces a cooperation rate of 51 percent, which is significantly more than the cooperation rate of 40 percent in the public bad prevention game. The treatment effect amounts to about 25 percent (Sonnemans et al. 1998, p. 149).

It is well documented that experimental contests are characterized by large behavioral variation. The phenomenon is known in the literature as *overspreading* (Sheremeta 2013; Masiliunas et al. 2014; Chowdhury et al. 2014). Therefore, in our power calculation, we have to take into account the notion that the variation in the data from contest experiments is usually larger than in the mentioned public goods experiments. To estimate the coefficient of variation (i.e., the ratio of standard deviation to mean), we rely on two prominent studies involving treatments with conventional Tullock contests under a similar experimental design to ours (e.g., repeated contest, two contestants, partner matching), namely Abbink et al. (2010) and Ahn et al. (2011). Table 4.5 in Appendix B demonstrates that in both studies the coefficient of variation was very similar and amounted to about 36 percent. To be conservative, we assume the higher of the two values, namely 0.366. Based upon an expected treatment effect of 25 percent and a coefficient of variation of 0.366, we need 28 independent observations to detect a one-sided effect at the 5 percent level with the power of 81.02 percent. Thus, in order to have sufficient power we aimed to collect 28 independent observations (i.e., 56 subjects playing in a fixed partner matching) in each treatment. In T0 we have 28 independent observations and in T1 we collected 30 independent observations. Thus, our study has sufficient power to detect treatment differences at conventional levels.

4.4.4. Procedure

The experiment was conducted at the Cologne Laboratory for Economic Research (CLER) in Cologne, Germany. Treatment T0 corresponds to treatment C1 from the study reported in Chapter 2. In all treatments, subjects were recruited with ORSEE (Greiner 2015) and participated in only one session. The experiment was computerized using z-Tree (Fischbacher 2007). Experimental sessions lasted between 60 and 75 minutes. Subjects (undergraduate and graduate students, 58% female, average age: 22.9)[6] earned on average 15.6 EUR.

4.5. Results

In conventional Tullock contests (T0) subjects invest on average 6.855 tokens, which is significantly more than the Nash equilibrium level of 5 (p=0.002, one-sided Wilcoxon signed-rank test).[7] As expected subjects invest on average more when competing to avoid a loss (T1) than when the winner receives a gain (see Figure 4.2). In T1 subjects invest on average 7.882, which is 15 percent more than in the baseline (T0).

[6] Missing information on one subject (self-reports were voluntary).
[7] A pair of subjects over 20 periods constitutes one independent observation.

However, the difference is not statistically significant (p=0.173, one-sided Mann-Whitney U-test).[8]

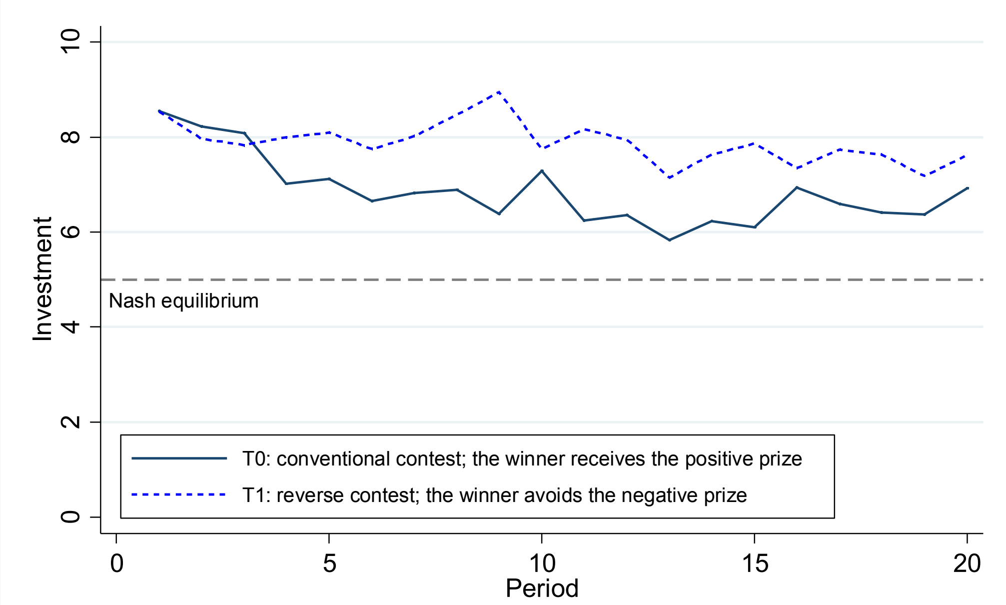


Figure 4.2: Average investments over time.

Table 4.2: Regression analysis.

Dependent variable: Investment	**[I]**	**[II]**
Belief	0.403***	
	(0.053)	
Period	-0.020	
	(0.017)	
1[T1]	0.638	1.027
	(0.505)	(0.827)
Constant	4.142***	6.855***
	(0.563)	(0.526)
Number of observations	2319	2319
Number of clusters	58	58
Overall R-squared	0.286	0.011

Notes: Panel regression with random subject effects. In parentheses, robust standard errors clustered at independent observations. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

[8] The difference is also not significant for periods 5-20 (p=0.119, one-sided MWU-test), periods 10-20 (p=0.123, one-sided MWU-test) and periods 15-20 (p=0.239, one-sided MWU-test).

***Result**: Investments in reverse contests in T1 are on average higher than in conventional Tullock contests (T0). However, the difference is not statistically significant. We reject our hypothesis.*

Our result is confirmed by the insignificant treatment differences in the parametric regression analysis presented in Table 4.2. Moreover, in several other dimensions, the behavior across the two treatments is fairly similar. The distribution of investments (see Figures 4.4 in Appendix B) as well as the pattern of belief-matching (see Figures 4.5 in Appendix B) are also similar to those in conventional contests.

4.6. Conclusion

We are the first to study Tullock contests for avoiding a loss. Prospect theory and results from experiments comparing positive and negative framings suggest that investments in contests for avoiding losses are higher than in contests for achieving gains. Indeed, we find that contests for avoiding losses are fiercer, although the effect is statistically not significant. Thus, it seems that behavior in contests for achieving gains may be an appropriate predictor for contests for avoiding losses.

4.7. Appendix A. Additional treatment T2

An alternative way of implementing the "avoiding the bad" condition would be that rather than lobbying for oneself to *avoid* the bad, contestants could lobby for the opponent to *receive* the bad. In the experimental implementation, players buy lottery tickets not for themselves, but rather for the opponent to increase the opponent's chance of being drawn and hence receiving the loss. However, such an implementation has the drawback that in comparison to T0 it not only differs in the rent dimension. Additionally, in such an implementation, contestants do not want to win the contest, but rather they want the opponent to win. It has been shown that in Tullock contests, players experience some non-monetary utility from the mere act of winning (Sheremeta 2010, 2013). This *joy of winning* is present in T0 and T1 alike. However, if contestants want the other player to win, they potentially lack the joy of winning and we may observe two counteracting effects as compared to T0: *loss aversion* drives higher investments, whereas reduced *joy of winning* lowers investments. For these reasons we decided to predominantly focus on the implementation of the Tullock contest for avoiding the bad as in treatment T1. Nonetheless, we conducted the alternative implementation of buying lottery tickets for the opponent as a treatment T2 (see Table 4.3). As expected, the average investments in T2 are slightly larger than in T0 but smaller than in T1 (see Figure 4.3), although the differences are not significant (see Table 4.4).

Table 4.3: Differences in the framing of the reverse contest.

Treatment	Endowment	Prize	Players buy lottery tickets for…	Contest winner
T1	40	-20	themselves	avoids prize
T2	40	-20	the opponent	receives prize

Table 4.4: Average investments across treatments.

Treatment	No of independent observations	Average investment	Investment difference to T2
T0	28	6.855	p=0.438
T1	30	7.882	p=0.210
T2	30	7.184	n.a.

Note: In the last column, p-values from the one-sided MWU-test for the null hypothesis that investments in T2 are equal to investments in the corresponding treatment.

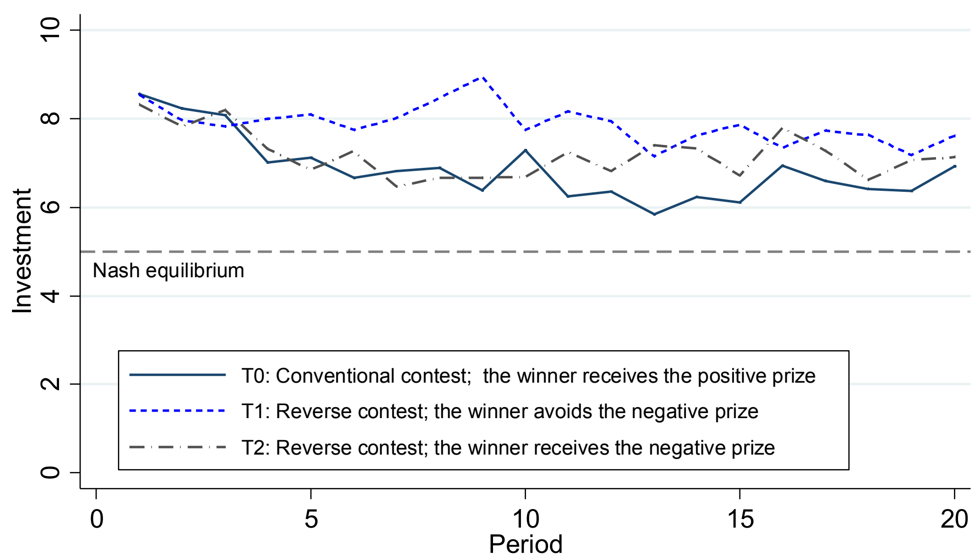


Figure 4.3: Average investments over time (with the control treatment T2).

4.8. Appendix B. Additional tables and figures

Table 4.5: Descriptive statistics of behavior in benchmark studies.

Study	**(1) Ind. Obs.**	**(2) Mean**	**(3) St. Dev.**	**Coefficient of variaton (2)/(3)**
Abbink et al. (AER 2010): treatment 1:1	14	512.96	184.94	0.3605
Ahn et al. (IJIO 2011): Baseline treatment	16	336.93	123.21	0.3657

Notes: Data from Abbink et al.'s study is publicly available under https://www.aeaweb.org/articles?id=10.1257/aer.100.1.420.
Data from Ahn et al.'s study was kindly provided by Tim C. Salmon.

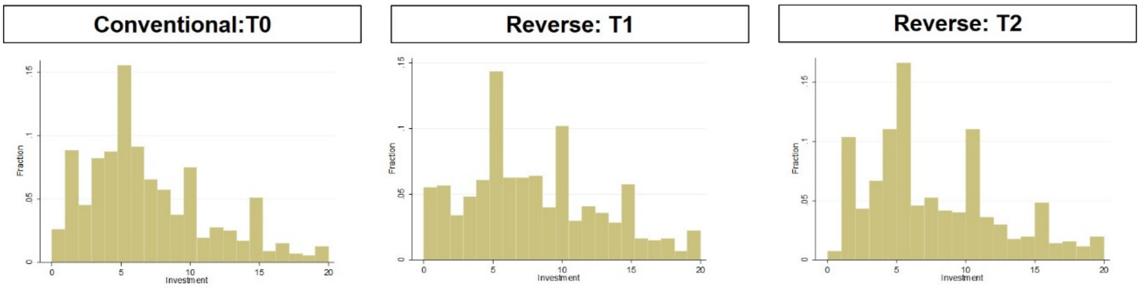


Figure 4.4: Distribution of investments across treatments.
Note: Corrected p-values from the exact Kolmogorov-Smirnov test for the equality of distributions (at the level of independent observations): T0 vs T1: p=0.167; T0 vs T2: p=0.676; T1 vs T2: p=0.536.

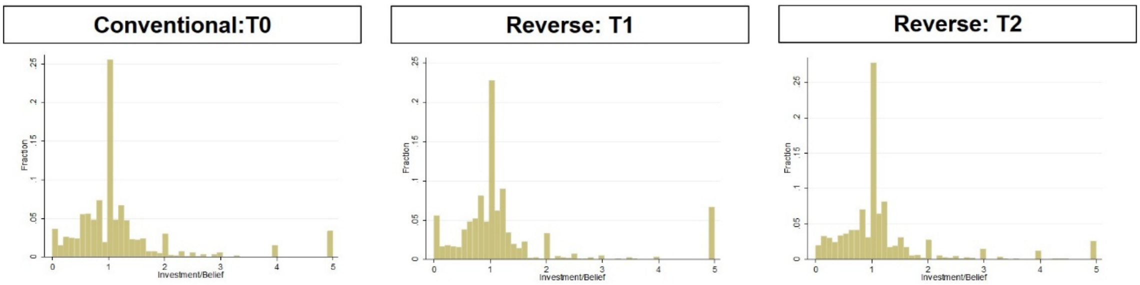


Figure 4.5: Distribution of Belief-Investment-ratio across treatments.
Notes: Ratio=investment / reported belief. Values are censored at 5. We present cases, in which beliefs equal zero as 5 (since the ratio cannot be computed).
Corrected p-values from the exact Kolmogorov-Smirnov test for the equality of distributions (at the level of independent observations): T0 vs T1: p=0.911; T0 vs T2: p=0.890; T1 vs T2: p=0.958.

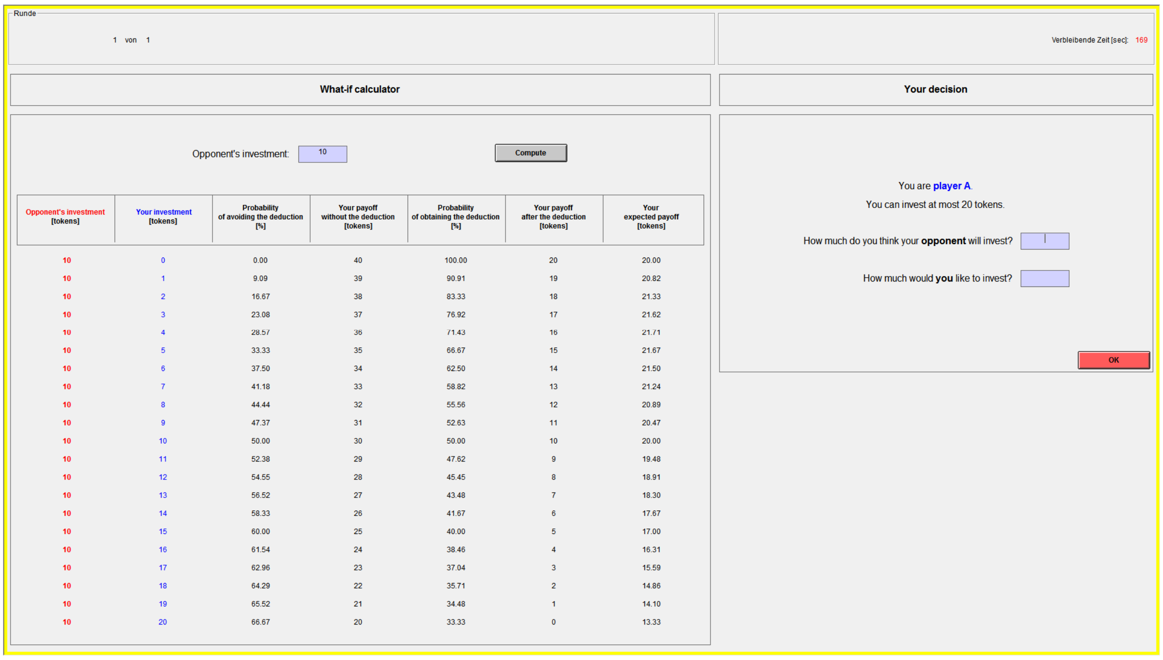


Figure 4.6: Decision screen in treatment T1 (also part of the written instructions to subjects). Translation from German.

4.9. Appendix C. Experimental instructions. Treatment T1 (translation from German)

Note: We report in the following the instructions for treatment T1, of which the instructions to T0 and T2 are straightforward variations. In the text, we only indicate the differences in the framing of the prize, as the remaining discrepancies are minor. Original instructions are available upon request.

General information

Welcome to our experiment! It is important that you carefully read and understand the following instructions. If you have a question, please raise your hand. We will then come to you and answer it. Communication with other participants before and during the experiment is prohibited. If you violate this rule, you will have to leave the experiment and will not receive any payment.

You can earn money in this experiment. You will receive 4.00 EUR for your participation. You can earn additional money during the experiment. The amount of money that you earn depends on your decisions and the those of other participants in the experiment. Your earnings are denoted in tokens. These will be converted into EUR and paid out in cash at the end of the experiment. The exchange rate is:

40 tokens = 1 EUR.

The experiment comprises several rounds. Tokens that you earn in each round are added to your tokens account. Your payoff is the sum of the tokens that you have earned in all rounds of the experiment. No participant will receive information about your payoff in the experiment or your identity.

Course of the experiment

In today's experiment, you will interact with another participant. You will shortly be randomly matched with an opponent. In every pair, there will be a player A (marked blue) and a player B (marked red). The roles are also randomly assigned. The pairings as well as the roles remain unchanged for the entire experiment. Information about your role will be displayed on the screen before the first round begins.

The experiment comprises **20 rounds**. All rounds proceed in the same way. In each round, you and your opponent will compete in a lottery.

At the beginning of each round, you receive from us an endowment of **40 tokens**. Then, you can decide how many tokens you want to spend on lottery tickets. For one token, you can purchase one ticket (1 token = 1 ticket). You can purchase as many tickets as you want. However, you are not allowed to spend more than 20 tokens in a single round. Tokens that you do not spend on lottery tickets are added to your account.

*[T1: The lottery decides who in a given round gets **20 points deducted from the endowment.** If one of your tickets is drawn, you avoid the deduction.]*

*[T0: The prize that you can win in the lottery in each round is **20 tokens**.]*

*[T2: The lottery decides who in a given round gets **20 points deducted from the endowment** and you buy the lottery tickets "for your opponent" (tickets of the opponent's color). This means that if you are player A (blue) you buy red tickets, and if you are player B (red) you buy blue tickets. If a ticket of your color is drawn, you are deducted 20 points.]*

Your chance of avoiding the deduction depends solely on how many tickets you have bought and how many your opponent has bought. The more tickets you have bought, the <u>more likely</u> it is that you will avoid the deduction. The more tickets your opponent has bought, the <u>less likely</u> it is that you will avoid the deduction. The probability with which you avoid the deduction is equal to the number of your tickets divided by the number of all tickets bought. This means that it is computed according to the following rule:

$$\text{Probability of avoiding deduction} = \frac{\text{Number of your tickets}}{\text{Number of your tickets} + \text{Number of opponent's tickets}}$$

Your earnings in a single round are as follows:

Your earnings if you win the lottery= 40 – your investment in the tickets

Your earnings if you lose the lottery = 40 – your investment in the tickets – 20

If only one player has bought tickets, she/he wins with certainty and her/his opponent is deducted the 20 tokens. If neither of the players has bought any tickets, the lottery does not take place and both players get the deduction.

Each round comprises three steps:

Step 1

In the first step, you decide how many tickets you would like to purchase. At the same time, your opponent makes the same decision.

While you make your decision, you can use a what-if calculator. You can insert a hypothetical investment by your opponent and the calculator computes the probabilities of avoiding and obtaining the deduction and corresponding payoffs, subject to your investment. The expected payoff is also computed.

The expected payoff is computed in the following way:

$$\text{Expected payoff} = \text{Probability that you avoid deduction} \times \text{Payoff without deduction} + \text{Probability that you obtain deduction} \times \text{Payoff after deduction}$$

You can use the calculator as often as you want. Please simply insert a value in the field of the opponent's investment and click on "Compute" (see red field (1) in Figure 1).

The example in Figure 1 shows the what-if-calculator for a hypothetical investment of 10. For instance, you can see in the table that if you invest 2 tokens, your probability of avoiding the deduction is 16.67%, whereby your payoff will amount then to 38 tokens. You will lose the lottery and obtain the deduction with the probability of 83.33%. In such a case, your payoff will be 18 tokens. Your expected payoff will be 21.33 tokens.

For example, if you invest 19 tokens, you will win the lottery and achieve a payoff of 21 tokens with a probability of 65.52%, while will lose and achieve a payoff of 1 token with a probability of 34.48%. Your expected payoff will be 14.10 tokens.

In every round you should insert within the given time your final decision in the fields on the right-hand side of the screen and confirm with the "OK" button (see red field (2) in Figure 1). You are not only asked about your investment, but also what investment you expect from your opponent. For this you can earn a bonus of maximal **4 tokens**. The amount of the bonus depends on how good your prediction was. The smaller the mistake in your prediction, the higher your bonus. The bonus is computed in the following way.

$$\text{Bonus} = 4 - 0.4 * \text{Mistake}^2$$

The mistake is the difference between your prediction and actual opponent's investment:

$$\text{Mistake} = |\text{Your prediction} - \text{actual opponent's investment}|$$

If the mistake is larger than 3, the bonus is 0. This means that there is no negative bonus, whereby you cannot lose any tokens for your prediction.

The time for your decision in every round is limited. In the first ten rounds you have 3 minutes time, in the next 10 rounds 2 minutes. If you do not insert any values within the given time, your investment in the round will be zero and you will not receive any bonus for your prediction.

Step 2

In the second step, you receive feedback on the opponent's decision, in other words, how many tickets she/he has bought. The probabilities of winning are also computed and displayed. All tickets bought are numerated. You receive information on which numbers correspond to your tickets and which to those of your opponent. In order to make it clearer, this is also displayed graphically.

Each ticket is equally likely to be drawn.

You also receive feedback concerning how good your prediction about your opponent's investment was and how large your bonus is.

Step 3

In the last step, the winning ticket is drawn. The computer draws one of the bought tickets. The number of the winning ticket and the information about who is awarded a deduction are displayed on the screen, as are your earnings in the current round.

[Subjects also received a screen shot of the decision stage mask, see Figure 4.6. The only difference is that we marked decision fields with red frames, as described in the instructions above.]

Chapter 5: HETEROGENEOUS EFFECT OF GROUP IDENTITY IN COLLECTIVE RENT-SEEKING

5.1. Introduction

Rent-seeking behavior, ubiquitous in many areas of economics, has usually been studied experimentally using the framework of a lottery contest (Tullock 1980; Dechenaux et al. 2015). Although rent-seeking behavior is not only pursued by individuals but also by groups (e.g., firms or political parties), collective rent-seeking has only recently gained attention in experimental economics. Abbink et al. (2010) provide the first experimental evidence on contests between teams and compare individual and group behavior in Tullock contests. They find that groups do not suffer from free-riding problems as much as predicted by game theory and therefore overinvest more than single players do. In addition, within teams subjects exhibit substantial and persistent heterogeneity in behavior. While some recent studies provide new experimental results on collective Tullock contests (e.g., Ahn et al. 2011; Cason et al. 2012, Leibbrandt and Sääksvouri 2012; see recent survey by Sheremeta 2015b), none of them addresses the puzzle of large behavioral heterogeneity within teams. To close this gap, in a new experiment I replicate Abbink et al. (2010)'s finding of substantial heterogeneity within teams and show that this is explained by the degree to which individuals identify with others in their group.

5.2. Related literature

My study relates to several streams of literature in economics and social psychology. I study behavior in collective rent-seeking. Theoretical literature on rent-seeking contests between groups was started by Katz et al. (1990) and Nitzan (1991). Experimental investigation on collective rent-seeking (pioneered by Abbink et al. 2010) is relatively new and recently summarized by Sheremeta (2015b).

Since I investigate the effect of group identity, this paper contributes also to the psychological and economic literature on group identity and group behavior. Due to its long tradition of research on groups, social psychology has profound understanding of how group identity emerges and how it changes individual behavior. Experimental evidence from the 1970s (e.g., Tajfel et al. 1971) fueled the development of social identity theory (Tajfel and Turner 1979) that remains a cornerstone of modern research on group behavior. However, psychologists only rarely consider environments involving a tension between the self-interest and the interest of others.[1] For that reason, recent economic investigation of group identity complements previous psychological findings. Since Akerlof and Kranton's (2000) theoretical work that introduced the notion of group identity into economics, much experimental evidence on economic implications of group identity has been collected. Among others, social identity has been shown to alter cooperative behavior (e.g., Eckel and Grossman 2005), social preferences (e.g., Chen and Li 2009), coordination behavior (e.g., Chen and Chen 2011), trust and trustworthiness (e.g., Heap and Zizzo 2009), or willingness to punish others (e.g., Goette et al. 2012). Less attention has been devoted to groups in competitive environments and this is where my study makes a contribution.

5.3. The experimental design

Katz et al. (1990) extend Tullock's model by introducing groups as contestants and the prize being a public good.[2] I implement the set-up by Katz et al. and largely follow the design of Abbink et al. (2010).

[1] Research by Gary Bornstein constitutes an important exception (Bornstein 1992, 2003, Kugler and Bornstein 2013). Bornstein experimentally investigated behavior in so-called team games (Bornstein 2008). However, he did not pay much attention to the group identification process itself.

[2] Note that the prize exhibits public good properties only within a group, i.e., in the case of winning all group members receive the same monetary gain. Excludability of non-members is, however, possible (Esteban and Ray 2001, p. 664).

5.3.1. The experimental set-up

In the experiment, randomly created pairs of five player groups (labelled A, B, etc.) play a Tullock contest for T=20 rounds. The composition of groups as well as the pairing of competing parties are fixed throughout the entire experiment. Each round has the same structure and proceeds in three steps.

At the beginning, each subject i is endowed with 1,000 points and decides how many of these she wants to spend on lottery tickets for her group k. One point corresponds to one ticket. All players make their decisions simultaneously and then feedback is given in two steps. First, the subject learns the aggregated ($X_k = \sum_i x_{i,k}$) and average ($\frac{1}{5}X_k$) investments for her team, as well as the aggregated investment of the opposing team (X_{-k}). The probabilities of winning for each team resulting from the lottery ticket investments are represented graphically on the screen. Second, the result of the lottery contest is presented. The computer randomly picks one of the lottery tickets and the group that owns it wins the prize. Subjects' payoffs in the current round are also computed and displayed. After that, the experiment proceeds to the next round. The prize (V) is fixed at 1,000 points and does not depend on the number of tickets purchased. If neither of the competing groups buys any tickets, however, the lottery is not conducted and neither team wins the prize. The expected payoff of subject i belonging to group k is:

$$E(\pi_{i,k}) = 1000 - x_{i,k} + \frac{X_k}{X_k + X_{-k}} 1000 \tag{5.1}$$

5.3.2. Hypotheses

I hypothesize that although not directly induced, group identity arises spontaneously in experimental collective contests, facilitated by the between-team competition (Eckel and Grossman 2005) and the elements of payoff commonality ("*either we all win the prize, or none of us",* see Brewer and Kramer 1986). The strength of the group attachment may vary from subject to subject, however, not only across but also within teams.

Social group theory (Tajfel and Turner 1979) recognizes social comparison as a crucial element of the group identification process. Groups seek for positive distinctiveness that should justify or legitimize their existence. In order to accomplish this goal, they are willing to compete against out-groups (Turner 1975). In the course of the so-called social competition, groups compete for some scarce resources that have no value outside of the context of competition, e.g., rank, status, prestige of winning (Tajfel 1982). In experimental contests, winning the prize is a natural device of building positive distinctiveness of a group against out-groups. This means that winning itself becomes important. Therefore, I expect subjects who more strongly identify with their teams to invest more than subjects who do not report strong attachment to their teams.

5.3.3. Procedure

The experiment was computerized and programmed in z-Tree (Fischbacher 2007). I conducted three sessions in October 2014. Ninety subjects (mostly students with various majors, average age: 24.7; 55.1% female[3]) were recruited with ORSEE (Greiner 2015) and earned on average 11.30 EUR. The subjects received written instructions, which were also read aloud (see Appendix A). Each session lasted about 45 minutes. I collected data on the decisions of 18 teams of five players, i.e., nine independent observations.

5.4. Results

In equilibrium, a group should invest a total of $V/4$ (i.e., 250 points in this experiment) regardless of the group's size. Abbink et al. (2010) find that teams invest more than the Nash equilibrium predicts. In their experiment, teams of four invest on average 1,035 points. Similarly in my experiment, teams of five also strongly overinvest. The average team contribution is 920 points, which is almost four times as much as in equilibrium. I focus, however, on another striking behavioral pattern. Like Abbink et al. (2010, pp. 431-432), I find substantial behavioral heterogeneity within teams.

For each team and each period, I identify players contributing the most and the least to the team investment. As presented in Figure 5.1, the behavioral heterogeneity is not only large but also very persistent. Top contributors invest on average 402.3 points in each round (390.9 in the last five rounds), whereas lowest contributors tend to free-ride on their peers from the beginning on and invest on average only 30.9 points (15.9

[3] One subject did not report her/his gender.

in the last five rounds). Moreover, top contributors lower their investments only in the initial rounds and keep investing twice as much as the average investment (184.0) until the end of the experiment. In general, I observe little convergence in behavior within teams. Similar to Abbink et al. (p. 432), I conclude that some individuals behave as activists and remain so throughout the experiment.

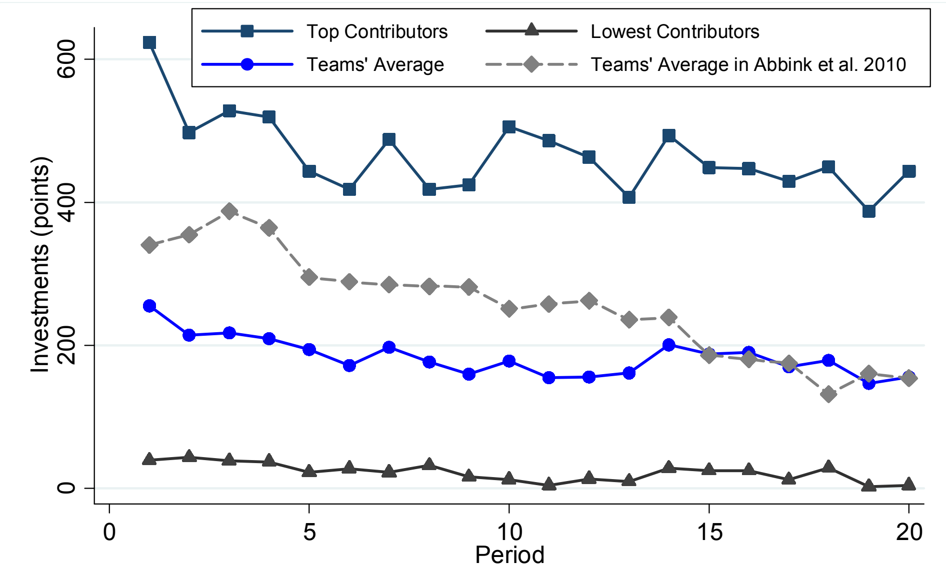


Figure 5.1: Individual investment behavior and within-group heterogeneity.

To examine the reasons for this persistent behavioral heterogeneity, subjects filled out a questionnaire about their motives and behavior in the experiment. One of the questions concerned group attachment: "*Please state your opinion on a scale from 1 (do not agree at all) to 7 (totally agree): In the experiment, I experienced a strong sense of a team spirit*". Eighty-nine out of 90 subjects responded to this question. The following analysis is based on data from these 89 subjects.

I classify answers into three categories: 1-2 = no identification; 3-5 = weak identification; 6-7 = strong identification. Figure 5.2 depicts mean investments of subjects that reported a similar level of group identification. There is a clear pattern: The stronger the degree of group attachment the higher the investment in the lottery. Behavior of subjects from different groups cannot be directly compared, since they were engaged in contests with different dynamics and hence different levels of investments. Therefore, I average investments of subjects from the same team, exhibiting similar levels of group identification (none, weak, strong). I non-parametrically compare pairwise behavior between the three categories at the group level using a Wilcoxon signed-rank test (WSR-test). All differences are statistically significant at the 5 percent level. The same significance levels hold if we only consider the data from the last five periods.

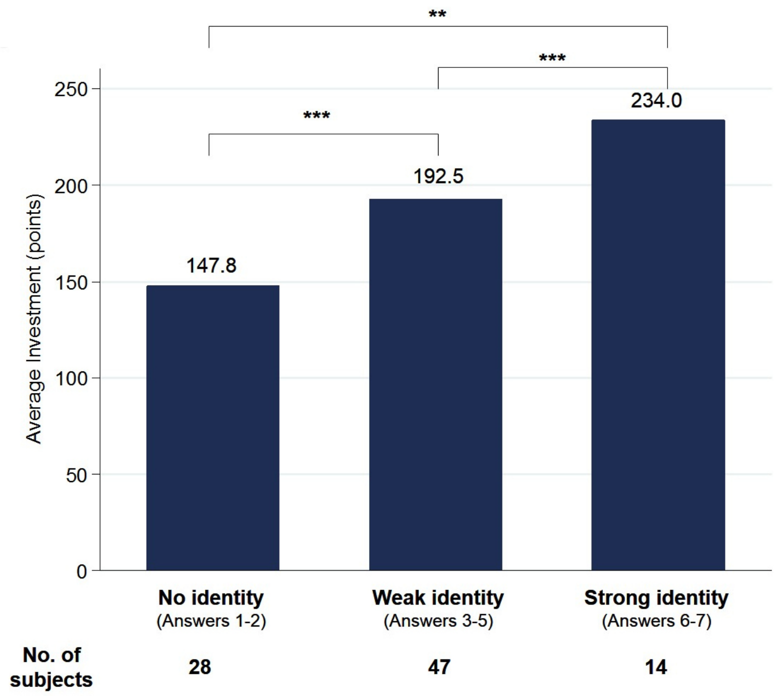


Figure 5.2: Group identity and average investment behavior.
Notes: Asterisks correspond to results of WSR-test (two-tailed) at the group level.
Significance levels: *** p<0.01; ** p<0.05.

The significant effect of group identification is confirmed in a parametric regression analysis. As 15.6 percent of the observations show zero investments, I estimate Tobit models censored at zero. I consider two specifications (see Table 5.1). In model [I], I follow the previously discussed classification of subjects into three categories. The group identity effect is captured with two dummies that correspond to the conditions of weak and strong group identification (hence, no group identification is the reference category). Since such an approach is prone to degrees of freedom in specifying the categories, I also test an alternative model [II] that does not involve any categorization, but treats team identity as a continuous variable. Regardless of how the group identity regressor is defined, its effect remains both statistically significant at the 5 percent level and sizeable. In model [I], a weak group identification induces an average increase in investments of about 52 points. Strong group identification leads to an average increase of 79.5 points relative to the reference category, corresponding to 43.2 percent of the mean investment in the experiment.

Table 5.1: Regression analysis (Tobit models).

Dependent variable: **Individual investment*(t)***	**[I]**	**[II]**
Own investment*(t-1)*	0.508***	0.512***
	(0.057)	(0.061)
Team investment*(t-1)*	-0.018	-0.018
	(0.026)	(0.027)
Competitor investment*(t-1)*	0.066***	0.065***
	(0.010)	(0.010)
1[Team identity weak]	52.383**	
	(21.892)	
1[Team identity strong]	79.453**	
	(28.595)	
Team identity		14.891**
		(5.399)
1[Lost*(t-1)*]	-1.722	-1.617
	(8.399)	(8.496)
Period	-0.425	-0.418
	(0.573)	(0.576)
Constant	-8.000	-21.720
	(26.921)	(30.263)
Observations	1691	1691
No. of subjects	89	89
Pseudo-R^2	0.028	0.027

Notes: Tobit models censored at 0. In parentheses, robust standard errors clustered on cohorts (pairs of teams). Significance levels: *** p<0.01, ** p<0.05.

The focus of the analysis is on the behavioral heterogeneity and not on the overinvesting. Nevertheless, the effect of group identity also helps to explain the latter phenomenon.[4] In equilibrium, two opposite effects of the group size just offset each other. On the one hand, the increasing size of the group induces stronger free-riding and hence lowers individual investments. On the other hand, it leads to a higher total

[4] Here, my evidence is complementary to the study on parochial altruism in collective contests by Abbink et al. (2012).

investment. Since group identity has been shown to reduce the free-riding incentive (Eckel and Grossman 2005), subjects identifying themselves with groups tend to invest more than game theory predicts. Despite its significant role, observed group identity is fairly weak as teams are randomly created and team members have no prior connection to one another. Real groups or organizations would be expected to display higher levels of identification, leading to even stronger overinvestment.[5] Moreover, depending on the context, rent-seeking may be perceived as wasteful from a social viewpoint. If this is the case, group identity has a welfare-decreasing effect as it drives higher investments. This is in stark contrast to previous results on welfare-enhancing effects of group identification.

5.5. Conclusion

In this paper, I show that group identity and its heterogeneous effect among subjects help to explain strong and persistent heterogeneity in behavior within groups engaged in experimental contests. The overall effect of group identity also rationalizes reported substantial overbidding. I demonstrate that subjects very differently respond to the same experimental conditions, which prevents the experimenter from maintaining control over the saliency of collective identities in the lab. This echoes the conclusions by Riener and Wiederhold (2013), who stress the importance of manipulation checks when inducing group identities under laboratory conditions. My results complement their findings by stating that such checks are also important in experiments in which group identity can arise endogenously and is not exogenously induced by the experimenter.

[5] In a very recent study, Chowdhury et al. (2016) show in a laboratory experiment that groups built on a real identity (race) overinvest more strongly than groups with artificial *ad-hoc* identities.

5.6. Appendix A: Written instructions for subjects (translation from German)

General information

Welcome to our experiment! It is very important that you carefully read and understand the following instructions. If you have any questions please raise your hand. We will then come to you and answer them. Communication with other participants before and during the experiment is prohibited. If you violate this rule, you will have to leave the experiment and will not receive any payments.

In this experiment you can earn money. You will receive 2.50 EUR for your participation. You may earn additional money during the experiment. Your income will depend on your decisions and decisions of other participants. During the experiment, your earnings will be quoted in points. These will be converted into EUR at the end of the experiment at the exchange rate of:

3000 points = 1 EUR.

The experiment will consist of multiple rounds. Points that you earn in each round will be added to your account. Your income in the experiment will be computed as a sum of points earned in all rounds of the experiment. Participants will not get information about identity or earnings of other participants.

Course of the experiment

In today's experiment, participants are divided into teams of five. Teams are labeled with letters (A, B, …). The assignment to teams is conducted randomly before the first round begins. The composition of teams is kept fixed for the entire experiment. Within a team, each participant is assigned a player number (1 to 5) and this number is also kept fixed.

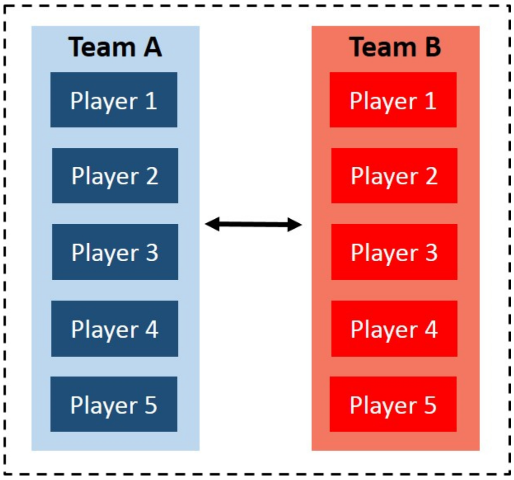
Information on your team and number assignment will be displayed before the first round begins.

In the experiment, your team will be assigned to an opponent team, with which your team will be interacting. Pairs of teams remain the same for the entire experiment.

Pairing procedure is explained on an example.

Example

In the example team A is assigned to team B. This means that team A will interact with team B (see graph) in all rounds of the experiment.



The experiment consists of 20 rounds. All rounds proceed in the same way. In each round, your team and the opponent team will compete in a lottery for a prize.

At the beginning of each round, you receive from us 1000 points. Then, you can decide how many points you want to spend on lottery tickets for your team. For one point you can buy one ticket (1 point = 1 ticket). You can buy as many tickets as you want, but you are not allowed to exceed your budget. Points that you do not spend on lottery tickets are added to your account.

The prize that your team can win in the lottery in each round is 1000 points for each team-member (i.e., 5000 in total for the team).

Your winning chance depends only on how many tickets your team has bought and how many the opponent team has. The more tickets your team has bought the more likely it is that your team wins. Another way around, the more tickets the opponent team has bought, the less likely it is that your team wins. The probabilities with which your team wins the prize is equal to the number of tickets of your team divided by the number of all tickets bought. This means it is computed according to the following rule:

$$\text{Your probability of winning} = \frac{\text{Number of tickets of your team}}{\text{Number of tickets of your team} + \text{Number of tickets of the opponent team}}$$

Your earnings in a round are as follows:

Your earnings if your team wins = 1000 – your investment in the tickets + 1000

Your earnings if your team loses = 1000 – your investment in the tickets

If only one team has bought the tickets, it wins with certainty. If neither of teams have bought any tickets, the lottery does not take place and nobody wins the prize.

Each round consists of three steps:

1. In the first step, you decide how many tickets you would like to buy. At the same time all other participants from your team and the opponent team make the same decision.
2. In the second step, you get the feedback on the decisions of your team and the opponent team. Probabilities of winning are also computed and displayed. All tickets bought are numerated. You get the information which numbers correspond to tickets of your team and which to tickets of the opponent team. In order to make it clearer, this is also displayed graphically. Each ticket is equally likely to be drawn.
3. In the third step, the winning ticket is drawn. The computer draws one of bought tickets. The number of the winning ticket and the winning group are displayed on the screen. So are your earnings in the current round.

Questionnaire (translation from German)

Please report your age: _________

Please report your gender: _________

Please report your major of studies: _________

Please report for how long you have been studying (number of terms): _________

Please describe briefly how you were deciding on how many tickets to buy.

____

____

Please report your opinion on the following statements:

In the experiment, I experienced a strong sense of a team spirit. ☐

(Scale: 1= don't agree at all, ... , 7= totally agree)

My decisions in the experiment were influenced by the fact that we were playing against another team. ☐

(Scale: 1= don't agree at all, ... , 7= totally agree)

In the experiment, it was important to me that my team wins more often that the opposite team. ☐

(Scale: 1= don't agree at all, ... , 7= totally agree)

Chapter 6:
ENVY IN DYNAMIC CONTESTS

Joint work with Uta K. Schier

6.1. Introduction

Situations of conflict and rent-seeking are as common as trade and cooperation. Therefore, contest and rent-seeking behavior has been extensively studied both theoretically (see e.g., Congleton et al. 2008a for an overview) and experimentally (for a survey, see Sheremeta 2013 and Dechenaux et al. 2015). Most of this research considers static set-ups, in which there is only one round of interaction. Yet in many situations (e.g., R&D competition, political election, sports) the winner is determined on the basis of more than one round. In order to win the prize or obtain the rent, a contestant needs to win a certain number of rounds. In theory, such situations are modeled as *multi-battle contests*.[1]

This paper focuses on incentives to keep participating in the contest despite having lost chance of winning and therefore contributes to the research on pervasiveness of dynamic contests.[2] We consider a setting with two players. Our theoretic analysis and a laboratory experiment show that the contestant who has already lost her chance of winning the prize may want to stay in the contest, if she displays other-regarding preferences and ties in the number of wins are possible. In other words, the underdog may not give up, even though he can no longer win the prize.

[1] In this paper, we consider a class of multi-battle contests called races. There are, however, also other types of dynamic contests, such as elimination contests or the tug-of-war. See Konrad (2009, Chapter 8).
[2] We recognize that whether pervasiveness of contests is desirable or not depends on the circumstances of the contest. Without making any claims on that, we investigate the pervasiveness *per se*.

Our analysis complements previous research on pervasiveness of dynamic contests. Konrad and Kovenock (2009) study a generic class of multi-battle contests and characterize a unique subgame perfect equilibrium. The authors show that the contest remains pervasive only in the presence of intermediate prizes. *Pervasive* means at all points the contest is non-trivial; none of the competitors gives up in any of the battles. Therefore, Konrad and Kovenock (2009, p. 266) argue that "*intermediate prizes are important to avoid the series of battles becoming rather uninteresting once one of the players has accumulated a sufficient advantage that the other player gives up*." The statement is very intuitive when applied to sporting events, but is also important in other contexts, e.g., patent races. Gelder (2014) extends Konrad and Kovenock's (2009) framework by introducing monetary penalties for losing. This means pervasiveness is achieved by adding financial incentives. However, these are discounted over time so that losing later induces smaller monetary consequences than losing earlier, which allows the author to rationalize last stand behavior (i.e., reluctance to surrender).

Our study provides an alternative explanation for why contests can be pervasive. Based on social utility models, we show that disadvantageous inequality aversion (i.e., envy) can lead to pervasive contests without intermediate prizes or monetary penalties. Social utility models have been extensively studied in the literature and suggest that utility not only depends on one's own monetary payoff but also on others' payoffs (e.g., Fehr and Schmidt 1999; Bolton and Ockenfels 2000; Charness and Rabin 2002).

We consider a framework with two battles, each modeled as a Tullock (1980) contest. In order to obtain the prize, one of the two players needs to win both battles. A tie in wins (1:1) leads to the prize not being awarded. Such a structure results in a straightforward game-theoretic prediction, in which the second battle is trivial, with the loser of the first battle (hereafter the "underdog") surrendering. Envious preferences of the players, however, change the theoretic prediction. The second battle is no longer trivial, mostly because an envious underdog, left without a chance to win the contest, might still invest resources just to harm the other player. This not only influences the total dissipation of rents, but might also impact the probability that the prize is awarded.

In fact, situations in which a tie in wins results in the prize not being awarded are frequently observed in markets. For example, blocking (or pre-emptive) patents are an increasing threat to many high-tech industries. Consider the prospect of launching a new product that would be innovative in several dimensions. The launching company needs to have the patents for all technologies involved. As a result, a company holding a patent on one particular (even small) innovation can block other market players from commercializing their products (Heller and Eisenberg 1998; Guellec 2012). Such situations are often found in sectors of complex technologies, such as software or pharmaceuticals. Multiple surveys (Dueget and Kabla 1998; Cohen et al. 2002; Blind et al. 2006) suggest that most companies consider defensive blocking an important reason for patenting at all.

We run a laboratory experiment to test predictions from our theoretic analysis and find that contestants indeed behave enviously. They are ready to spend considerable amounts of their limited resources to prevent their competitor from winning the prize. Interestingly, such behavior is not surprising for prospective winners; they expect envy and adjust their own investments. Moreover, subjects already anticipate competitive behavior in the second battle when investing in the first battle. This means that envy-driven competition leads to lower investments in the initial round of competition.

The remainder of the chapter is organized as follows. In Section 6.2 we briefly discuss the related literature. Section 6.3 presents our theoretic model and its game-theoretic analysis under both standard and envious preferences. In Section 6.4 we develop an experimental design that allows us to test our theoretic predictions from Section 6.3. Finally, we present the experimental results in Section 6.5 and conclude in Section 6.6.

6.2. Related literature

This work contributes to literature on races, which are a type of dynamic contests. Harris and Vickers (1985, 1987) opened this theoretic research in the context of patent races. They show that under technological uncertainty the leader in a race exerts higher efforts than the follower. Moreover, efforts are higher if the gap between competitors is small. This research was applied by Klumpp and Polborn (2006) to study the dynamics of the US political elections. The authors consider the rationale behind sequential (not simultaneous) campaigning in different US states, as a setting of multi-battle contests. In a more general and context-free approach, Konrad and Kovenock (2009) then describe equilibrium behavior in multi-battle (all-pay) contests. This model has recently been extended by Gelder (2014), who introduces monetary penalties for losing thereby changing the equilibrium predictions.

There is little experimental evidence on behavior in dynamic contests. Zizzo (2002) is the first to experimentally test Harris and Vickers' (1987) model and finds only limited support for its predictions. More recently, Mago and Sheremeta (2012), as well as Irfangolu et al. (2015), compare behavior in sequential and simultaneous multi-battle (best-of-three) contests, with battles modeled either as Tullock contests or all-pay auctions, respectively. Moreover, Mago et al. (2013) study best-of-three Tullock contests and find support for 'strategic' but not 'psychological' momentum. Finally, Gelder and Kovenock (2014) examine last stand behavior in best-of-seven contests, testing the theoretical predictions of Gelder's (2014) model, as well as those of the model by Konrad and Kovenock (2009). Therefore, their experimental design involves both monetary prizes and penalties. In line with Gelder's model, they find that high penalties for losing prevent the prospective loser from giving up. No research has so far looked at social comparison as a behavioral motive in such settings. Considering that contests typically involve interactions between several players, we are interested in how envy affects contest behavior.

Social preferences have already been shown to influence behavior in other competitive settings. Besides contests, relevant examples of such environments are auctions and tournaments. For instance, Morgan et al. (2003) theoretically study how disutility from losing an auction changes equilibrium bidding behavior. The authors show that such preferences explain behavior observed in experimental auctions better than standard preferences. In particular, they help explain overbidding.[3] Kimbrough and Reiss (2012) provide experimental evidence on spiteful bidding in second-price auctions. The authors show that subjects frequently submit a non-zero bid in order to increase the price paid by the competitor. Such behavior could also be explained by inequity aversion (i.e., envy). Also tournaments are important contest models in modern microeconomics. In tournaments, inequity aversion has also been shown to play a role. Grund and Sliwka (2005) game-theoretically analyze behavior in tournaments, assuming

[3] Note that Morgan et al. (2003) refer to the modeled deviation from standard preferences as *spite.* Nevertheless, their intuition behind this term corresponds to what we call *envy.* Both terms are frequently used interchangeably in the literature when describing disadvantageous inequity aversion in the mold of Fehr and Schmidt (1999).

preferences based on the inequity aversion model by Fehr and Schmidt (1999), and demonstrate that inequity aversion drives higher efforts. This result has also been confirmed by experimental studies. Balafoutas et al. (2012) test in the laboratory the link between social preferences and tournament behavior and show that more spiteful subjects react more strongly to the competitive condition. In another experimental study, Eisenkopf and Teyssier (2013) provide additional evidence that envy drives higher efforts in tournaments. Herrmann and Orzen (2008) investigate spite in Tullock contests. Our approach is most similar to their study. However, unlike the authors, who examine the impact of other-regarding preferences on bidding in standard (static) Tullock contests, we investigate dynamic rent-seeking settings.

Finally, our theoretic framework and experimental investigation are related to the literature on armed conflicts. This literature rests on the canonical model of the Tullock (1980) contest, but adapted to peculiarities of military contests (e.g., Hirshleifer 1989, 1991; Skaperdas 1992; see Garfinkel and Skaperdas 2007 for an overview). The experimental literature on war behavior is relatively new and limited, but rapidly growing. Abbink (2012) surveys experiments on this topic. In this stream of literature, our work is related to the recent paper by Lacomba et al. (2014). The authors study how post-conflict behavior (e.g., possibility of income destruction) influences conflict expenditures and stealing rates. In some experimental treatments, the loser of the conflict can lower the winner's loot by destroying a part (or all) of her own income. While this treatment may be similar to our experimental game in expected terms, the settings are different in several dimensions. Most importantly, in the experiment by Lacomba et al. (2014), the value of the prize is endogenous and results from subjects' decisions. Endogeneity of the prize value (typical for studies on military conflicts) is one of the major differences from the set-ups utilized in research on traditional rent-seeking contests.

6.3. The theoretical framework

Previous experiments on dynamic environments of rent-seeking have mostly examined best-of-three (or best of $2n+1$) contests (Mago and Sheremeta 2012; Mago et al. 2013; Irfangoulu et al. 2015). However, in such games one cannot disentangle strategically driven behavior from behavior driven by social comparison. In best-of-three contexts, the contest only runs as long as of minimum of two contestants have a chance of obtaining the rent. Hence, it is not clear to what extent behavior is driven by a motive to win the prize or to simply avoid lagging behind. Therefore, we develop a new game that allows us to distinguish these motives.

Moreover, in contrast to Konrad and Kovenock (2009) and Gelder (2014), who model battles as all-pay auctions, we define the battles as lottery contests (Tullock 1980). In the literature on contest behavior, both approaches are very common, and which model is considered more appropriate depends rather on the real-world examples to which it is being applied.[4] Konrad and Kovenock (2009, p. 258), as well as Gelder (2014, p. 444), opt for an all-pay auction model due to analytical convenience. However, for testing theoretic predictions in laboratory experiments, a Tullock contest seems to be more favorable. Unlike all-pay auctions, where equilibria exist only in mixed strategies (Baye et al. 1996), a Tullock contest with two contestants is characterized by a unique equilibrium in pure strategies (Perez-Castrillo and Verdier 1992; Szidarovszky and Okuguchi 1997).[5] Such an equilibrium prediction provides a better behavioral benchmark for experimental investigations.

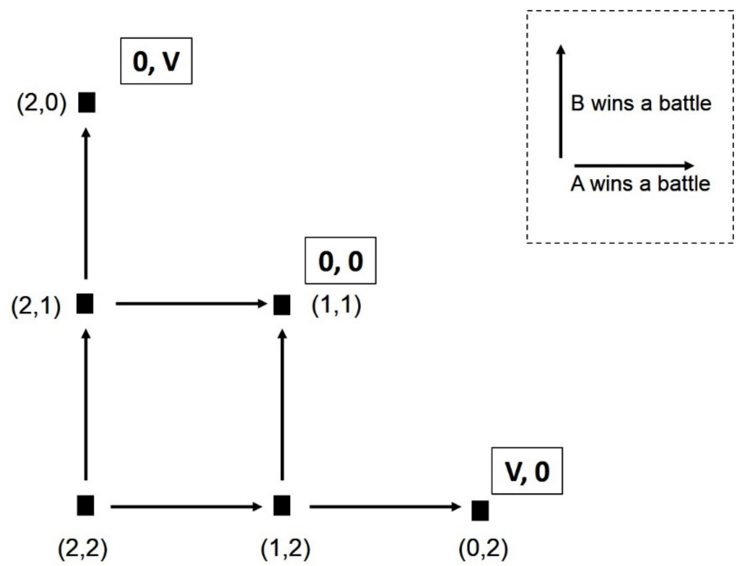


Figure 6.1: Contest structure.
Note: In parentheses are the numbers of winnings needed to obtain the prize. In the boxes are the prizes from the contest.

[4] For example, battles have been modeled as lottery contests in previous studies on patent races (Harris and Vickers 1987), as well as US presidential nomination campaigns (Klumpp and Polborn 2006).
[5] This holds under the assumption that the parameter $r \leq 2$. We use the most standard version of the model, with $r = 1$. See the references for more details.

6.3.1. The two-battle contest

We consider a two-battle contest with two ex-ante symmetric and risk-neutral players, each of them initially endowed with E. Let X_A and X_B denote the total contest investments by player A and B, respectively. $X_A = x_{A,1} + x_{A,2}$ and $X_B = x_{B,1} + x_{B,2}$, where $x_{A,t}$ and $x_{B,t}$ describe investments in battle *t*. There is a single prize V for the winner of both battles. In case of a tie in wins (1:1), neither of the players wins the prize (see Figure 6.1). Moreover, we assume that if in one of the battles both players make zero investments, no player wins the battle and the prize is not awarded.

Every battle is a rent-seeking contest (Tullock 1980). The probability that player A wins battle *t* is:

$$p_{A,t} = \frac{x_{A,t}}{x_{A,t} + x_{B,t}} \tag{6.1}$$

Hence, her competitor, player B, wins the battle with probability: $p_{B,t} = 1 - p_{A,t}$. Further, player A's payoff is as follows:

$$\pi_A = \begin{cases} E - X_A + V & if\ player\ A\ wins\ both\ battles \\ E - X_A & otherwise \end{cases} \tag{6.2}$$

Player B's payoff is computed analogously. Both players are informed about the outcome of the first battle before the second battle begins.

6.3.2. Equilibrium analysis under standard preferences

We solve the game by backward induction and find a unique symmetric subgame perfect Nash equilibrium (SPNE).

Battle 2

Assume player A won the first battle. Player B then has no chance of winning the overall contest and therefore gives up for the second battle, with an investment of $x^*_{B,2} = 0$. Player A anticipates that her competitor surrenders, invests almost nothing ($x^*_{A,2} \to 0^+$) and wins the second battle and therefore the entire contest.[6]

Battle 1

Ex ante, players are symmetric. Therefore, the expected payoff of player A is:

$$E[\pi_A] = E - x_{A,1} + \frac{x_{A,1}}{x_{A,1} + x_{B,1}} V \tag{6.3}$$

[6] For analytic simplicity, we assume in the consecutive analysis that $x^*_{A,2} = 0$.

First order condition yields:

$$\frac{\partial E[\pi_A]}{\partial x_{A,1}} = -1 + \frac{x_{B,1}}{\left(x_{A,1} + x_{B,1}\right)^2} V = 0 \tag{6.4}$$

Analogously, we can define FOC for player B. In a symmetric equilibrium, both players invest:

$$x_{A,1}^* = x_{B,1}^* = \frac{V}{4} \tag{6.5}$$

Thus, the payoffs of the winner (player A) and loser (player B) are in equilibrium:

$$\pi_A = E + \frac{3}{4}V\ ;\ \ \pi_B = E - \frac{1}{4}V \tag{6.6}$$

As such, the game-theoretic prediction for this dynamic contest does not differ from the prediction for a conventional Tullock contest. In other words, contestants behave in the first battle *as if* there were only one battle (i.e., a static contest). In the equilibrium, 50 percent of the rent is dissipated.

6.3.3. Other-regarding preferences: intuition

In the next step, we introduce a behavioral extension of the standard analysis. Assume that contestants display other-regarding preferences. In particular, assume that they are envious (but not compassionate, i.e., display only disadvantageous inequity aversion). We adjust the utility function as in Fehr and Schmidt (1999); we assume that parameter $\alpha \geq 0$ and $\beta = 0$.[7,8] The utility of player B is therefore now equal to her monetary utility and possibly the disutility stemming from the disadvantageous inequity aversion:

$$U_B(\pi_A, \pi_B) = \pi_B - \alpha \max\{0, \pi_A - \pi_B\} \tag{6.7}$$

We restrict our attention to cases in which α is strictly positive, but not larger than 1 $(0 < \alpha \leq 1)$.[9]

---

[7] Note that the intuition would not change if players were compassionate ($\beta > 0$), as long as $\alpha \geq \beta$, which is assumed by Fehr and Schmidt (1999, p. 822). The "reduced" form of Fehr-Schmidt preferences, including only envy (but not compassion), has been applied by Eisenkopf and Teyssier (2013) to derive behavioral predictions for their experiment on tournament environments. Moreover, such an approach is similar to the one put forward by Bolton (1991).

[8] Moreover, in appendix C we analyze the contest under an evolutionary definition of other-regarding preferences. We show that the theoretic predictions are qualitatively very similar and therefore robust against a particular modeling approach.

[9] Without a significant loss of generalizability, the assumption makes the analysis more tractable.

Let us reconsider the situation of player B in the second battle. We have seen that in the subgame perfect Nash equilibrium under standard preferences, he surrenders and his investment in the second battle is 0. We show that if player B is envious, such behavior is no longer a part of SPNE. We check incentives for the player to deviate from the studied equilibrium by making a small but strictly positive investment in the second battle: $x_{B,2} = \epsilon > 0$. Then, expected utilities are as follows:

$$E\left[U_B \middle| x_{B,2} = 0\right] = E - \frac{V}{4} - \alpha V \tag{6.8}$$

$$E\left[U_B \middle| x_{B,2} = \epsilon > 0\right] = E - \frac{V}{4} - \epsilon - \alpha\epsilon \tag{6.9}$$

It is straightforward to show that expression (6.8) is smaller than (6.9), if $\frac{\alpha}{1+\alpha} > \frac{\epsilon}{V}$. Therefore, under the assumption of a continuous strategy space, a strictly positive value of the envy parameter α always leads to a non-zero investment in the second battle. If player A (potential winner) sticks to the SPNE, player B has an incentive to invest ϵ in the contest. Player B can almost completely avoid the envy-related disutility for a very low monetary cost.

6.3.4. Equilibrium analysis under symmetric envious preferences

We have shown that the SPNE from Section 6.3.2 no longer holds if the underdog is envious. In the following, we conduct a more systematic analysis of the role of envy in the considered set-up. For simplicity, we assume that the value of the envy parameter α is symmetric and common knowledge.[10] Moreover, we restrict our attention to cases in which behavior in the first round is symmetric (i.e., symmetric equilibria). Investments in the first round in a symmetric SPNE are equal ($x^*_{\mathrm{A},1} = x^*_{\mathrm{B},1}$). Therefore, investment inequalities can only arise due to asymmetric investment behavior in the second battle ($x^*_{\mathrm{A},2} \neq x^*_{\mathrm{B},2}$).[11]

We proceed by backward induction to obtain a new subgame perfect Nash equilibrium prediction.

[10] See Herrmann and Orzen (2008) for a similar approach in conventional Tullock contests.
[11] This means that later on we can replace the condition $X_A \geq X_B$ with $x_{\mathrm{A},2} \geq x_{\mathrm{B},2}$.

Second battle

First, we consider the winner of the first battle (player A). Her expected payoff is:

$$E[U_A] = \begin{cases} E - X_A + \dfrac{x_{A,2}}{x_{A,2} + x_{B,2}} V - \alpha \dfrac{x_{B,2}}{x_{A,2} + x_{B,2}} (X_A - X_B) & if\ X_A \geq X_B \\ E - X_A + \dfrac{x_{A,2}}{x_{A,2} + x_{B,2}} V & if\ X_A < X_B \end{cases} \tag{6.10}$$

Note that player A can experience envy if she loses the second battle and her total investment is larger than the investment of her competitor ($X_A > X_B$). The first order condition, after simplification, is defined by:[12]

$$x_{B,2} V - 2\alpha x_{B,2}^2 = \left(x_{A,2} + x_{B,2}\right)^2 \ \ if X_A \geq X_B \tag{6.11a}$$

$$x_{B,2} V = \left(x_{A,2} + x_{B,2}\right)^2 \ \ if X_A < X_B \tag{6.11b}$$

Therefore, under the restriction that the investment cannot be negative, the best reply function is given by:

$$x_{A,2}\left(x_{B,2}\right) = \begin{cases} \max\left\{0,\ \sqrt{x_{B,2} V - 2\alpha x_{B,2}^2} - x_{B,2}\right\} & if\ X_A \geq X_B \\ \max\left\{0,\ \sqrt{x_{B,2} V} - x_{B,2}\right\} & if\ X_A < X_B \end{cases} \tag{6.12}$$

Now, we consider the loser of the first battle. The expected payoff function of player B is:

$$E[U_B] = \begin{cases} E - X_B - \alpha \dfrac{x_{A,2}}{x_{A,2} + x_{B,2}} (V - X_A + X_B) & if\ X_A \geq X_B \\ E - X_B - \alpha \dfrac{x_{A,2}}{x_{A,2} + x_{B,2}} (V - X_A + X_B) - \alpha \dfrac{x_{B,2}}{x_{A,2} + x_{B,2}} (-X_A + X_B) & if\ X_A < X_B \end{cases} \tag{6.13}$$

The loser experiences envy if the winner gets the prize or/and if his total investment was higher than the competitor's investment. The first order condition, after simplification, gives:[13]

$$\alpha x_{A,2} V - 2\alpha x_{A,2}^2 = \left(x_{A,2} + x_{B,2}\right)^2 \ \ if\ X_A \geq X_B \tag{6.14a}$$

$$\alpha x_{A,2} V = (1 + \alpha)\left(x_{A,2} + x_{B,2}\right)^2 \ \ if\ X_A < X_B \tag{6.14b}$$

Therefore, the best reply function is given by:

$$x_{B,2}\left(x_{A,2}\right) = \begin{cases} \max\left\{0,\ \sqrt{\alpha x_{A,2} V - 2\alpha x_{A,2}^2} - x_{A,2}\right\} & if\ X_A \geq X_B \\ \max\left\{0,\ \sqrt{\dfrac{\alpha}{1 + \alpha} x_{A,2} V} - x_{A,2}\right\} & if\ X_A < X_B \end{cases} \tag{6.15}$$

---

[12] The second order condition for a local maximum is fulfilled if $x_{B,2} < \frac{V}{2\alpha}$.

[13] The second order condition is fulfilled as long as $x_{A,2} < \frac{V}{2}$.

It can be shown that that $x_{A,2}^{*} \geq x_{B,2}^{*}$ for all values of $0 < \alpha \leq 1$.[14]

The Nash equilibrium of the second stage is determined by the intersection of the two well-defined best-reply functions (eq. (6.12) and (6.15)). In order to find a symmetric Nash equilibrium, we need to solve a system of two quadratic equations with two variables. The system is defined by eq. (6.11a) and (6.14a).

A non-trivial (unique) solution in which $x_{A,2}$, $x_{B,2}>0$ exists, but its closed form solution cannot be easily derived and is not very useful due to its complexity. However, using iterative methods we can find a unique solution for particular values of parameters α and V.[15] Figure 6.2 depicts equilibrium investments as a function of α. Moreover, examples of equilibrium behavior for several values of the envy parameter are presented in Table 6.1. It becomes apparent that as long as $\alpha \leq 1$, the underdog's investment increases with the strength of inequality aversion (envy), but the winner's investment is hyperbolic. The total investment made in the second battle increases with α.

First battle

With regard to the first battle, recall that contestants are ex ante symmetric. When we consider player A, we can re-write the expected winner's and loser's utilities in eq. (6.10) and (6.13) as $-x_{A,1} + U^W$ and $-x_{A,1} + U^L$, respectively. Thus, we can express player A's expected utility as:

$$E[U_A] = E - x_{A,1} + \frac{x_{A,1}}{x_{A,1} + x_{B,1}} U^W + \frac{x_{B,1}}{x_{A,1} + x_{B,1}} U^L \qquad (6.16)$$

The first order condition simplifies to:

$$\frac{x_{B,1}}{(x_{A,1} + x_{B,1})^2} U^W - \frac{x_{B,1}}{\left(x_{A,1} + x_{B,1}\right)^2} U^L = 1 \qquad (6.17)$$

We can derive an analogous condition for player B, which results in a symmetric equilibrium. Hence, equilibrium investments in the first battle are equal to:

$$x_{A,1}^{*} = x_{B,1}^{*} = \frac{U^W - U^L}{4} \qquad (6.18)$$

Again, a general algebraic solution is not useful. However, for fixed values of α and V we can compute the values U^W and U^L and can therefore estimate equilibrium

[14] See Proof 1 in Appendix A.
[15] See Klumpp and Polborn (2006) for a similar approach.

investments in the first battle (see Table 6.1). Theoretic predictions are graphically displayed in Figure 6.2.

Figure 6.2b reveals that envious players invest significantly more in the contest than non-envious players with standard preferences (the standard Nash equilibrium prediction). This is due to positive investments in the second battle, which are zero under standard preferences. Investments in the first battle decrease when non-zero investments in the second battle are anticipated. However, this effect is only marginal. As a consequence, rent dissipation in the SPNE increases with envious players.

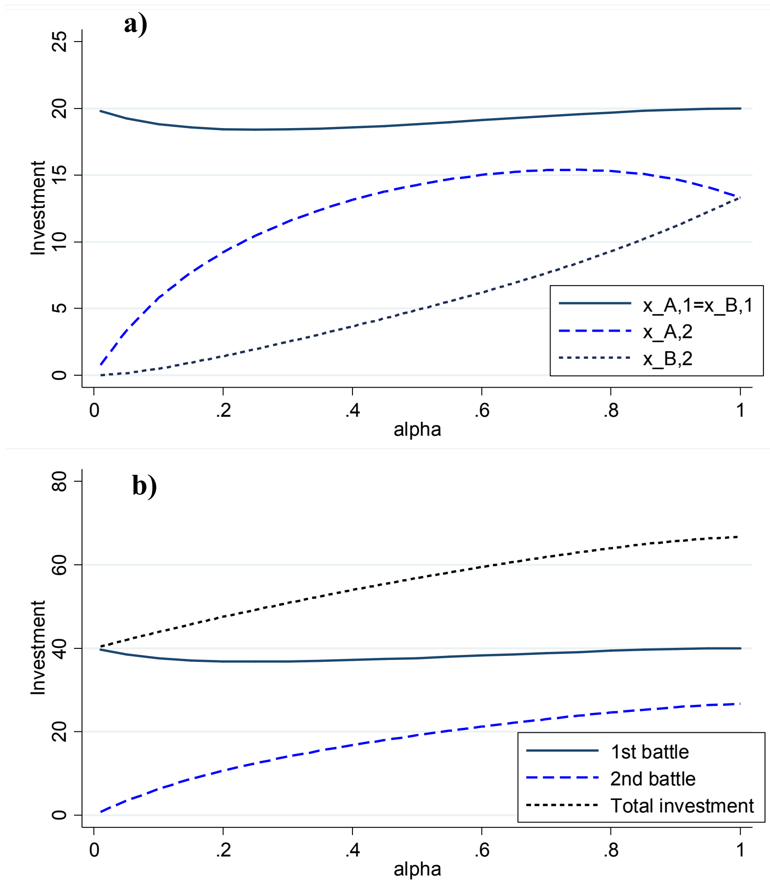


Figure 6.2: Equilibrium behavior under envious preferences (V=80): (a) individual investments; (b) total contest expenditures
Note: Computed in STATA® (using a non-linear equation system).

6.3.5. Efficiency of equilibrium behavior

Further, behavior induced by envious preferences has important implications for the efficiency of the entire contest. Here, we can identify two sources of inefficiency:

rent dissipation due to contest investments (direct effect) and the possibility that the prize is not being awarded (indirect effect).

Figure 6.2b shows that the total investment in the contest increases under stronger envy parameters α. Moreover, the probability of the prize not being awarded also increases with α. Both aspects lead to less efficient contest outcomes. The last two columns of Table 6.1 demonstrate this result. For strong envious preferences, the expected overall rent dissipation is larger than the prize.[16] In comparison to contests with standard preferences, where the prize is always awarded and 50 percent of the rent is dissipated in the equilibrium, we can conclude that envy can cause tremendous efficiency losses in the setting studied.[17]

Table 6.1: Equilibrium behavior (*V*=80).

	Static contest		**Dynamic contest**				
α	$x^*_{A,1} = x^*_{B,1}$	Rent dissipation	$x^*_{A,1} = x^*_{B,1}$	$x^*_{A,2}$	$x^*_{B,2}$	Direct rent dissipation	Prob. prize awarded
0.00	20.00	50.00%	20.00	0.00	0.00	50.00%	100.00%
0.10	20.95	52.38%	18.81	5.80	0.50	54.89%	92.12%
0.25	22.22	55.56%	18.40	10.48	1.96	61.55%	84.26%
0.30	22.61	56.52%	18.42	11.52	2.51	63.60%	82.12%
0.50	24.00	60.00%	18.83	14.27	4.89	71.02%	74.49%
0.65	24.91	62.26%	19.27	15.24	6.91	75.86%	68.81%
0.75	25.45	63.64%	19.56	15.40	8.44	78.71%	64.60%
0.90	26.21	65.52%	19.91	14.69	11.18	82.12%	56.80%
1.00	26.67	66.67%	20.00	13.33	13.33	83.33%	50.00%

Notes: Computed in STATA® (using a non-linear equation system). Equilibrium behavior for static contests under symmetric envious preferences is described by:
$x^*_{A,1} = x^*_{B,1} = \frac{1+\alpha}{2+\alpha}\frac{V}{2}$ (see Herrmann and Orzen 2008, p. 39).

[16] Denote γ the probability that the prize is awarded. Then, the total dissipation of rent (in expected term) is equal to: $(1-\gamma)V + X_A + X_B$. Note that for instance for α=0.75, total dissipation would amount to 114.1% (compare Table 6.1).

[17] Note that in certain contexts, additional contest investments may not be considered wasteful. For instance, when applying the setting to sporting events, a non-trivial second battle can induce social benefits.

6.4. The experiment

In order to test our theoretic predictions presented in Section 6.3, we ran a laboratory experiment. The general structure of the experiment in the main treatment follows a set-up of a dynamic contest with two battles. We framed and organized the rent-seeking contests as lotteries.

6.4.1. The experimental implementation[18]

In the main treatment, two subjects (called A and B) are symmetrically endowed with E=80 tokens and compete for a monetary prize of V=80 tokens. Ten tokens correspond to 1 EUR. The entire game consists of two battles, neutrally framed for subjects as "*stages*." Each stage proceeds in three identical steps. First, subjects are allowed to purchase lottery tickets, using their individual endowment (one ticket costs one token). Subjects make decisions simultaneously. When deciding how many tickets to buy, they can use a customized calculator to compute winning probabilities (see Figure 6.11 in Appendix D). After subjects have decided on their investment, they receive feedback about the investment decision of their opponent and the resulting winning probabilities. In the last step, the computer randomly draws one of the tickets bought by both players in that stage.[19] Whoever bought that ticket is the winner of the stage, which is announced for both players on screen. This three-step procedure is repeated for the second stage. Payoffs across both stages for each player are equal to:

$$\pi = \begin{cases} 80 - spendings\ on\ lottery\ tickets & if\ prize\ is\ not\ won \\ 80 - spendings\ on\ lottery\ tickets + 80 & if\ prize\ is\ won \end{cases} \tag{6.19}$$

In our experiment, we investigate one-shot interactions, which are standard for studies on social preferences, often involving ultimatum or dictator games. Across all treatments, we also elicited beliefs about the behavior of the competitor. This was done simultaneously with the investment decision at each stage, and subjects' estimations were incentivized according to a quadratic loss function, capped at 0:

$$Bonus = \max\{0;\ 5 - 0.05(Belief - Actual\ behavior)^2\} \tag{6.20}$$

[18] For written instructions, see Appendix D.

[19] Analogous to our assumption in Section 6.3, subjects were told that if no tickets are bought by any player in a single stage, the lottery does not take place and nobody wins the stage. However, this actually never occurred in the main treatment.

6.4.2. Treatments

In our main treatment, called ENVY, subjects play in pairs the game described in Section 6.3, under the implementation presented in Section 6.4.1. They compete against each other in two consecutive lotteries. Only subjects who win both lotteries are awarded a prize.

Our experiment aims at testing whether envy drives behavior in the second battle. However, envy may not be the only reason that losers from the first battle make a positive investment in the second battle. Some of the non-zero investments may be due to mistakes (Potters et al. 1998) or experimenter demand effect (Zizzo 2010). Moreover, subjects may attribute a non-monetary utility to the mere fact of winning the stage, i.e., display *joy of winning* (Sheremeta 2010, 2014), which can drive non-zero investments in the second battle. To disentangle envious behavior from these other non-monetary motives, we use the tie-breaking rule as a treatment variable.

In a control treatment, called NOENVY, we implement an alternative tie-breaking rule, which stipulates that if there is a tie in wins, the prize is awarded to the winner of the first battle. For example, if player A wins the first battle and player B wins the second battle, the prize is awarded to player A. Under such a tie-break rule, the first battle is decisive for who wins the prize. The first battle loser has no possibility to prevent this. Consequently, envy cannot drive behavior in the second battle, which, by contrast, is possible in the ENVY treatment. However, even without scope for envy, subjects in the NOENVY treatment can still make mistakes or experience joy of winning or demand effects in the second stage. Hence, our treatment variation manipulates only the presence of envy motives, and it has no impact on motives other than envy. The comparison between the NOENVY and the ENVY treatments will reveal the net effect of envy in the considered setting.

Further, we introduced a static contest as another benchmark treatment (STATIC). In this treatment, the contest is reduced to a conventional (one-battle) contest. All other parameters remain unchanged. This treatment allows us to test whether subjects indeed behave *as if* there were only one relevant round of contest. The experimental treatments are summarized in Table 6.2.

Table 6.2: Overview of experimental treatments.

Treatment	Tie-breaking rule	Focus
STATIC	n.a. (only one battle)	Benchmark for *as-if* behavior
NOENVY	If 1:1, the first battle winner wins the prize	No envy-driven behavior
ENVY	If 1:1, no one wins the prize	Envy effect

6.4.3. Hypotheses

Based on our theoretic predictions from Section 6.3, we derive four main hypotheses for our experiment. The null hypothesis describes behavior of a *homo oeconomicus* player (perfect rationality and selfish preferences), whereas the alternative hypotheses specify behavioral players (with bounded rationality and envious preferences).

Following backward induction, we start with behavior in the second stage. Recall that a perfectly rational player who maximizes her monetary payoff has no incentive to invest in the second battle in the NOENVY treatment because she has already won or lost the prize. To understand whether players display any motives other than payoff-maximization, we formulate Hypothesis 6.1.

Hypothesis 6.1 (Second-stage behavior): Game-theoretic rationality

H0: In the second stage of the dynamic contest in the NOENVY treatment, both players invest 0.

HA: Both contestants invest more than 0 in the second stage of the contest in the NOENVY treatment.

Moreover, we introduce Hypothesis 6.2 to examine whether envy leads to differences in behavior between the ENVY and NOENVY treatments. Remember that under selfish preferences, subjects who lost the chance of winning the prize should behave the same way in the NOENVY compared to the ENVY condition.

Hypothesis 6.2 (Second-stage behavior): Envy

H0: Investments of prospective losers in ENVY are as high as those in NOENVY.

HA: Investments of prospective losers in ENVY are higher than in NOENVY.

Furthermore, to investigate whether winners of the first stage anticipate envious behavior in the second stage, we compare behavior of the first-stage winner in ENVY with the NOENVY treatment. Under selfish preferences of contestants, subjects should not expect any investments in the second stage. This also implies that subjects who still have a chance of winning the prize (ENVY) behave in the second stage in the same way as subjects who have already won the prize (NOENVY).

Hypothesis 6.3 (Second-stage behavior): Anticipation of Envy
H0: Investments of prospective winners in ENVY are as high as in NOENVY.

HA: Investments of prospective winners in ENVY are higher than in NOENVY.

Finally, we show in our theoretic analysis that under standard preferences and perfect rationality, subjects behave in the first stage as if there were only one stage. Thus, our last hypothesis is as follows:

Hypothesis 6.4 (First-stage behavior): Anticipation effect
H0: Across treatments, investments in the first stage are equal.

HA: First-stage investments in the static contest are higher than in the dynamic contests.

6.4.4. Experimental procedure

We conducted a between-subject experiment with three treatments in the Cologne Laboratory for Economic Research (Germany) in November 2015. The experiment was computerized with z-Tree (Fischbacher 2007), and 212 subjects were recruited via ORSEE (Greiner 2015). Subjects were undergraduate and graduate students from various faculties (66% female, mean age: 23.1) and earned on average 13.68 EUR (standard deviation 4.44), including a show-up fee of 4 EUR.

The experimental sessions lasted between 45 – 60 min and always proceeded in the same way across all treatments. At the beginning of the experiment, we elicited risk preferences of subjects using a menu of incentivized scenarios, in which subjects had to choose between a lottery or a safe option (similar to Holt and Laury 2002).[20] After the main part of the experiment, one of the scenarios was randomly drawn, played and paid out. The results of risk preferences task were revealed to subjects after the main part.

6.5. Results

In this section, we present results following our main hypotheses. For the analyses, we consider one pair of subjects as an independent observation. We collected 36 independent observations each in the NOENVY and ENVY treatments, and 34 in the STATIC treatment.

6.5.1. Behavior in the second battle

First, we analyze behavior in the second battle to examine Hypothesis 6.1. As can be seen in Figure 6.3 subjects on average invest positive amounts in the second battle. Specifically, in the treatment NOENVY, both winners and losers on average make positive investments (5.4 and 3.5, respectively), implying that non-envy motives (mistakes, joy of winning, experimenter demand) play a role for investments in the second stage. In both cases, the behavior is significantly different from zero (both $p<0.001$; Wilcoxon signed-rank test, hereafter WSR-test). Winners tend to invest slightly more. The difference is, however, not statistically significant: $p=0.539$ (WSR-test). This leads to the first result:

Result 6.1: *The total effect of mistakes, joy of winning and experimenter demand is non-negligible. We reject null Hypothesis 6.1.*

Second, our main experimental question concerns the difference in behavior between the NOENVY and ENVY treatments. Looking at the ENVY treatment, losers from the first stage invest on average 12.4 tokens, which is more than triple the amount we observe in the NOENVY treatment. This difference is statistically significant: $p=0.011$ (Mann-Whitney U-test, hereafter MWU-test). Similarly, non-zero investments are more frequent in the ENVY treatment than in the NOENVY treatment (61% and 36%, respectively). This difference is also significant by a χ^2-test ($p=0.034$). The result is also

[20] See the experimental screen from z-Tree in Figure 6.10 in Appendix D, as well as the distribution of elicited proxy of risk preferences depicted in Figure 6.8 in Appendix B.

confirmed in the parametric regression analysis (see column [I] in Table 6.4). The treatment dummy is statistically significant in the regression for losers from the first battle. They invest on average about 8.5 tokens more in ENVY treatment than in NOENVY treatment, which amounts to more than 10 percent of their initial endowment.

Responses collected in a post-experimental questionnaire provide additional evidence that envy is a significant decision-driver. After the experiment, we asked subjects to recall how intense their emotions were after they had learned the outcome of the first stage. We used an elicitation procedure similar to Bosman and van Winden (2002) and Hopfensitz and Reuben (2009), i.e., subjects self-reported their indicated emotions on a Likert scale between 1 and 7. Subjects who lost the first battle are in general rather reluctant to admit to being envious (the average reported level is 2.9). Still, the self-reported level of envy is significantly correlated with the level of investment in the second battle (Spearman's rho 0.350, p=0.039).[21]

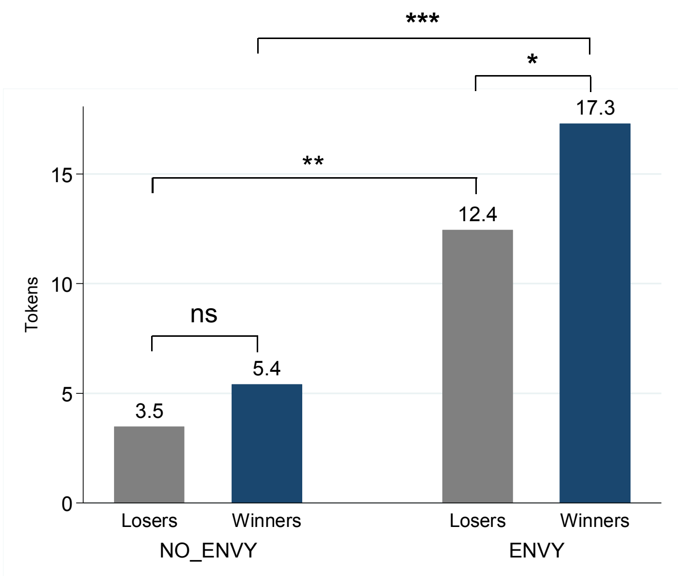


Figure 6.3: Average investments in the second battle.

Notes: Asterisks correspond to results of non-parametric tests (WSR-test for the within-treatment comparison and MWU-test for the between-treatment comparison). Significance levels: *** p<0.01; ** p<0.05; * p<0.1; ns p≥0.1.

Zizzo and Oswald (2001) show in their "*burning money*" experiment that the willingness to reduce others' income depends on the perceived desert of this income

[21] Note that the English word "envy" can be translated into German either as "Neid" or as "Missgunst," which correspond to "benign" and "malicious" envy, respectively (see e.g., Smith and Kim 2007). We ask subjects to report both. The values reported in the main text describe "Missgunst," i.e., malicious envy.

(i.e., whether someone "*earned*" the income). We do not find such a regularity in our setting. Desert of winning the first battle does not matter for envy-driven behavior in our experiment. There is no systematic difference in behavior between losers who lost the first battle despite having invested more than their competitor (and who therefore may perceive the outcome as unfair) from those who lost after having invested less than the competitor.

These results demonstrate that we can summarize the behavior of losers in the second battle as follows:

Result 6.2: *Envy substantially increases losers' investment in the second battle. Players are willing to invest significant amounts of money to prevent competitors from winning the prize. We reject null Hypothesis 6.2.*

Third, we examine whether this envy-driven behavior by losers in the second battle is expected by winners from the first battle. In other words, do they adjust their investments to counteract envy-driven behavior in the second stage? We indeed find a highly significant behavioral adjustment. In the ENVY treatment, winners from the first stage increase their average investments in the second battle to 17.3, which is more than triple the amount that we observe in the NOENVY treatment ($p<0.001$, MWU-test). The parametric regression analysis arrives at the same conclusion (see column [II] in Table 6.4). The treatment dummy variable is highly significant. Winners invest on average almost 12 tokens more in the ENVY treatment than in the NOENVY treatment, which corresponds to 15 percent of the initial endowment. We also find evidence that the increase in investments is at least partially driven by the anticipation of envy. The average reported beliefs about the underdog's investment increase from 11.4 in NOENVY to 17.6 in ENVY. The difference is not statistically significant ($p=0.173$, MWU-test). Nevertheless, the higher beliefs about underdog's envy-driven investment drive higher investments at least to some extent; these two are significantly correlated (Spearman's rho 0.370, $p=0.027$).

Result 6.3: *In the ENVY treatment, first-battle winners expect envy-driven behavior and respond by increasing their investments in the second battle (as compared to NOENVY). We reject null Hypothesis 6.3.*

6.5.2. Behavior in the first battle

In this section, we analyze behavior in the first battle. As derived in Section 6.3, given standard preferences, subjects in dynamic two-battle contests should behave as if there were only one battle. We test this prediction by comparing the results from the ENVY treatment with results from the NOENVY and the STATIC treatment.

First, in the ENVY condition, subjects correctly anticipate the non-trivial second battle and therefore reduce their investments in the first battle (see Figure 6.4). In numbers, the average investment in the ENVY treatment of 19.5 is significantly lower than 28.5 in the NOENVY treatment ($p<0.01$, MWU-test).

We also observe a weak framing effect between the STATIC and NOENVY treatments. Note that in both treatments behavior in the first battle completely determines the winner of the entire contest. Nevertheless, framing the contest as a two-battle game may influence the perception of the prize. Therefore, average investments slightly decrease from 35.5 in the STATIC treatment to 28.5 in the NOENVY treatment. However, this difference is not statistically significant ($p=0.112$, MWU-test).

Result 6.4: *In the ENVY treatment, subjects anticipate envy and competition in the second battle and therefore invest less in the first battle than in the NOENVY treatment. We reject null Hypothesis 6.4.*

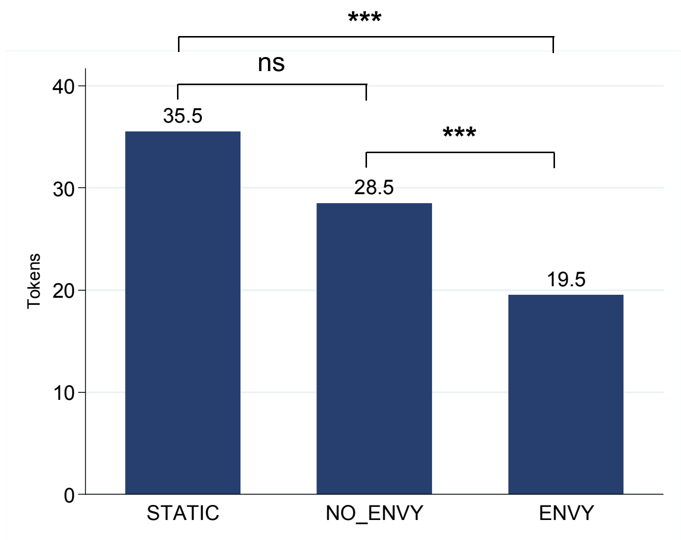


Figure 6.4: Average investments in the first battle.

Note: Asterisks correspond to results of a non-parametric MWU-test;
Significance levels: *** $p<0.01$; ** $p<0.05$; * $p<0.1$; ns $p\geq0.1$.

Table 6.3: First battle behavior. Regression analysis.

Dependent variable: Investment 1. battle	**STATIC** [I]	**STATIC** [II]	**All treatments** [III]	**All treatments** [IV]
Belief		0.901*** (0.141)		0.737*** (0.068)
1[NOENVY treatment]			-5.680* (3.431)	-0.739 (2.735)
1[ENVY treatment]			-16.342*** (3.443)	-5.940** (2.873)
1[Female]	9.409 (5.937)	4.339 (4.650)	5.074* (2.923)	5.661** (2.299)
Risk preferences (proxy)	-1.933 (1.993)	-0.655 (1.552)	0.421 (1.085)	0.560 (0.853)
Constant	36.987*** (10.175)	1.702 (9.592)	28.746*** (5.925)	1.187 (5.311)
Number of observations	62	62	192	192
R-squared	0.053	0.446	0.119	0.459

Notes: OLS regressions. In parentheses, standard errors.
Risk proxy: number of risky choices (integer number between 0 and 10).
We consider only subjects with consistent risk preferences (i.e., single switching point).
Significance levels: *** p<0.01; ** p<0.05; * p<0.1.

Table 6.4: Second battle behavior. Regression analysis.

Dependent variable: Investment 2. battle	**LOSERS** [I]	**WINNERS** [II]
1[ENVY treatment]	8.497** (3.417)	11.889*** (2.787)
1[Female]	3.421 (3.759)	2.311 (2.901)
Constant	1.287 (3.387)	3.941 (2.705)
Number of observations	72	72
R-squared	0.103	0.215

Notes: OLS regressions. In parentheses, standard errors.
Significance levels: *** p<0.01; ** p<0.05; * p<0.1.

6.5.3. Beliefs and behavior in static Tullock contests (STATIC treatment)

Aside from inter-treatment comparisons and treatment effects, results from the STATIC treatment serve as our benchmark for comparing our results to previous experimental evidence on Tullock contests, summarized in e.g., Sheremeta (2013). Unlike most previous studies that investigate repeated interactions, we report behavior in a one-shot experiment. Still, we replicate two major phenomena prominent in the literature. First, we find significant overbidding; subjects invest significantly more than predicted by the Nash equilibrium under standard preferences ($p<0.001$, WSR-test). On top, the average amount of overbidding in our study (77.6%) is larger than what has been reported in many previous studies with repeated contests.[22] We also observe *overspreading*; in contrast to the unique equilibrium in pure strategies, subjects frequently use the entire strategy space (see Figure 6.6 in Appendix B).

Since we elicited subjects' beliefs, we can compare patterns of behavioral responses to beliefs in our static game, using the empirical response functions reported by Rockenbach and Waligora (see Chapter 2) in a repeated partner settings. Our results suggest that reported beliefs and observed behavior are strongly correlated (Spearman's rho 0.624, $p<0.001$). Figure 6.7 in Appendix B depicts average responses to beliefs in the same fashion as presented by Rockenbach and Waligora (see Chapter 2). We find the same linear response function, which contrasts starkly to the theoretic prediction of best replies. The linearity of the response function is also confirmed by the regression analysis in Table 6.3 (see column [II]), which shows a highly significant and strong effect of reported beliefs on contest behavior; the estimated parameter amounts to 0.9.

6.5.4. Efficiency of the rent-seeking

Finally, we investigate how treatment variation influences the efficiency of the contests. We aggregate investments made by both contestants during the entire contest. The average values are depicted in Figure 6.5a. Across all treatments, we observe very strong overbidding as compared to the theoretic prediction under standard preferences. Whereas in the symmetric Nash equilibrium only 50% of the rent is dissipated by the investments, in all of our treatments subjects dissipated on average more than 80% of

[22] For instance, Rockenbach and Waligora (see Chapter 2) report an average overbidding of 29.7% in repeated Tullock contests.

the prize. However, we do not find any treatment effects on the dissipation of rents (all p>0.5, MWU-tests).

Result 6.5: *In the* ENVY *treatment, subjects predict envy-driven competition in the second stage and reduce investments in the first stage (as compared to the* NOENVY *treatment); the total dissipation of rents does not increase due to envy.*

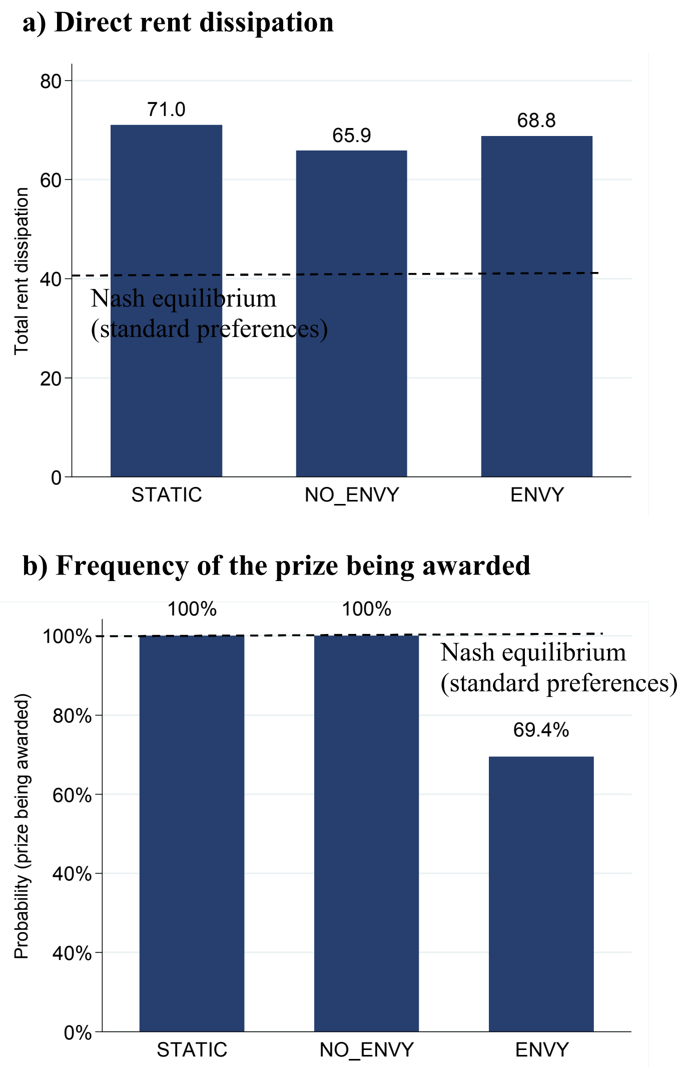


Figure 6.5: Efficiency of contests across treatments.

This result is probably driven by the fact that subjects in the ENVY treatment reduce their investments in the first stage. This reduction is stronger than predicted by the theoretic analysis. However, this is in line with experimental evidence from Mago and Sheremeta (2012), who test behavior in sequential three-battle contests. The authors find that subjects underinvest in the first battle. Although our experimental design differs from Mago and Sheremeta's study in several ways,[23] our results qualitatively suggest the same behavioral pattern of players being *over-cautious* at the beginning of the contest.

As pointed out in Section 6.3.5, rent dissipation is only one of two possible efficiency concerns in the considered set-up. Envy-driven behavior may also impact the likelihood of the prize being awarded. This effect becomes apparent when looking at Figure 6.5b. Whereas in the STATIC and NOENVY treatments the prize is always awarded, envy-driven investments in the ENVY treatment lead to the prize not being awarded in 30 percent of the cases. Depending on the contest, this may lead to a significant social loss.

Result 6.6: *Due to envy-driven behavior in the ENVY treatment, the prize is not awarded in 30 percent of the cases.*

6.6. Conclusion

In this paper we provide evidence that envy is a significant and strong driver of behavior in dynamic contests and often prevents the prospective loser from surrendering. As a consequence, the amount of resources spent in the second battle increases, as compared to the theoretic benchmark. Such behavior has an impact on the efficiency of the contest. We observe that total dissipation of rents does not increase, as players anticipate behavior in the second battle and reduce investments in the first battle more than equilibrium behavior would predict. However, we observe another source of inefficiencies. We find that envy-driven behavior results in the prize/rent not being awarded in 30 percent of the cases.

This study may provide useful insights in the area of patent protection laws. We observe that subjects in our experiment are often determined to prevent the opponent from winning the prize. It is not unsound to presume that similar behavior can be expected from firms competing against each other in markets. If they also engage in fierce patent blocking behavior, this may prevent many products from being commercialized and consequently reduce social welfare substantially.

[23] Most importantly, Mago and Sheremeta (2012) model battles as all-pay auctions.

6.7. Appendix A: Proofs

Proof 1: *In the 'symmetric' equilibrium, if* $0 < \alpha \leq 1$ *the second battle investment of the prospective winner is not smaller than investment of the prospective loser.*

Since we analyze only equilibria in which first battle behavior is symmetric, $X_A \geq X_B$ if and only if $x_{A,2} \geq x_{B,2}$. Assume now by contrast that $x_{A,2} < x_{B,2}$, which implies that $X_A < X_B$. Then, in equilibrium, it must hold from eq. (11b) that $x_{B,2}V = (x_{A,2} + x_{B,2})^2$. Moreover, from eq. (13b) it must hold that $\alpha x_{A,2}V = (1+\alpha)(x_{A,2} + x_{B,2})^2$. From these two conditions it follows that $x_{B,2} = \frac{\alpha}{1+\alpha} x_{A,2}$ as soon as $V \neq 0$. However, if $0 < \alpha \leq 1$ the expression $\frac{\alpha}{1+\alpha} < 1$, which implies that $x_{A,2} > x_{B,2}$. We obtain a contradiction. ■

6.8. Appendix B: Additional figures

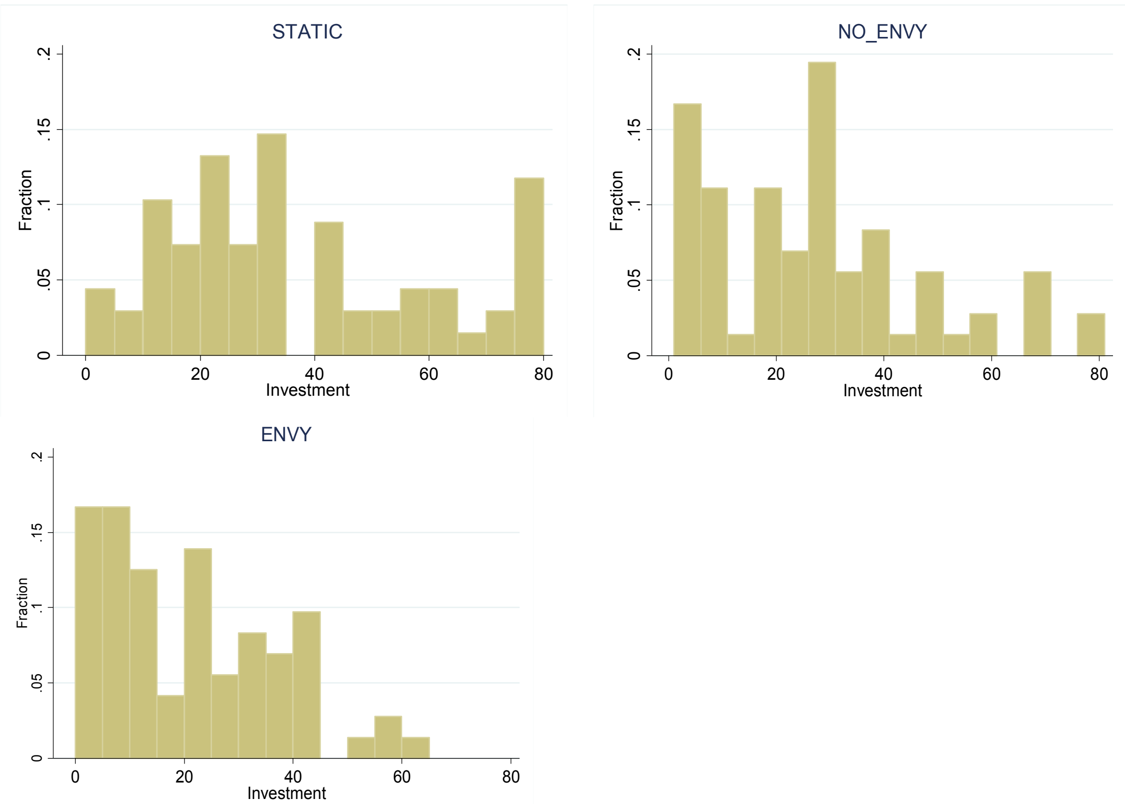


Figure 6.6: Distribution of investments in the first stage.

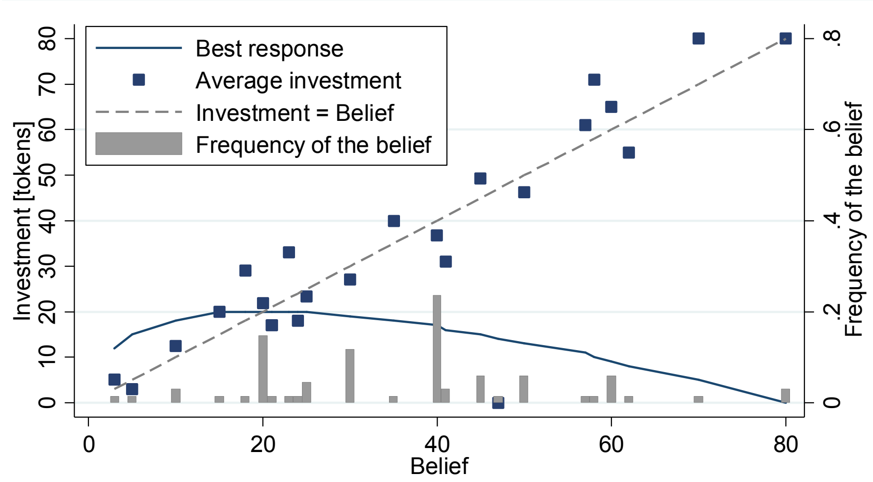


Figure 6.7: Average responses to beliefs in treatment STATIC.

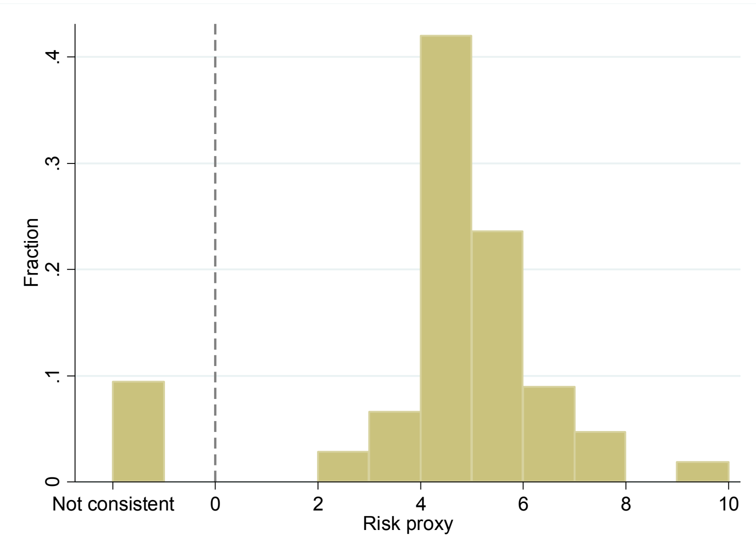


Figure 6.8: Distribution of risk proxy (all three treatments pooled).

6.9. Appendix C: Theoretic prediction under relative payoff maximization

1. Assumption:

Player's expected utility depends on the difference between her own payoff and the weighted payoff of the opponent (see Sheremeta 2015a). Player's utility:

$$U_B(\pi_A, \pi_B) = \pi_B - r\pi_A$$

r is relative payoff parameter (r>0 reflects competitive preferences)

2. Incentive to deviate in the second battle [first battle loser]

Expected payoffs, assuming the equilibrium behavior of the competitor:

$$E[U_B | x_{B,2} = 0] = E - \frac{V}{4} - r\left[E + \frac{3}{4}V\right] = (1-r)E - \frac{V}{4}(1+3r)$$

$$\begin{aligned} E[U_B | x_{B,2} = \epsilon > 0] &= E - \frac{V}{4} - \epsilon - r\left[E - \frac{V}{4}\right] \\ &= (1-r)E - \frac{V}{4}(1-r) - \epsilon \end{aligned}$$

Therefore, there is an incentive to deviate from the equilibrium derived under standard preferences.

3. Equilibrium prediction under relative payoff maximization

We apply backward induction and find second-battle equilibrium behavior. Loser's expected payoff in the second battle is:

$$E[U_B] = E - x_{B,1} - x_{B,2} - r(E - x_{A,1} - x_{A,2} + \frac{x_{A,2}}{x_{A,2} + x_{B,2}}V)$$

First-order condition gives:

$$\frac{\partial E[U_B]}{\partial x_{B,2}} = -1 + r\frac{x_{A,2}}{\left(x_{A,2} + x_{B,2}\right)^2}V = 0$$

Analogously, winner's expected payoff in the second battle is:

$$E[U_A] = E - x_{A,1} - x_{A,2} + \frac{x_{A,2}}{x_{A,2} + x_{B,2}}V - r(E - x_{B,1} - x_{B,2})$$

First order condition gives:

$$\frac{\partial E[U_A]}{\partial x_{A,2}} = -1 + \frac{x_{B,2}}{\left(x_{A,2} + x_{B,2}\right)^2}V = 0$$

Combining the two first-order conditions, we obtain that:

$$r x_{A,2} V = x_{B,2} V$$

Inserting this in the first-order conditions and solving for optimal investment levels gives us the equilibrium prediction for the second battle:

Loser: $x^*_{A,2} = \frac{r}{(1+r)^2} V$

Winner: : $x^*_{B,2} = \frac{r^2}{(1+r)^2} V$

By backward induction, we can find symmetric first battle behavior:

$$x^*_{A,1} = x^*_{B,1} = \frac{U^W - U^L}{4}$$

$$\text{where } U^W = E + \frac{V}{(1+r)^2};\ U^L = E - \frac{r^2 V}{(1+r)^2}$$

Using the derived predictions, we can depict equilibrium behavior as a function of parameter *r* (see Figure 6.9).

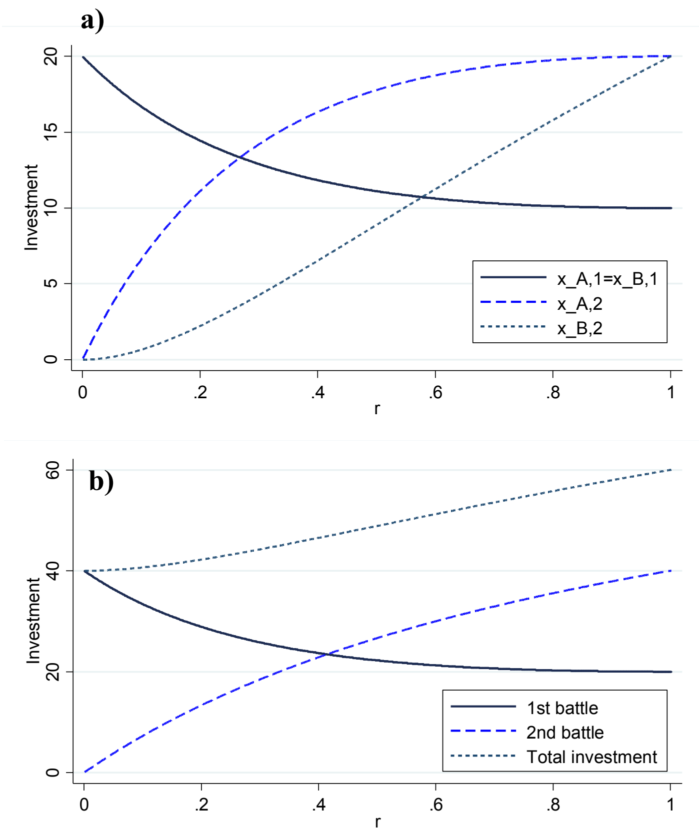


Figure 6.9: Equilibrium prediction under relative payoff maximization (*V*=80). (a) individual investments; (b) total contest expenditures.

6.10. Appendix D: Computer screens and experimental instructions

Figure 6.10: Computer mask in the risk elicitation task. Translation from German.

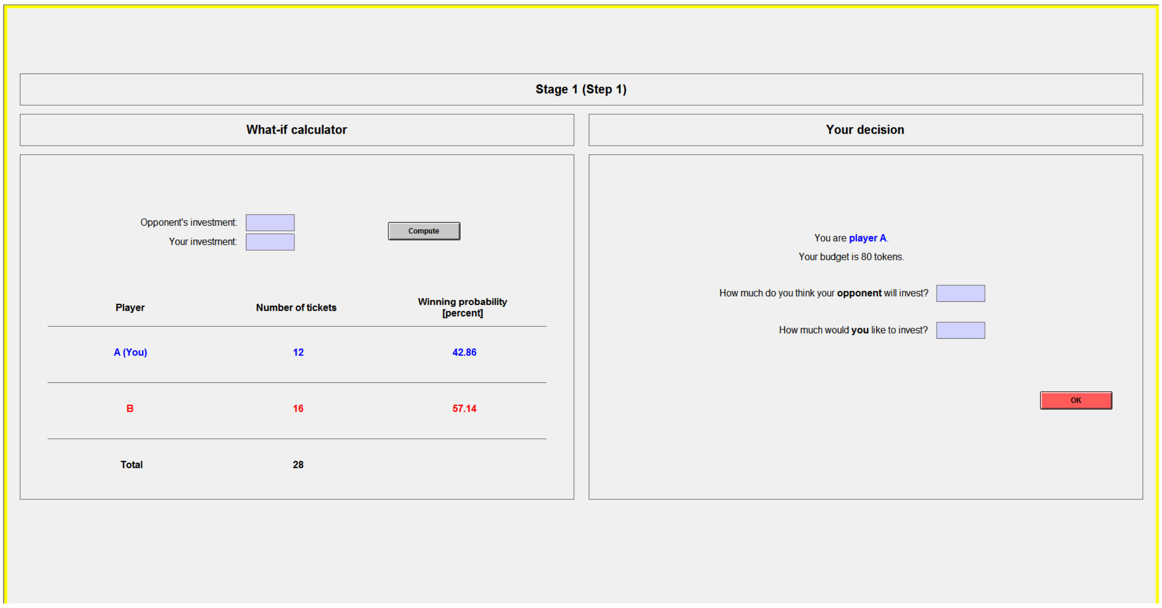


Figure 6.11: Computer mask at the decision stage (treatments ENVY & NOENVY). Translation from German.

Experimental written instructions: ENVY-treatment (translation from German)

General information

Welcome to our experiment! It is very important that you carefully read and understand the following instructions. If you have any questions, please raise your hand. We will then come to you and answer them. Communication with other participants before and during the experiment is not allowed. If you violate this rule, you will have to leave the experiment and will receive no payment.

You can earn money in this experiment. You will receive 4 EUR for participation. You can earn additional money during the experiment. The amount you earn depends on your and other participants' decisions during the experiment. You will be paid in cash at the end of the experiment. Your payoff or identity will not be revealed to other participants.

Today's session consists of two independent experiments that we call Part 1 and Part 2. Your earnings in both will be added up.

Part 1

In Part 1 you will make a number of decisions, for which you can earn money. How much you will earn depends partially on your decisions and partially on luck.

In a moment, you will see a menu of ten scenarios. In each scenario, there is an Option A and an Option B. Please decide which option you prefer.

After you have made all of your decisions, one of the scenarios will be randomly chosen and played out. The corresponding amount of money you earned will be added to your payment account.

Option A is the same in all scenarios. This is a lottery that gives you 2 EUR with 50% probability and 0 EUR with 50 % probability.

Option B differs across scenarios. This is an amount of money you can get with certainty (i.e., 100% probability).

Which scenario was chosen and played out will be revealed to you after you have finished Part 2 of today's experiment.

<SCREEN 1: see Figure 6.10>

Part 2

Your earnings in this part are denoted in tokens. These will be converted into EUR and added to your account. The exchange rate is:

10 tokens = 1 EUR

In this part of the experiment you will interact with another participant. You will be randomly matched with an opponent in a minute. In each pair, there will be player A (marked blue) and player B (marked red). These roles will be assigned randomly. Information on whether you are player A or player B will appear on the screen before the game begins.

You and your opponent will compete in a two-stage game for a prize. Each stage is a lottery. Both stages (i.e., lotteries) proceed in the same way. Only the player who wins both stages wins the prize of **80 tokens.**

For the entire game, you have an endowment of **80 tokens.**

In each stage, you decide on how many points you want to spend on lottery tickets. One ticket costs 1 token (1 token = 1 ticket). You can purchase as many tickets as you wish. However, you are not allowed to exceed your budget of 80 tokens. In stage 2, you can only use tokens you have not spent in stage 1. The tokens you did not spend in either stage are added to your account.

Your probability of winning in a particular stage depends on how many tickets you have bought in this stage and how many your opponent has bought. The more tickets you bought, the more likely it is that you win the stage. Similarly, the more tickets your opponent bought, the less likely it is that you win the stage. The probability that you will win the stage is equal to the number of your tickets divided by the number of all tickets bought. This means in each stage the probability of winning is computed as follows:

$$\text{Your probability of winning} = \frac{\text{Number of your tickets}}{\text{Number of your tickets} + \text{Number of tickets of the opponent}}$$

If only one player has bought tickets, she/he wins the stage with certainty. If neither of the players bought any tickets, the lottery does not take place and nobody wins the prize.

The prize of 80 tokens is awarded only to the player who wins both stages.

Your earnings are as follows:

Your earnings if you win the prize = 80 – your investment in tickets in both stages + 80

Your earnings if you lose = 80 – your investment in tickets in both stages

Each stage consists of three steps:

Step 1

In the first step, you decide how many tickets you would like to purchase. At the same time your opponent makes the same decision.

While you make your decision, you can use a what-if-calculator. You can insert hypothetical investments for your opponent and yourself, and the calculator computes the probabilities of winning for you and your opponent, according to the formula above.

You can use the calculator as often as you want. Please just insert in the red field (see red field (1) in Figure 1) the opponent's investment and click on "Compute."

You should insert your final decision in the field on the right-hand side of the screen and confirm with the button "OK" (see red field (2) in Figure 1). You are not only asked about your investment, but also about what investment you expect from your opponent. For this, you can earn a bonus of up to **5 tokens**. The amount of the bonus depends on how accurate your prediction is. The smaller the deviation of your prediction, the higher your bonus is. The bonus is computed in the following way.

$$\text{Bonus} = 5 - 0.05 * \text{Deviation}^2$$

The deviation is the difference between your prediction and the actual opponent's investment:

$$\text{Deviation} = |\text{Your prediction} - \text{actual opponent's investment}|$$

It is not important that you understand the formula well. In general, it holds that the better you guess the behavior of the opponent, the higher your bonus is. If the mistake is larger than 10, the bonus is 0. This means there is no negative bonus; you cannot lose any tokens for your prediction. Your bonuses from both stages will be added to your account.

Step 2

In the second step, you get feedback on your opponent's decision (how many tickets she/he has bought). Probabilities of winning are also computed and displayed. All tickets bought are numbered. You get information on which numbers correspond to your tickets and which to tickets of your opponent. In order to make it clearer, this is also displayed graphically.

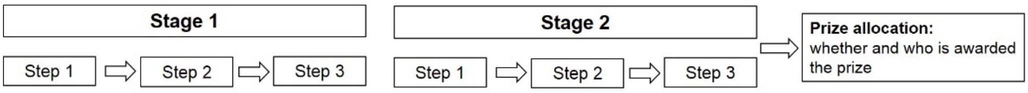
Each ticket is equally likely to be drawn.

You also receive feedback on how accurate your prediction about the opponent's investment was and how large your bonus is.

Step 3

In the last step, the winning ticket is drawn. The computer draws one of the tickets that were purchased. The number of the winning ticket and the winner are displayed on the screen.

To sum up, the game goes as follows:



Before the game starts, we would like you to answer three test questions in order to make sure that you have understood the rules. They appear on the screen as first.

Figure 6.12: Figure 1 from the written instructions. Translation from German.

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