

# Contests with group size uncertainty: Experimental evidence

Luke Boosey\* Philip Brookins<sup>†</sup> Dmitry Ryvkin<sup>‡</sup>

## Abstract

In many contest situations, the number of participants is not observable at the time of investment. We design a laboratory experiment to study individual behavior in Tullock (lottery) contests with group size uncertainty. There is a fixed pool of  $n$  potential players, each with independent probability  $q \in (0, 1]$  of participating. As shown by Lim and Matros (2009; *Games and Economic Behavior*, vol. 67, pp. 584–597), the unique symmetric equilibrium investment level in this setting can exhibit nonmonotonicity with respect to both  $n$  and  $q$ . We independently manipulate each of the parameters and test the implied comparative statics predictions. Our results provide considerable support for the theory, both in terms of comparative statics and point predictions. Most surprisingly, we find no evidence of overbidding in treatments where there is a nontrivial probability that group size is one. This stands in stark contrast to the robust overbidding observed in experimental contests with deterministic group size. We propose a one-parameter model that incorporates nonlinear probability weighting and a modified version of joy of winning, which we call *Constant Winning Aspirations* (CWA), and show that it neatly organizes all of our results. The CWA model applies to a broad range of contexts and may be used to explain existing evidence on the differences in overbidding across many other contest and auction experiments.

**Keywords:** contest, group size uncertainty, experiment, overbidding, probability weighting, joy of winning, constant winning aspirations

**JEL classification codes:** C72, C91, D72, D82

---

\*Department of Economics, Florida State University, Tallahassee, FL 32306-2180, USA; E-mail: lboosey@fsu.edu

<sup>†</sup>Max Planck Institute for Research on Collective Goods, Kurt-Schumacher-Str. 10, D-53113, Bonn, Germany; E-mail: brookins@coll.mpg.de

<sup>‡</sup>Department of Economics, Florida State University, Tallahassee, FL 32306-2180, USA; E-mail: dryvkin@fsu.edu

# 1 Introduction

In contests, such as R&D races, political campaigns, lobbying, litigation, or job market tournaments, participants spend resources to secure a valuable prize.<sup>1</sup> While in most contest models it is assumed that the number of contestants is common knowledge, it is often the case that at the time a contestant has to choose her investment, she is not aware of how many other competitors she will face. Thus, in modeling the behavior of agents involved in contests, group size uncertainty can be both an appealing and realistic feature.

There are, in general, two ways to think about the emergence of uncertainty about the number of contestants. On the one hand, entry into contests may be endogenous, with each potential contestant deciding simultaneously, and privately, whether or not to participate. Typically, a model with endogenous entry will have a symmetric equilibrium in mixed strategies, which induces uncertainty about the realized size of the group. The equilibrium entry probability,  $q^*$ , can be manipulated exogenously by varying the parameters of the environment, such as the value of the prize, outside option or entry fee. Changes in  $q^*$  induced in this manner will in turn lead to changes in the equilibrium contest investment. Thus, one might think of the underlying group size uncertainty as being driven by the decisions of a third party, such as a contest organizer, industry regulator, or governing body. For example, an R&D firm may only be able to participate in a contest if particular quality control or safety protocols are satisfied, regulatory barriers are removed, or certain legislation is passed.<sup>2</sup> In these settings, it is important to understand how the resulting contest investment changes in response to a change in external parameters (e.g., regulation), which requires one to understand the comparative statics of equilibrium investment with respect to  $q^*$ .

On the other hand, a somewhat simpler and more direct approach is to bypass the entry decision entirely and assume that group size uncertainty is exogenous. One possibility is to assume, as in [Lim and Matros \(2009\)](#), that there is a fixed number  $n$  of potential contestants and, from the perspective of an active contest participant, the number of other active participants is distributed as Binomial( $n - 1, q$ ), with some underlying probability of participation  $q$ . By studying the comparative statics of equilibrium investment with respect to  $q$  and  $n$ , one can gain valuable insight into the effects of group size uncertainty in contests in isolation from the entry decision. Indeed, in any symmetric equilibrium of a model with entry, an active participant forms beliefs about others' participation probability and consequently about the distribution of actual contest size. In a setting with

---

<sup>1</sup>For a recent survey of the theoretical literature on contests, see, e.g., [Konrad \(2009\)](#), [Congleton, Hillman and Konrad \(2008\)](#), [Corchón \(2007\)](#), [Connelly et al. \(2014\)](#); for a summary of some nonexperimental empirical results in personnel economics and sports, see, e.g., [Prendergast \(1999\)](#) and [Szymanski \(2003\)](#); for a survey of the experimental literature on contests, see [Dechenaux, Kovenock and Sheremeta \(2015\)](#).

<sup>2</sup>We are grateful to an anonymous referee for suggesting regulation as a source of exogenous variation in the participation probability. In addition to R&D competition, other examples of contest environments where entry probability, and hence the underlying group size uncertainty, may be manipulated by third parties include lobbyists competing in a rent-seeking contest or job candidates applying for a promotion or new position.

exogenous group size uncertainty, these beliefs can be manipulated directly, but the resulting comparative statics are essential in understanding the effects of external factors in an environment with endogenous entry as well.

In this paper, we use a laboratory experiment to study the behavior of individuals in a contest with group size uncertainty. In particular, we test two theoretical predictions about individual equilibrium investment derived by [Lim and Matros \(2009\)](#) for [Tullock \(1980\)](#) lottery contests with a stochastic number of players following the binomial distribution with parameters  $(n, q)$ . There is a unique symmetric equilibrium that exhibits two key features. First, for any fixed number of potential players  $n > 2$ , individual equilibrium investment is single-peaked in the probability of participation  $q$ . Second, for any two values of  $n$ , the individual equilibrium investment functions satisfy a single-crossing property; as a result, for different values of  $q$ , increasing the number of potential players can have opposite effects on individual investment. The intuition for this reversal is as follows. When  $q$  is small and  $n$  is low, the modal group size is one (i.e., a player in a group by herself) and the equilibrium investment is very low. As  $n$  increases, group sizes larger than one become more likely and the equilibrium investment goes up. In contrast, when  $q$  is large, the group size is almost certainly greater than one, and hence an increase in  $n$  has the same effect on individual equilibrium investment as in standard contests where the number of players is known, i.e., the investment goes down.

In our experiment, we use a  $2 \times 2$  between- and within-subject hybrid design to test these comparative statics. In the resulting four treatments, we independently manipulate the maximal number of bidders ( $n = 3$  and  $n = 6$ ) and the participation probability ( $q = 0.2$  and  $q = 0.8$ ). The parameters are selected so that individual equilibrium investment is increasing in  $n$  for the low value of  $q$  and decreasing in  $n$  for the high value of  $q$ . As a robustness check, we also vary the participation probability within subjects for each  $n$ , accounting for possible order effects.

We make three key contributions in this paper. First, to the best of our knowledge, this is the first experimental study to explore the comparative statics of behavior in contests with group size uncertainty. There is a well-developed theoretical literature on contests with a stochastic number of participants ([Münster, 2006](#); [Myerson and Wärneryd, 2006](#); [Lim and Matros, 2009](#); [Fu, Jiao and Lu, 2011](#); [Kahana and Klunover, 2015, 2016](#); [Ryvkin and Drugov, 2017](#)), including contests with endogenous entry ([Fu and Lu, 2010](#)). Our study is also related to that of [Morgan, Orzen and Sefton \(2012\)](#), who explored endogenous entry in contests experimentally. However, [Morgan, Orzen and Sefton \(2012\)](#) model entry as a sequential process, and there is no group size uncertainty at the time when subjects make their investment decisions. Games with population uncertainty have also been studied in other environments, including auctions,<sup>3</sup> as well as voting, public

---

<sup>3</sup>For a theoretical analysis of auctions with a stochastic number of bidders see, e.g., [McAfee and McMillan \(1987\)](#), [Harstad, Kagel and Levin \(1990\)](#) and [Levin and Ozdenoren \(2004\)](#). For a theoretical analysis of endogenous entry in auctions see, e.g., [Levin and Smith \(1994\)](#) and [Pevnitskaya \(2004\)](#). Experiments on auctions with a stochastic number of bidders and endogenous entry include [Dyer, Kagel and Levin \(1989\)](#), [Isaac, Pevnitskaya and Schnier \(2012\)](#) and [Palfrey and Pevnitskaya \(2008\)](#).

goods, and coordination game settings modeled as Poisson games.<sup>4</sup>

The second key contribution of our study is methodological. Specifically, our experimental design has two important novel features. The first is that instead of informing subjects about the underlying participation probability,  $q$ , we inform them directly about the probabilities,  $q_m$ , of different realizations of random group size  $m \in \{1, \dots, n\}$ . We do this for a couple of reasons. On the one hand, we believe this information is easier for subjects to understand. On the other hand, and arguably more importantly, we feel that this approach is also more “ecologically valid,” in the sense that people in the field are more likely to think about situations with population uncertainty in terms of probabilities of possible outcomes (group sizes) as opposed to the underlying stochastic process (which may be unknown). In this respect, our results are not dependent on subjects’ abilities to correctly calculate binomial outcome probabilities, nor do they depend on the precise underlying stochastic process used to generate these probabilities. The second novel feature of our design is that, even though subjects participate in games with random group sizes, we draw the relevant groups and determine the probability of winning independently for each subject. This approach allows us to maximize the number of observations and avoid issues with uneven grouping and subjects sitting out, while retaining the same incentive structure as in other ways of implementing contest experiments.

Our third, and most important, contribution is substantive. Overall, we find remarkable agreement between the theory and our experimental data, especially in terms of the point predictions. With the exception of the treatment where  $n = 6$  and  $q = 0.8$ , average observed investment is extremely close to the risk-neutral Nash equilibrium levels, in stark contrast with the literature on lottery contests with deterministic group size, where overbidding has been widely documented (Sheremeta, 2013, 2016). Furthermore, in an additional control treatment where groups of size one cannot occur, we find that overbidding is restored. Thus, comparing the treatment conditions where overbidding occurs with those where it does not suggests a link between the absence of overbidding and the possibility that realized group size equals one and conflict is dissolved.

We propose a unifying explanation for these results based on a combination of non-linear probability weighting and a modified version of joy of winning. Specifically, we argue that individuals use decision weights, consistent with the probability weighting feature of Cumulative Prospect Theory (Tversky and Kahneman, 1992), when faced with group size uncertainty. As such, subjects place higher weight on the payoff-maximizing possibility that they are the only participant in the contest; that is, they overweigh the probability that the realized group size is one. At the same time, there is considerable evidence to suggest that individuals derive some nonmonetary utility from winning (i.e., joy of winning), which can induce an increase in the contest investment (e.g., Goeree, Holt and Palfrey, 2002; Sheremeta, 2010; Brookins and Ryvkin, 2014). We provide a simple modification to the standard joy of winning model, which we call the *Constant Winning Aspirations* (CWA) hypothesis, that allows us to explain differences in overbidding between treatments in our experiment. The key innovation in the CWA approach is to allow

---

<sup>4</sup>See, e.g., Myerson (1998, 2000); Makris (2008, 2009); De Sinopoli and Pimienta (2009); Mohlin, Östling and Wang (2015); Ioannou and Makris (2017).

the joy of winning effect to vary with the equilibrium probability of winning in such a way that the expected value of winning remains constant.

Thus, under our proposed model, bidding is influenced by potentially conflicting motives. On the one hand, the joy of winning, captured by our CWA hypothesis, induces higher investment; on the other hand, the possibility that conflict is dissolved (group size equals one), with an extreme gain from not wasting any resources, is overweighed in line with Cumulative Prospect Theory, and induces lower investment. We show that the different effects of probability weighting across treatments can explain, at least qualitatively, the differences in overbidding across treatments. Moreover, under our CWA model, the effects from joy of winning on overbidding are also different across treatments. Together, our one-parameter unified model is able to fully explain the differences in overbidding observed across treatments in our experiment.<sup>5</sup>

Additionally, unlike the standard way of modeling joy of winning, our CWA hypothesis can also be used to explain differences in overbidding *across games* in various contest environments. For instance, existing experimental evidence on the effect of group size in Tullock contests finds either no systematic variation in average investment, or else a smaller reduction in average investment than is predicted by theory (e.g., [Anderson and Stafford, 2003](#); [Morgan, Orzen and Sefton, 2012](#); [Lim, Matros and Turocy, 2014](#); [Baik et al., 2015](#)). Thus, empirical evidence suggests that overbidding increases with group size, while the standard joy of winning approach predicts it to be decreasing. In contrast, under the CWA hypothesis, the effect from joy of winning depends on group size through the equilibrium probability of winning, such that, consistent with the empirical evidence, overbidding is predicted to increase with group size.

The rest of the paper is organized as follows. In [Section 2](#), we present the theoretical model and predictions for the parameters used in the experiment. [Section 3](#) presents the experimental design and procedures. The results of the experiment are presented in [Section 4](#). Then, in [Section 5](#), we propose and develop the model incorporating probability weighting and Constant Winning Aspirations to explain the differences in overbidding observed across treatments. [Section 6](#) provides concluding remarks.

## 2 Theoretical model and predictions

In this section, we summarize the results of [Lim and Matros \(2009\)](#) that are relevant for our experiment. Consider a game of  $n$  identical risk-neutral players indexed by  $i \in N = \{1, \dots, n\}$ , each of whom participates in a contest with independent probability  $q \in (0, 1]$ . Assuming player  $i$  participates in the contest, let  $M \subseteq N \setminus \{i\}$  denote the (random) set of participating players other than  $i$ . The participating players  $\{i\} \cup M$  compete by spending

---

<sup>5</sup>[Parco, Rapoport and Amaldoss \(2005\)](#) combine nonmonetary utility of winning with probability weighting to explain behavior in a two-stage contest with budget-constrained players. However, they do not propose the feature that equilibrium expected utility of winning remains constant across settings, and the validity of their parameterization for the joy of winning is not tested across different group sizes. Furthermore, the probability weighting in their model applies to the probability of winning in the contest success function rather than to the probabilities of different group sizes.

$(x_i, x_M) \in \mathcal{R}_+ \times \mathcal{R}_+^{|M|}$ , where  $x_i$  is the investment level of player  $i$  and  $x_M$  is the vector of investments of all other participating players. The probability of player  $i$  winning is given by the [Tullock \(1980\)](#) lottery contest success function

$$P_i(x_i, x_M; M) = \begin{cases} \frac{1}{|M|+1}, & \text{if } x_i = 0 \text{ and } x_j = 0 \text{ for all } j \in M, \\ \frac{x_i}{x_i + \sum_{j \in M} x_j}, & \text{otherwise.} \end{cases} \quad (1)$$

The winner of the contest receives a prize  $V > 0$ . Thus, the expected payoff of a participating player  $i$  given the realization of  $M$  and investments  $(x_i, x_M)$  is  $VP_i(x_i, x_M; M) - x_i$ .

[Lim and Matros \(2009\)](#) have shown that this game has a unique symmetric pure-strategy Nash equilibrium (NE), with individual investments given by

$$x^*(n, q) = V \sum_{i=0}^{n-1} \binom{n-1}{i} q^i (1-q)^{n-i-1} \frac{i}{(i+1)^2}. \quad (2)$$

Equation (2) implies nontrivial comparative statics of individual equilibrium investment with respect to the participation probability,  $q$ , and the maximal group size,  $n$ . Specifically, as shown by [Lim and Matros \(2009\)](#), for a given  $n$ ,  $x^*(n, q)$  is nonmonotonic in  $q$ , with a single peak  $\hat{q}(n) \in (0, 1)$  for  $n \geq 3$ . Additionally,  $x^*(n, q)$  satisfies the single-crossing property, i.e., for any two maximal group sizes  $n$  and  $n'$  there is a unique  $q \in (0, 1]$  such that the individual equilibrium investments are equal,  $x^*(n, q) = x^*(n', q)$ .

These results lead to a possibility that the direction of the effect of maximal group size  $n$  on  $x^*(n, q)$  changes depending on the value of  $q$ . Specifically, for a relatively low  $q$ , individual investment  $x^*$  may be increasing in  $n$ , whereas for a relatively high  $q$  it may be decreasing in  $n$ . The intuition for this reversal is as follows. When  $q$  is low, smaller group sizes are more likely; in particular, the modal group size may be the group of one, i.e., an active player in a group by herself, in which case the optimal investment is zero. As  $n$  increases, the probability of larger group sizes (two and higher) also increases, leading to an increase in the optimal investment. On the contrary, when  $q$  is high, the number of participating players is likely to be large, and an increase in  $n$  has an effect on individual investment that is similar to the effect of an increase in group size in a standard contest with deterministic group size; that is,  $x^*$  will decrease with  $n$ .

In the experiment, we set out to test these comparative statics using a  $2 \times 2$  design. We vary the maximal group size between  $n = 3$  and  $n = 6$  and the participation probability between  $q = 0.2$  and  $q = 0.8$ . All amounts in the experiment are denominated in points, with the prize set at  $V = 120$ . Based on Eq. (2), the equilibrium individual investments under these parameters are

$$\begin{aligned} x^*(3, 0.2) &= 10.67, & x^*(3, 0.8) &= 26.66, \\ x^*(6, 0.2) &= 19.03, & x^*(6, 0.8) &= 19.49. \end{aligned} \quad (3)$$

The main comparison we are interested in is the difference in the effect of increasing the maximal group size  $n$  for different values of  $q$ . As seen from the predictions (3), there

is indeed a reversal of the effect of  $n$  on  $x^*$  for a high  $q$  as compared to a low  $q$ . When  $q = 0.2$ , individual equilibrium investment is higher in the larger population ( $n = 6$ ) than in the smaller population ( $n = 3$ ). On the other hand, when  $q = 0.8$ , equilibrium investment is lower in the larger population than in the smaller population. In addition, we examine the comparative statics with respect to  $q$ . That is, for a fixed maximal group size, we compare individual investment under the low ( $q = 0.2$ ) and high ( $q = 0.8$ ) values of  $q$ . As seen from (3),  $x^*$  increases in  $q$  when  $n = 3$  but practically does not change with  $q$  when  $n = 6$ .

### 3 Experimental design

All experimental sessions were conducted using z-Tree (Fischbacher, 2007), with subjects making decisions at visually separated computer terminals at the XS/FS laboratory at Florida State University. A total of 216 subjects (57.4% of them female) were randomly recruited via ORSEE (Greiner, 2015) from a subpopulation of FSU undergraduate students who pre-registered to receive announcements about participation in upcoming experiments. Ten sessions were conducted, and each subject participated in only one session. Each session consisted of four parts. Instructions were distributed and read aloud prior to the start of each part. The instructions for the main portion of the experiment are provided in Appendix B.<sup>6</sup> A session lasted approximately 70 minutes, with subjects earning \$25.53, on average, including a \$7.00 show-up fee.<sup>7</sup>

In Part 1 of the experiment, we measured subjects’ attitudes towards risk, ambiguity, and losses. Each of these attitudes was elicited using a “list-style” environment similar to the methods used by Holt and Laury (2002) and Sutter et al. (2013).<sup>8</sup> The lists for the three measures were presented to subjects in a random order. One of the lists, and one of the rows in that list, were selected randomly for actual payments. If a subject’s choice in that row were the sure amount of money, that amount was paid; if the choice were a lottery, the outcome was randomly realized. The results and payoffs from this part were withheld until the end of the experiment.

---

<sup>6</sup>All other instructions not reproduced in Appendix B are straightforward and available from the authors upon request.

<sup>7</sup>Each subject’s earnings are the show-up fee plus the sum of their earnings from the four parts of the experiment. We describe the way that earnings were calculated for each part in the explanation of the procedures.

<sup>8</sup>In each case, subjects were presented with a list of 20 choices between a sure amount of money and a gamble with two outcomes. The sure amounts of money changed gradually from the top to the bottom of the list. Subjects were asked to select a row where they were willing to switch from preferring a gamble to preferring a sure amount. In the risk list, the gamble was a lottery (0, \$2; 0.5, 0.5) and sure amounts changed between \$0.10 and \$2; in the ambiguity list, the same sure amounts were used but the gamble was a lottery (0, \$2;  $p, 1 - p$ ), with a uniform random  $p$  drawn from  $[0, 1]$  and not disclosed to subjects; in the loss list, the gamble was a lottery (0,  $-\$2$ ; 0.5, 0.5) and sure amounts changed between  $-\$2$  and  $-\$0.10$ . Our measures for risk aversion (RA) and loss aversion (LA) were constructed using the row numbers where subjects switched. The measure for ambiguity aversion (AA) was constructed as the difference between the row numbers where the subject switched in the ambiguous and risky lists.

Treatment	$n$	$q$ (Part 2)	$q$ (Part 3)	Sessions	Subjects	Groups
3LH	3	0.2	0.8	2	48	8
3HL	3	0.8	0.2	2	42	7
6LH	6	0.2	0.8	2	42	7
6HL	6	0.8	0.2	2	42	7
3HL <sup>+</sup>	3+1	0.8	0.2	2	42	7

Table 1: Summary of experimental treatments.

The main parts of our experiment (Part 2 and Part 3) were designed to test the comparative statics of individual equilibrium investment with respect to both maximal group size,  $n$ , and participation probability,  $q$ , cf. (3). We implemented a  $2 \times 2$  between- and within-subject hybrid design with four treatments. In any given session, we kept the maximal group size fixed at  $n = 3$  or  $n = 6$ . In Part 2 of the experiment, all subjects participated in a contest with a maximal group size of  $n$  (fixed for the session), and with a fixed participation probability (either  $q = 0.2$  or  $q = 0.8$ ). Then, in Part 3, we changed the participation probability within each session; that is, in sessions where subjects experienced  $q = 0.2$  (respectively,  $q = 0.8$ ) in Part 2, all subjects in the session then faced  $q = 0.8$  (respectively,  $q = 0.2$ ) in Part 3.<sup>9</sup> Comparisons of behavior in Part 2 across treatments allow us to test the comparative static predictions between subjects. Similar between-subjects comparisons using behavior in Part 3 provide a useful robustness test for our results. However, subjects in Part 3 have also experienced different treatments in Part 2. Thus, we concentrate our analysis on the comparison between treatments in Part 2. Similarly, although we do not report them, comparisons between Part 2 and Part 3 are designed to provide within-subject reactions to changes in  $q$  for each  $n$ , accounting for possible order effects. The resulting four treatments are referred to as  $n$ LH and  $n$ HL, depending on  $n$  and on whether (L)ow ( $q = 0.2$ ) or (H)igh ( $q = 0.8$ ) participation probability was used in Part 2 and Part 3, respectively. The parameters of the treatments are summarized in Table 1. Within each of the main parts of the experiment (Part 2 and Part 3), we denote the relevant treatment conditions by 3Low, 3High, 6Low, and 6High.

In addition to our four main treatments, we also conducted a control treatment, called 3HL<sup>+</sup>, where the possible group sizes were  $m \in \{2, 3, 4\}$ . Otherwise, procedures were the same as in all other treatments. In this treatment, the players were informed that there would always be at least one other player in their group. To facilitate the comparison with the other treatments, we kept fixed the underlying stochastic process for the number of additional players. Thus, the distribution over group sizes was the same as for  $n = 3$ , with the support of the distribution shifted up by 1. In this control treatment, subjects faced the high participation probability ( $q = 0.8$ ) in Part 2, and the low participation probability ( $q = 0.2$ ) in Part 3. We refer to the treatment condition implemented in Part 2 as 3<sup>+</sup>High and the treatment condition implemented in Part 3 as 3<sup>+</sup>Low. The

<sup>9</sup>When subjects were making decisions in Part 2 they were not yet informed about the existence of or instructions for Part 3.



Group Size $m$	Treatment					
	3Low	3High	6Low	6High	3 <sup>+</sup> Low	3 <sup>+</sup> High
1	0.64	0.04	0.32768	0.00032		
2	0.32	0.32	0.4096	0.0064	0.64	0.04
3	0.04	0.64	0.2048	0.0512	0.32	0.32
4			0.0512	0.2048	0.04	0.64
5			0.0064	0.4096		
6			0.00032	0.32768		

Table 2: Probabilities,  $q_m$ , for each possible group size  $m$  for the different treatment conditions.

purpose of this additional control treatment was to explore our proposed explanation for the absence of average overbidding observed in our main treatments. Further details are provided in Section 5.

In Part 2, subjects participated in 30 rounds of the contest game with random group size described in Section 2. Before the first round in all treatments, subjects were randomly placed into matching groups consisting of six subjects each. Matching groups were fixed for the duration of the experiment, and interactions between subjects were confined within the matching groups. In each round, subjects were given an endowment of 120 points and asked to choose how many points to invest into a project. Any points not invested were retained as part of their payoff for the round.

At the time of making their investment decisions, subjects were not informed about the actual size of their group but only knew the probabilities for each possible group size to occur. That is, instead of providing subjects with the participation probability  $q$ , we gave them the resulting probabilities,  $q_m$ , for each possible group size  $m$ . In the main treatments  $nLH$  and  $nHL$ ,  $q_m = \binom{n-1}{m-1} q^{m-1} (1-q)^{n-m}$ ,  $m = 1, \dots, n$ , whereas in treatment  $3HL^+$  where the support of the distribution is shifted by 1,  $q_m = \binom{n-2}{m-2} q^{m-2} (1-q)^{n-m}$ , with  $n = 4$  and  $m = 2, 3, 4$ . These probabilities are shown in Table 2 for each of the treatment conditions. Following the investment decisions, each subject received their own independent group size realization,  $m$ , drawn according to the relevant probabilities. Then,  $m - 1$  other subjects were randomly selected from the subject's matching group to form their contest group for the round.

A subject's project could either succeed or fail, with success given by the lottery contest success function (1). If the project was successful (respectively, failed) the subject received 120 (respectively, 0) points in revenue for the round. Therefore, a subject who invested  $x$  received  $240 - x$  (respectively,  $120 - x$ ) points for the round if her project was successful (respectively, failed). After the investment decisions were made, subjects were shown the realized size of their contest group, their own investment, the investments of all other contest group members (if any), the probability that their project was successful, the outcome, and their payoff. At the end of Part 2, the payoffs from five randomly selected rounds counted towards final earnings at the exchange rate of  $\$1 = 100$  points.

Earnings from this and other parts of the experiment were not disclosed until the end of the experiment.

Part 3 of the experiment was *identical* to Part 2, with the exception that subjects now experienced a new set of probabilities for the different sizes of their group (see Table 2), due to the change in the participation probability  $q$ . Subjects remained in the same matching groups as in Part 2. Earnings from Part 3 were obtained using the same procedure as in Part 2 (adding together the payoffs from five randomly selected rounds in Part 3) and added to the final earnings.

In Part 4, we introduced a control task to measure subjects' behavior when strategic uncertainty about others' investment decisions is removed. Subjects were asked to make two decisions based on the information about others' investments in selected rounds of Parts 2 and 3. We do not use the results from Part 4 in the analysis below and, therefore, omit the specifics. Details are available from the authors upon request.

Finally, at the conclusion of the experiment, subjects were informed about their earnings in all four parts and paid privately by check the sum of their earnings from all parts and show-up fee.

As mentioned in the Introduction, our implementation of the contest with random group size incorporated two novel features. First, by providing subjects with the probabilities,  $q_m$ , for each possible group size, rather than the participation probability  $q$ , we greatly simplified the decision making environment. With this feature, our experimental test of the theory does not rely on the subjects' ability to correctly calculate the likelihood of different group sizes for given  $n$  and  $q$ . Furthermore, we view this information as more representative of the way contest participants think about group size uncertainty in practice; i.e., in terms of the likelihood of various outcomes rather than the underlying stochastic process.

Second, we allowed subjects to have their own independent group size realization in each round. That is, for a subject with randomly determined group size  $m$ , we used the investments from  $m - 1$  other participants, drawn randomly from their matching group without replacement, to calculate their probability of success in the current round.<sup>10</sup> As a result, all subjects were active in each round, and therefore, any issues involved with matching subjects to different group sizes and having some of them sitting out were avoided.

## 4 Results

We organize the results as follows. First, in Section 4.1, we report summary statistics and examine the comparative static predictions described in Section 2 using only between-subject comparisons of the data from the first 30 rounds of contests (Part 2 of the experiment). We also compare average investments with the NE point predictions for each treatment. To check the robustness of our results, we also conducted the same

---

<sup>10</sup>Note that, in this way, player  $i$ 's payoff is unaffected by the payoff calculation for another player  $j$ , even if  $j$ 's investment was used to determine player  $i$ 's probability of success.

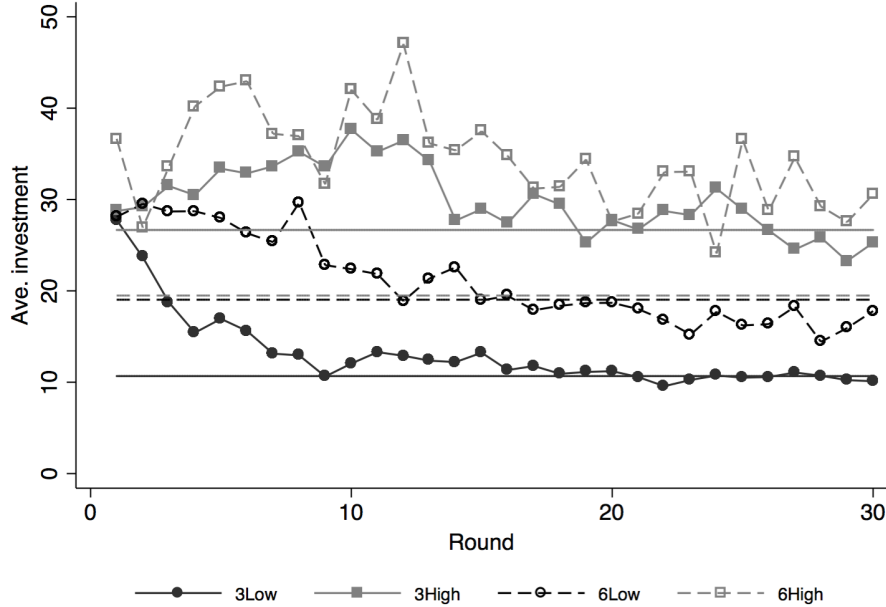


Figure 1: Average observed investment and NE predictions in Part 2 (rounds 1-30), by treatment.

between-subject analysis using the data from the second 30 rounds of contests (Part 3 of the experiment), although the results are relegated to Section A.1 in the Appendix.<sup>11</sup> In Section 4.2, we analyze the dynamics of individual behavior in Part 2 over time, controlling for feedback and various individual characteristics. In Section 4.3, we report the results from our additional control treatment 3HL<sup>+</sup>. Throughout this Section, all standard errors are clustered at the matching group level and matching groups are used as the unit of analysis for nonparametric tests.

#### 4.1 Average investment and comparative statics

Since we confine our analysis to the data from Part 2, we will refer to treatments by the relevant treatment condition used in Part 2 only. Thus, we identify treatments  $nLH$  as  $nLow$  and to treatments  $nHL$  as  $nHigh$ , for each value of  $n \in \{3, 6\}$ . Figure 1 shows the average individual investment across all 30 rounds for each treatment condition, along with the corresponding NE predictions. As seen from the figure, in all treatments except 6High, the average investment converges closely to the NE predictions by round 15.

Table 3 reports the average individual investments by treatment over all 30 rounds as well as over rounds 16-30, with robust standard errors in parentheses. For comparison,

<sup>11</sup>Recall that subjects who faced  $q = 0.2$  in Part 3 had experienced contests with  $q = 0.8$  in Part 2, and vice versa. Hence, by the beginning of Part 3, subjects experienced different histories; therefore, the comparisons using Part 3 decisions across different treatments should be interpreted with some caution.

Treatment	Ave. investment: 1-30	Ave. investment: 16-30	NE investment
3Low	13.05 (1.16)	10.71 (1.22)	10.67
3High	29.95 (3.29)	27.32 (3.86)	26.66
6Low	21.09 (1.98)	17.32 (2.89)	19.03
6High	34.34 (1.66)	31.01 (1.66)	19.49

Table 3: Average individual investment in Part 2, by treatment, with robust standard errors in parentheses, clustered by matching group.

the table also provides the NE predictions. The one-sample Wilcoxon signed-rank tests comparing the average investments in rounds 16-30 to the NE predictions produce the two-sided  $p$ -values of 0.889, 0.866, 0.612 and 0.028 for treatments 3Low, 3High, 6Low and 6High, respectively. Thus, over the last 15 rounds, only the 6High treatment exhibits average overbidding relative to the NE. This surprising level of agreement with the point predictions is in stark contrast to the widely documented phenomenon of overbidding in lottery contest experiments with deterministic group size (Sheremeta, 2013).

**Result 1** (a) *In treatment conditions 3Low, 3High, and 6Low, average investment agrees with the NE point predictions.*

(b) *In treatment 6High, we find significant overbidding relative to the NE point prediction.*

Regarding comparative statics with respect to maximal group size  $n$ , as predicted, when the participation probability is low ( $q = 0.2$ ), increasing the maximal group size from  $n = 3$  to  $n = 6$  leads to a significant increase in average investment from 13.05 to 21.09 (Wilcoxon rank-sum test,  $p = 0.004$ ). On the other hand, when the participation probability is high ( $q = 0.8$ ), the prediction that spending decreases in the maximal group size is not supported. In fact, average investment increases slightly (though not significantly) from 29.95 to 34.34 (Wilcoxon rank-sum test,  $p = 0.110$ ). We find the same results for these comparative static predictions when we examine average investment over only the last 15 rounds of the sequence (rounds 16-30).

Similarly, as predicted, increasing the participation probability from  $q = 0.2$  to  $q = 0.8$  leads to a significant increase in average investment from 13.05 to 29.95 when  $n = 3$  (Wilcoxon rank-sum test,  $p = 0.001$ ). However, contrary to theory, the increase in  $q$  also leads to a significant increase in investment from 21.09 to 34.34 (Wilcoxon rank-sum test,  $p = 0.002$ ) when  $n = 6$ . The same comparisons hold for the data from rounds 16-30.

**Result 2** (a) *Investment increases in  $n$ , as predicted, for low  $q$ , but does not change, contrary to the prediction (reduction), for high  $q$ .*

(b) Investment increases in  $q$ , as predicted, for low  $n$ , but also increases in  $q$ , contrary to the prediction (no change), for high  $n$ .

Overall, our basic results agree well with the theory. The disagreement is driven by subjects' behavior in 6High – the only treatment where we observe substantial overbidding relative to the equilibrium predictions. To check the robustness of our findings, we also analyzed behavior between treatments using the data from Part 3 of the experiment. The results, reported in Section A.1 in the Appendix, strongly confirm all four of the comparative static predictions.<sup>12</sup>

Finally, although our design and analysis focuses on individual investment, we also find evidence consistent with the predicted effects of  $n$  and  $q$  on expected total investment  $X^* = nqx^*$ . Lim and Matros (2009) show that although there may be nonmonotonicity in individual investment, expected total investment is always increasing in  $n$  and in  $q$ , holding other parameters constant. We use the individual investment decisions in Part 2 to compute the (ex ante) expected total investment, averaged across matching groups, for each treatment. Consistent with the prediction, an increase from  $n = 3$  to  $n = 6$  leads to a significant increase in average total investment from 7.83 to 25.31 when  $q = 0.2$  (Wilcoxon rank-sum test,  $p = 0.001$ ), and from 71.88 to 164.85 when  $q = 0.8$  (Wilcoxon rank-sum test,  $p = 0.002$ ). Similarly, as predicted, an increase from  $q = 0.2$  to  $q = 0.8$  leads to a significant increase in average total investment from 7.83 to 71.88 when  $n = 3$  (Wilcoxon rank-sum test,  $p = 0.001$ ) and from 25.31 to 164.85 when  $n = 6$  (Wilcoxon rank-sum test,  $p = 0.002$ ).

## 4.2 Dynamics of individual investment

In this section, we explore the dynamics of individual investment in Part 2 of the experiment. We analyze how subjects adjust their investment over time in response to the payoff-relevant feedback they receive, and also control for individual characteristics.

Table 4 reports the OLS estimates from several regression models. Model (1) is a simple robustness check of comparative statics of individual investment ( $Investment_t$ ) across treatments controlling for the time trend ( $Round$ ) and individual characteristics – the measures of the subject's ambiguity aversion ( $AA$ ), risk aversion ( $RA$ ), loss aversion ( $LA$ ), and the subject's gender ( $Female = 1$  if the subject is a female and zero otherwise). The results are rather standard: The previously reported comparisons between treatments hold up, investment exhibits an overall negative time trend, and more ambiguity-averse subjects tend to invest less.<sup>13</sup>

In model (2), the dependent variable is  $\Delta Investment_t$  defined as the difference in the subject's investments between rounds  $t$  and  $t - 1$ . As in model (1), we include the

---

<sup>12</sup>As noted earlier, between-subjects comparisons using data from Part 3 are not perfectly clean, since the subjects in different treatments will have experienced different treatment conditions in Part 2. Nevertheless, the comparative statics analysis provided in the Appendix can inform somewhat on the robustness of our main findings.

<sup>13</sup>While there is no ambiguity in the structure of the game itself, subjects face strategic ambiguity with respect to the behavior of others. It is, therefore, plausible that ambiguity aversion should play a role.

	Investment <sub>t</sub>	ΔInvestment <sub>t</sub>	ΔInvestment <sub>t</sub> 1 ≤ t ≤ 15	ΔInvestment <sub>t</sub> 16 ≤ t ≤ 30
	(1)	(2)	(3)	(4)
Round	-0.360*** (0.0818)	0.0103 (0.0183)		
3High	16.37*** (3.075)	-1.758*** (0.600)	-0.475 (1.531)	0.866 (1.073)
6Low	8.470*** (2.652)	-0.812** (0.378)	-1.430 (1.669)	1.669** (0.648)
6High	19.51*** (1.688)	-3.223*** (0.664)	1.259 (1.643)	1.231 (0.820)
Win <sub>t-1</sub>		-6.459*** (1.230)	-5.055*** (1.760)	-0.891* (0.461)
Win <sub>t-1</sub> × 3High			-1.522 (2.178)	-2.868 (2.382)
Win <sub>t-1</sub> × 6Low			1.448 (2.104)	-2.747** (1.018)
Win <sub>t-1</sub> × 6High			-13.70*** (3.013)	-10.04*** (3.106)
Ave.Inv.Others <sub>t-1</sub>		-0.0130 (0.0258)		
AA	-0.691* (0.377)	-0.0512* (0.0278)		
RA	0.561 (0.351)	0.0156 (0.0260)		
LA	-0.157 (0.201)	0.0194 (0.0177)		
Female	1.075 (3.418)	-0.216 (0.329)		
Intercept	14.90*** (3.953)	4.370*** (1.250)	3.107** (1.490)	0.511* (0.280)
Observations	5,220	5,046	2,436	2,610
Clusters	29	29	29	29
R-squared	0.140	0.016	0.034	0.015

Table 4: OLS regression results for the dynamics of individual investment in Part 2. Standard errors in parentheses are clustered by matching group. Significance levels: \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

treatment dummies and individual characteristics. In addition, we control for whether the subject won the contest in the previous round ( $Win_{t-1} = 1$  if the subject won and zero otherwise) and for the average investment of other subjects in the subject's realized group in the previous round ( $Ave.Inv.Others_{t-1}$ ). As seen from the results, subjects exhibit strong reactions to winning and losing. For example, *ceteris paribus* in the baseline

Treatment	$\Delta\text{Investment}_t$		Win Freq.	Ave. $\Delta\text{Investment}_t$
	$\text{Win}_{t-1} = 1$	$\text{Win}_{t-1} = 0$		
<b>Rounds 1-15</b>				
3Low	-5.055	3.107	0.817	-3.558
3High	-6.577	2.633	0.397	-1.022
6Low	-3.607	1.678	0.643	-1.719
6High	-18.759	4.366	0.227	-0.883
<b>Rounds 16-30</b>				
3Low	-0.891	0.511	0.811	-0.626
3High	-3.759	1.377	0.427	-0.816
6Low	-3.638	2.180	0.629	-1.477
6High	-10.931	1.742	0.203	-0.833

Table 5: Average round-by-round investment adjustments in response to winning and losing, average winning frequencies, and total average investment adjustments, by treatment.

treatment (3Low), they increase investment by 4.37 (the coefficient on the intercept,  $p < 0.01$ ) in response to losing and decrease investment by 2.09 (the sum of the intercept and the coefficient on  $\text{Win}_{t-1}$ ,  $p < 0.01$ , Wald test) in response to winning.<sup>14</sup> These effects are consistent with a combination of reinforcement learning (e.g., Roth and Erev, 1995) and directional learning (e.g., Selten and Stoecker, 1986) theories whereby strategies that lead to a reduction in payoff are played less frequently and investment is adjusted in the direction of higher payoffs (for a similar approach, see Grosskopf, 2003; Dutcher et al., 2015). Indeed, winning provides a signal that the same high payoff could possibly be achieved with a lower investment while losing indicates that a higher investment is needed. Notably, conditional on the winning/losing feedback the information about investments of other group members has no effect on effort adjustment. In line with model (1), more ambiguity-averse subjects tend to increase their investment less.

In models (3) and (4), we analyze investment adjustment in response to winning/losing in more detail, controlling for possible differences in the adjustment process across treatments via interactions of  $\text{Win}_{t-1}$  with the treatment dummies. We do not control for any other factors, which allows us to obtain more precise quantitative estimates for average investment adjustments conditional on winning and losing in each treatment. Column (3) reports the results using data from rounds 1-15, while column (4) uses data from rounds 16-30. The average estimated effects based on these regressions are summarized in Table 5.

Table 5 shows the estimated average change in investment from round  $t-1$  to round  $t$  in response to winning ( $\text{Win}_{t-1} = 1$ ) and losing ( $\text{Win}_{t-1} = 0$ ), average winning frequency

<sup>14</sup>These estimates are approximate as they do not take into account changes over time following the coefficient on *Round*. As time progresses, the estimated increase in investment after losing is increasing slightly while the estimated reduction in investment after winning is decreasing slightly, although these trends are not statistically significant.

and overall average change in investment for each treatment. As seen from the table, subjects reduce investment after winning and increase it after losing in all treatments. These effects are stronger at the beginning (in rounds 1-15 as compared to rounds 16-30), with the exception of treatment 6Low where they appear to be stable over time. Importantly, winning frequencies differ substantially across treatments, from around 0.8 in 3Low to 0.2 in 6High. Total average investment adjustments (the last column in Table 5) are a weighted average of the adjustments after winning and losing with the corresponding winning and losing frequencies as weights. As a result, the initial downward trend is the strongest in 3Low and 6Low where the frequencies of winning are the highest, whereas in 6High there is nearly no reduction in investment over time even though the downward adjustment after winning is very strong. These differences in adjustment are consistent with the observed differences in overbidding between 6High and the other three treatments.

We conclude that the observed dynamics in individual investment are consistent with the basic models of reinforcement and directional learning. Reinforcement works differently in different treatments due to differences in the frequencies of positive and negative stimuli.

### 4.3 Average investment in 3HL<sup>+</sup>

In this section, we further explore the surprising absence of overbidding summarized in Result 1. We first provide some intuition based on the differences between our main treatments. A notable difference between the treatment where overbidding still arises, 6High, and the other three treatments, is that the probability of group size being equal to one is negligible in the former, but nontrivial for the latter. This key difference suggests that the possibility of no conflict (being in a group of size one) reduces overbidding when group size is uncertain. Based on this intuition, we implemented the additional control treatment, 3HL<sup>+</sup>, in which the support of the probability distribution over group size is shifted up by one.<sup>15</sup> That is, realized group size could be 2, 3, or 4. If the possibility of being in a group of size one is a driving factor behind our lack of overbidding, then we would expect overbidding to be restored in both sequences of the 3HL<sup>+</sup> treatment.

As described in Section 3, the procedures for the 3HL<sup>+</sup> treatment were identical to the procedures for the other treatments, except with respect to the possible group sizes. Specifically, the realized group size was equal to  $(m + 1)$ , where  $m - 1$  is a binomial random variable with success parameter  $q$ , representing the number of (additional) active participants. For each treatment condition, we calculate the NE investments, given the prize value of  $V = 120$ . For 3<sup>+</sup>High, the NE is  $x^* = 24.13$ , while for 3<sup>+</sup>Low, the NE is  $x^* = 28.63$ .

Table 6 provides the summary statistics for the average investment in our 3HL<sup>+</sup> treatment, as well as the corresponding NE investment level. Using the one-sample Wilcoxon signed-rank test to compare average investment with the NE point predictions, we find significant overbidding in both treatment conditions, 3<sup>+</sup>High and 3<sup>+</sup>Low. For 3<sup>+</sup>High,

---

<sup>15</sup>We are especially grateful to an anonymous referee for suggesting this as an additional treatment.



Treatment (Rounds)	Ave. investment	NE investment
3 <sup>+</sup> High (1-30)	36.58 (1.95)	24.13
3 <sup>+</sup> High (16-30)	34.31 (3.36)	24.13
3 <sup>+</sup> Low (31-60)	37.26 (3.46)	28.63
3 <sup>+</sup> Low (46-60)	34.63 (3.05)	28.63

Table 6: Average individual investment in treatment 3HL<sup>+</sup>, with robust standard errors in parentheses, clustered by matching group.

the difference is positive and significant over all 30 rounds of the first sequence ( $p = 0.018$ ) and over the last 15 rounds of the first sequence ( $p = 0.028$ ). Likewise, for 3<sup>+</sup>Low, which is played only in the second sequence (and thus may be influenced by subjects' experiences in Part 2), the difference is positive and significant across all 30 rounds ( $p = 0.063$ ) and over the last 15 rounds of the second sequence ( $p = 0.091$ ). These summary statistics, and the corresponding average overbidding are also illustrated by Figure 2, which plots the average investment in each round for the 3<sup>+</sup>High treatment condition implemented in Part 2.<sup>16</sup>

**Result 3** *In both 3<sup>+</sup>High and 3<sup>+</sup>Low, we find significant overbidding relative to the NE point prediction.*

Thus, our additional control treatment provides further evidence that the absence of overbidding is linked to the possibility of group size being equal to one. When the probability of this outcome is negligible (6High) or absent altogether (3<sup>+</sup>High and 3<sup>+</sup>Low), we find overbidding, consistent with the rest of the experimental contests literature. However, when the probability is nonnegligible, even if it is small (e.g. 4% in 3Low), there is no average overbidding relative to the NE investment level.

## 5 Explaining the differences in overbidding

Before we turn to our formal explanation, it will be helpful to outline the various alternative explanations that have been proposed for overbidding in standard contest experiments. The survey study by [Dechenaux, Kovenock and Sheremeta \(2015\)](#) presents a summary of these different explanations, while [Sheremeta \(2016\)](#) provides a comprehensive experimental test of the various theories.

<sup>16</sup>We find similar visual support for overbidding in all rounds for the 3<sup>+</sup>Low treatment condition implemented in Part 3. However, since subjects had already experienced the high participation probability in Part 2, we omit the data from Figure 2.

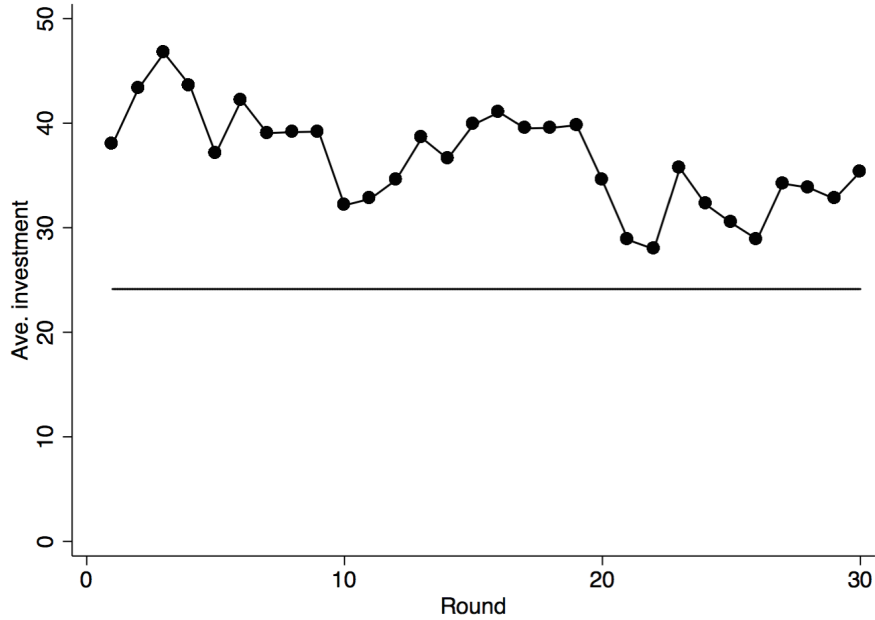


Figure 2: Average investment in the treatment condition 3+High. The reference line indicates the NE point prediction.

One important explanation is that subjects derive some additional nonmonetary utility from winning, and that this induces higher effort investments by contestants. There is ample evidence that winning in contests *per se* has value beyond the monetary value of the prize due to the joy of winning (e.g., [Goeree, Holt and Palfrey, 2002](#); [Sheremeta, 2010](#); [Brookins and Ryvkin, 2014](#)). A second proposed explanation is that subjects who are concerned with status or relative payoffs may invest more effort in the contest ([Hehenkamp, Leininger and Possajennikov, 2004](#); [Mago, Samak and Sheremeta, 2016](#)). A third explanation is based on the argument that individuals often make mistakes, which introduces noise into the equilibrium solution. One of the standard approaches for explaining overbidding in this context is the Quantal Response Equilibrium (QRE) framework ([McKelvey and Palfrey, 1995](#)) where it is assumed that players mix over their available strategy spaces so that a strategy  $s$  is chosen with a probability that increases in the expected payoff from  $s$  given the behavior of other players. Several other studies have provided supporting evidence for this argument using the QRE framework in standard lottery contests ([Sheremeta, 2011](#); [Chowdhury, Sheremeta and Turocy, 2014](#); [Lim, Matros and Turocy, 2014](#); [Brookins and Ryvkin, 2014](#)). Another explanation contends that individuals are subject to certain judgmental biases, such as nonlinear probability weighting or the hot hand fallacy, which lead individuals to bid above the Nash equilibrium ([Parco, Rapoport and Amaldoss, 2005](#); [Baharad and Nitzan, 2008](#); [Amaldoss and Rapoport, 2009](#); [Sheremeta, 2011](#)).

Our proposed model is based on a combination of nonlinear probability weighting

with respect to group size uncertainty and a joy of winning effect that depends on the expected frequency of winning. Although we think that bounded rationality, particularly as captured in the QRE framework, is also an important consideration, we do not include it in our proposed model. However, we illustrate the implications of QRE behavior in our setting in Appendix A.2.

## 5.1 Probability weighting

The basic intuition for our model is as follows. First, we argue that subjects overweigh the probability of being in a contest on their own, consistent with the formation of decision weights in Cumulative Prospect Theory (Tversky and Kahneman, 1992). In deriving the NE level of effort when group size is uncertain, the possibility of being in a group of size one (i.e. of being the only participant) has the effect of reducing the optimal effort in the contest, since in a group of size one, the payoff maximizing expenditure is 0. As such, if subjects place higher decision weight on the possibility that the size of their group is one, it may lead them to further reduce their equilibrium spending relative to the NE.

To illustrate the effects of such probability weighting, we derive the adjusted, probability-weighted NE investment levels assuming that subjects form decision weights according to the probability weighting function estimated by Tversky and Kahneman (1992). The weighting function is given as follows,

$$\pi(p) = \frac{p^\gamma}{(p^\gamma + (1-p)^\gamma)^{\frac{1}{\gamma}}}. \quad (4)$$

Tversky and Kahneman (1992) estimate the value of  $\gamma$  to be 0.61 for outcomes that are expressed in the realm of gains in their setting.<sup>17</sup> Consistent with the extension of Prospect Theory to Cumulative Prospect Theory (CPT), we apply the weighting function to the cumulative probabilities. To this end, we first rank the outcomes (group size) from lowest (1) to highest ( $n$ ).<sup>18</sup> For the highest group size, the decision weight is given by  $\hat{\pi}(p_n) = \pi(p_n)$ , as defined above. For the second highest group size,  $n-1$ , the decision weight is given by

$$\hat{\pi}(p_{n-1}) = \pi(p_n + p_{n-1}) - \pi(p_n), \quad (5)$$

while for all other group sizes  $k < n-1$ ,

$$\hat{\pi}(p_k) = \pi\left(\sum_{i=k}^n p_i\right) - \pi\left(\sum_{i=k+1}^n p_i\right). \quad (6)$$

The decision weights calculated for each treatment in our experiment are shown in Table 7. In addition, the table reports the NE (assuming the actual probabilities are used) and the adjusted (probability-weighted) NE investment level, assuming subjects use the calculated decision weights.

---

<sup>17</sup>Other studies have estimated slightly different values for  $\gamma$ . However, the effect of probability weighting in our environment is not particularly sensitive to the exact parameter value. Thus, we adopt the original estimate from Tversky and Kahneman (1992) for our calculations.

<sup>18</sup>In treatment 3HL<sup>+</sup>, groups sizes change between 2 and 4.

Treatment	Group Size $m$	Actual $p_m$	CPT decision weights $\hat{\pi}(p_m)$	NE	Adjusted NE
3Low	1	0.64	0.65029	10.67	10.10
	2	0.32	0.23242		
	3	0.04	0.11729		
3High	1	0.04	0.18493	26.66	22.796
	2	0.32	0.31833		
	3	0.64	0.49674		
6Low	1	0.32768	0.48377	19.03	13.739
	2	0.4096	0.21828		
	3	0.2048	0.15607		
	4	0.0512	0.09776		
	5	0.0064	0.03682		
	6	0.00032	0.00729		
6High	1	0.00032	0.01186	19.49	20.618
	2	0.0064	0.05892		
	3	0.0512	0.15155		
	4	0.2048	0.21854		
	5	0.4096	0.22612		
	6	0.32768	0.33300		
3 <sup>+</sup> High	2	0.04	0.18493	24.13	25.21
	3	0.32	0.31833		
	4	0.64	0.49674		

Table 7: Decision weights and Adjusted NE investment levels for each treatment using the CPT probability weighting function.

Table 7 shows that if subjects use decision weights that distort the objective probabilities, consistent with CPT, the adjusted NE investment levels are lower than when the objective probabilities are used, in all treatments except 6High and 3<sup>+</sup>High, where the probabilities of group size being one are negligible and zero, respectively. Thus, directionally, we would expect nonlinear probability weighting to lower investment in the three treatments where overbidding is absent, and to increase investment in the two treatments where overbidding is present. Furthermore, the adjusted equilibrium investment level is substantially lower in 3High and 6Low, compared with the unadjusted NE.

The forces underlying these differences between treatments are quite straightforward. In 3High and 6Low, the decision weights assigned to  $m = 1$  (group size equal to one) are substantially higher than the objective probabilities. Since the optimal investment is zero for a contestant who faces no other participants, these decision weights significantly reduce the adjusted equilibrium investment below the NE. In contrast, in 3Low, the decision weight for  $m = 1$  is only slightly higher than the objective probability. However,

the decision weight on  $m = 3$  is substantially higher, while the decision weight on  $m = 2$  is substantially lower. The net result is also a reduction in the equilibrium investment level, albeit less pronounced for the 3Low treatment than for 3High and 6Low.

By contrast, in 6High, the inflation of decision weights on groups of size  $m = 2$  and  $m = 3$ , which provide incentives for much higher investment, is stronger than the effect of inflating the negligible probability that  $m = 1$ . In 3<sup>+</sup>High, the probability of the lowest possible group size is significantly inflated, the same as in 3High. However, the lowest possible group size is two in 3<sup>+</sup>High, which provides incentives for higher investment, unlike the lowest possible group size in 3High.

To summarize, in our setting where group size is uncertain, the effects of nonlinear probability weighting on equilibrium investment depend on whether or not there is a non-trivial probability of group size being one. In the three treatments where this probability is nontrivial, probability weighting has a strong negative effect on the equilibrium investment level. This negative effect may counter the effect of nonmonetary utility of winning, which might otherwise be expected to generate overbidding. In contrast, when the probability that group size equals one is trivial, as in 6High, or zero, as in 3<sup>+</sup>High, probability weighting actually increases the equilibrium investment relative to the standard NE. As such, any effects driven by the joy of winning will be unfettered by the effect of probability weighting, which also works to (marginally) increase investment levels above the standard NE.

While probability weighting is a critical part of the explanation for our results, the adjusted equilibrium calculations in Table 7 do not explain the observed investment. However, we have yet to account for any other potential influences on investment levels, such as joy of winning, relative payoff concerns, or bounded rationality. To unify our model, we next develop a simple modification of the standard joy of winning model that, together with probability weighting, provides a full explanation for our results.

## 5.2 A Constant Winning Aspirations model of joy of winning

The standard way of modeling joy of winning in contests is to include an additive term,  $w$ , on top of the monetary prize value  $V$  (see, e.g., [Goeree, Holt and Palfrey, 2002](#); [Sheremeta, 2010](#); [Brookins and Ryvkin, 2014](#)). The expected utility of player  $i$  then takes the form

$$u_i(x_i, x_{-i}) = (V + w)P_i(x_i, x_{-i}) - x_i,$$

which leads to a symmetric equilibrium investment level,  $x_w^*$ , above the standard equilibrium investment in the absence of joy of winning,  $x_0^*$ . For example, in a Tullock contest with group size  $m$  the resulting equilibrium investment is  $x_w^* = \frac{(V+w)(m-1)}{m^2}$ , and the predicted level of overbidding is  $\delta x_w^* \equiv x_w^* - x_0^* = \frac{w(m-1)}{m^2}$ .

By appropriately selecting  $w$ , average overbidding in any contest experiment can be explained.<sup>19</sup> However, the existing evidence suggests that a single additive parameter

---

<sup>19</sup>[Sheremeta \(2010\)](#) proposes a method for independently estimating  $w$  by letting subjects bid in a contest for zero prize.

$w$  cannot explain differences in overbidding *across games* within the same experiment. To see this, recall that one of the standard equilibrium comparative statics in Tullock contests is the reduction of equilibrium investment with group size. However, the existing experimental evidence on the effect of group size on investment in contests is mixed, showing either no systematic variation of average bids with group size or a much smaller than predicted reduction. For example, [Anderson and Stafford \(2003\)](#) found no significant variation in spending across group sizes between 2 and 5. Similarly, [Morgan, Orzen and Sefton \(2012\)](#) and [Lim, Matros and Turocy \(2014\)](#) found no systematic effect of group size on spending in contests of 2, 3, 4 and 2, 4 and 9 players, respectively; and [Baik et al. \(2015\)](#) found no significant differences in average spending between contests of 2 and 3 players. In any of these experiments, a single joy of winning parameter  $w$  would not be able to explain overbidding across group sizes because the predicted overbidding,  $\delta x_w^*$ , is *decreasing* in the group size, whereas the observed overbidding is *increasing*.

In what follows, we propose a simple modification to the standard model of joy of winning, which helps explain this phenomenon and also allows us to explain differences in overbidding between treatments in our experiment where group size is uncertain.

In the original model, parameter  $w$  measures how much, in monetary terms, a player values winning, on top of the value of the prize. However, there is no reason to think that subjects should place the same value on winning contests with different group sizes, or, more generally, with different probabilities of winning. In fact, there is ample evidence that people value winning more the harder it is to win. For example, in sports competitions, national championship is valued more than regional championship; and in academia, publications in journals with lower acceptance rates are more valuable.<sup>20</sup>

In order to construct a parsimonious model, we propose a *Constant Winning Aspirations* (CWA) hypothesis, whereby across similar contests of different group sizes  $m$ , joy of winning changes with group size in such a way that the expected value of winning in equilibrium,  $W = wP_m^*$ , is a constant. Here,  $P_m^*$  is the equilibrium probability of winning in the contest of size  $m$ . In symmetric contests with deterministic group sizes,  $P_m^* = \frac{1}{m}$ , and hence  $W = \frac{w}{m}$ .<sup>21</sup> For contests with stochastic group sizes,  $P_m^*$  can be calculated accordingly. The modified model still contains only one parameter,  $W$ , which now measures the expected value of winning.

For any given contest,  $P_m^*$  is fixed and the CWA assumption is not distinguishable from the original joy of winning model. Across contests of different sizes, however, the CWA predicts the joy of winning parameter  $w = mW$  increasing linearly with group size. For a Tullock contest, this gives the equilibrium effort  $x_W^* = \frac{(V+mW)(m-1)}{m^2}$ . The resulting

---

<sup>20</sup>Of course, monetary rewards (or their equivalents) also tend to be higher in larger contests, but *ceteris paribus* winning a larger contest, or, more generally, winning in any situation where it is more difficult to win, still carries more intrinsic value.

<sup>21</sup>This linear functional form was proposed by [Sheremeta \(2010\)](#) as one of alternative specifications for the joy of winning, but it was never explored. [Parco, Rapoport and Amaldoss \(2005\)](#) proposed a nonlinear functional form for the joy of winning depending on the group size, the number of groups competing at the first stage, and the prize value. Keeping the group sizes and equilibrium winning probabilities fixed across treatments, they varied the value of the prize and were able to fit the observed expenditures reasonably well.

Treatment	CPT-adjusted Prob(win)	Observed Ave. Investment (rounds 16-30)	CWA-predicted Ave. Investment	Estimated Joy of Winning, $w$
3Low	0.806	10.71	11.73	19.34
3High	0.510	27.32	28.61	30.57
6Low	0.678	17.32	16.37	22.99
6High	0.247	31.01	31.46	63.12
3 <sup>+</sup> High	0.432	34.31	32.79	36.09

Table 8: CPT-adjusted winning probabilities, observed and predicted average investment and estimated joy of winning, by treatment, with CWA parameter  $W = 15.59$ .

level of overbidding  $\delta x_W^* = \frac{W(m-1)}{m}$  is *increasing* in the group size  $m$ , consistent with the empirical evidence.

The CWA assumption can be interpreted statically as based on the individual’s expectation about how difficult it is to win the contest. It can also be interpreted dynamically in a repeatedly played contest as a long-run equilibrium of a learning process in which winning and losing serve as reinforcements. In a larger contest, over time the individual realizes that winning is more rare and values it more. This interpretation is related to our findings regarding the adjustment of investment over time reported in Section 4.2. As seen from Table 5, in treatment 6High subjects reduce investment very strongly in response to winning; this indicates that, on average, subjects realize that they are overbidding and are happy to scale down their bids. However, winning is so rare in 6High that the relatively moderate upward adjustments in response to losing, driven by winning aspirations, completely compensate these reductions, and excessive investment does not decline over time.

In our experiment with group size uncertainty, the equilibrium probabilities of winning are very different across treatments. Under the CWA assumption, we should observe the highest joy of winning effects in 6High and the lowest in 3Low. In order to calibrate the model, we assume that subjects use the CPT-adjusted probabilities of different group sizes, as shown in Table 7, to form beliefs about the resulting probabilities of winning. We then use those probabilities to calculate the predicted level of overbidding, relative to the CPT-adjusted NE, in each treatment as a function of parameter  $W$ . Finally, we run an OLS regression, without the intercept, of observed average overbidding in rounds 16-30 on the coefficient on  $W$  in the predicted overbidding. This produces the estimate of  $W = 15.59$  (SE=1.30,  $N = 5$ ,  $R^2 = 0.86$ ). The estimates of joy of winning parameter  $w$  by treatment range from  $w = 19.34$  in 3Low to  $w = 63.12$  in 6High. Interestingly, the latter estimate is very close to the value of  $w = 62.9$  obtained by [Sheremeta \(2010\)](#) for contests with deterministic group size four by letting subjects invest in a contest with zero prize. The agreement is rather striking considering that the equilibrium probability of winning in a four-player contest is 0.25, which is very close to the CPT-adjusted equilibrium probability of winning in 6High (0.247). The results are summarized in Table 8, which

shows CPT-adjusted winning probabilities, observed and predicted average investments and estimated joy of winning for each of the five treatments. As seen from the table, the one-parameter CWA model with CPT-adjusted winning probabilities predicts investment across treatments remarkably well.

## 6 Conclusions

In this paper, we report the first experimental study of contest environments with unknown group size. We designed our experiment to test the comparative static predictions for individual investment derived by [Lim and Matros \(2009\)](#) for lottery contests with a binomially distributed number of participants.

Our main results are quite surprising, especially when compared with the substantial evidence of overbidding in contests where the number of players is known. Average investment converges very closely to the theoretical point predictions except in the treatment with  $n = 6$  and  $q = 0.8$ . The overbidding in this treatment (together with the consistency between theory and data in the other treatments) contradicts two of the four comparative static predictions. Furthermore, in an additional control treatment, where the possibility of group size equal to one is eliminated, we find that overbidding is restored. These results suggest that when there is a nontrivial probability that conflict is dissolved due to the lack of competitors, overbidding is significantly reduced.

We propose a unifying explanation for the differences in overbidding across the different treatment conditions. Our model incorporates nonlinear probability weighting, consistent with CPT, and develops a modified notion of the joy of winning, called Constant Winning Aspirations (CWA). With probability weighting, in the three treatments where we find close alignment between theory and average behavior, subjects tend to overweigh the possibility of being in a group of size one, which provides a countervailing influence on the tendency to overbid. When the probability of being in a group of size one is negligible or absent altogether, no such countervailing influence emerges. In addition, our CWA hypothesis predicts differential effects due to joy of winning, depending on the expected equilibrium probability of winning in different treatment conditions. Together, these two features are able to neatly organize and explain all of our results. Admittedly, our experiment was not designed to systematically test the CWA hypothesis, and with only five treatment conditions varying the equilibrium probability of winning, our results provide a rather limited support for it.

There are several extensions to our experiment that could be explored. First, our CWA hypothesis also provides a possible explanation for the existing experimental results concerning the effects of deterministic group size on overbidding in contests and auctions. While the standard joy of winning model would predict reduced overbidding in larger groups, several experimental studies have reported higher overbidding in larger groups. Under the CWA assumption, we show that the equilibrium level of overbidding (relative to the standard NE) is increasing in the number of contestants. Thus, one exercise for future research would be to estimate the CWA parameter for these existing



experimental studies, or for a new experiment that systematically compares similar contests or auctions with different, deterministic group sizes. This analysis can be further extended, and the CWA assumption tested, in other environments, such as contests with heterogeneous players, group contests or dynamic contests. Second, while our experiment provides important results on the comparative statics of behavior in response to exogenous variation in underlying group size uncertainty, a richer model might explore a contest environment with endogenous entry, in which the contestants must also make investment decisions before group size uncertainty is resolved. Finally, in this paper we have explored group size uncertainty only in the context of Tullock lottery contests with homogeneous participants. For future work, it may be interesting to extend the study to contests with heterogeneous agents, or with different contest success functions.

## **Acknowledgments**

We would like to thank an Associate Editor and three referees for several insightful comments and suggestions that greatly improved the manuscript. We gratefully acknowledge financial support from the College of Social Sciences and Public Policy at Florida State University.

## References

- Amaldoss, Wilfred, and Amnon Rapoport.** 2009. “Excessive expenditure in two-stage contests: Theory and experimental evidence.” *Game Theory: Strategies, Equilibria, and Theorems*. Hauppauge, NY: Nova Science Publishers.
- Anderson, Lisa R., and Sarah L. Stafford.** 2003. “An experimental analysis of rent seeking under varying competitive conditions.” *Public Choice*, 115(1-2): 199–216.
- Baharad, Eyal, and Shmuel Nitzan.** 2008. “Contest efforts in light of behavioural considerations.” *The Economic Journal*, 118(533): 2047–2059.
- Baik, Kyung Hwan, Subhasish Chowdhury, Abhijit Ramalingam, et al.** 2015. “Group size and matching protocol in contests.” School of Economics, University of East Anglia, Norwich, UK.
- Brookins, Philip, and Dmitry Ryvkin.** 2014. “An experimental study of bidding in contests of incomplete information.” *Experimental Economics*, 17(2): 245–261.
- Chowdhury, Subhasish M., Roman M. Sheremeta, and Theodore L. Turocy.** 2014. “Overbidding and overspreading in rent-seeking experiments: Cost structure and prize allocation rules.” *Games and Economic Behavior*, 87: 224–238.
- Congleton, Roger D., Arye L. Hillman, and Kai A. Konrad.** 2008. *40 Years of Research on Rent Seeking*. Vol. 1 and 2, Berlin:Springer.
- Connelly, Brian L., Laszlo Tihanyi, T. Russell Crook, and K. Ashley Gangloff.** 2014. “Tournament Theory: Thirty Years of Contests and Competitions.” *Journal of Management*, 40(1): 16–47.
- Corchón, Luis C.** 2007. “The theory of contests: a survey.” *Review of Economic Design*, 11(2): 69–100.
- Dechenaux, Emmanuel, Dan Kovenock, and Roman M. Sheremeta.** 2015. “A survey of experimental research on contests, all-pay auctions and tournaments.” *Experimental Economics*, 18(4): 609–669.
- De Sinopoli, Francesco, and Carlos Pimienta.** 2009. “Undominated (and) perfect equilibria in Poisson games.” *Games and Economic Behavior*, 66(2): 775–784.
- Dutcher, E. Glenn, Loukas Balafoutas, Florian Lindner, Dmitry Ryvkin, and Matthias Sutter.** 2015. “Strive to be first or avoid being last: An experiment on relative performance incentives.” *Games and Economic Behavior*, 94: 39–56.
- Dyer, Douglas, John H. Kagel, and Dan Levin.** 1989. “Resolving uncertainty about the number of bidders in independent private-value auctions: an experimental analysis.” *The RAND Journal of Economics*, 268–279.

- Fischbacher, Urs.** 2007. “z-Tree: Zurich toolbox for ready-made economic experiments.” *Experimental Economics*, 10(2): 171–178.
- Fu, Qiang, and Jingfeng Lu.** 2010. “Contest design and optimal endogenous entry.” *Economic Inquiry*, 48(1): 80–88.
- Fu, Qiang, Qian Jiao, and Jingfeng Lu.** 2011. “On disclosure policy in contests with stochastic entry.” *Public Choice*, 148(3-4): 419–434.
- Goeree, Jacob K, Charles A Holt, and Thomas R Palfrey.** 2002. “Quantal response equilibrium and overbidding in private-value auctions.” *Journal of Economic Theory*, 104(1): 247–272.
- Greiner, Ben.** 2015. “Subject pool recruitment procedures: organizing experiments with ORSEE.” *Journal of the Economic Science Association*, 1(1): 114–125.
- Grosskopf, Brit.** 2003. “Reinforcement and directional learning in the ultimatum game with responder competition.” *Experimental Economics*, 6(2): 141–158.
- Harstad, Ronald M., John H. Kagel, and Dan Levin.** 1990. “Equilibrium bid functions for auctions with an uncertain number of bidders.” *Economics Letters*, 33(1): 35–40.
- Hehenkamp, Burkhard, Wolfgang Leininger, and Alexandre Possajennikov.** 2004. “Evolutionary equilibrium in Tullock contests: spite and overdissipation.” *European Journal of Political Economy*, 20(4): 1045–1057.
- Holt, Charles A., and Susan K. Laury.** 2002. “Risk aversion and incentive effects.” *American Economic Review*, 92(5): 1644–1655.
- Huck, Steffen, Kai A. Konrad, Wieland Müller, and Hans-Theo Normann.** 2007. “The merger paradox and why aspiration levels let it fail in the laboratory.” *The Economic Journal*, 117(522): 1073–1095.
- Ioannou, Christos A., and Miltiadis Makris.** 2017. “An Experimental Study of Uncertainty in Coordination Games.” <https://eprints.soton.ac.uk/381524/>.
- Isaac, R. Mark, Svetlana Pevnitskaya, and Kurt S. Schnier.** 2012. “Individual behavior and bidding heterogeneity in sealed bid auctions where the number of bidders is unknown.” *Economic Inquiry*, 50(2): 516–533.
- Kahana, Nava, and Doron Klunover.** 2015. “A note on Poisson contests.” *Public Choice*, 165(1-2): 97–102.
- Kahana, Nava, and Doron Klunover.** 2016. “Complete rent dissipation when the number of rent seekers is uncertain.” *Economics Letters*, 141: 8–10.

- Konrad, Kai A.** 2009. *Strategy and dynamics in contests*. New York, NY:Oxford University Press.
- Levin, Dan, and Emre Ozdenoren.** 2004. "Auctions with uncertain numbers of bidders." *Journal of Economic Theory*, 118(2): 229–251.
- Levin, Dan, and James L. Smith.** 1994. "Equilibrium in auctions with entry." *American Economic Review*, 84(3): 585–599.
- Lim, Wooyoung, Alexander Matros, and Theodore L. Turocy.** 2014. "Bounded rationality and group size in Tullock contests: Experimental evidence." *Journal of Economic Behavior & Organization*, 99: 155–167.
- Lim, Wooyoung, and Alexander Matros.** 2009. "Contests with a stochastic number of players." *Games and Economic Behavior*, 67: 584–597.
- Mago, Shakun D, Anya C Samak, and Roman M Sheremeta.** 2016. "Facing your opponents social identification and information feedback in contests." *Journal of Conflict Resolution*, 60(3): 459–481.
- Makris, Miltiadis.** 2008. "Complementarities and macroeconomics: Poisson games." *Games and Economic Behavior*, 62(1): 180–189.
- Makris, Miltiadis.** 2009. "Private provision of discrete public goods." *Games and Economic Behavior*, 67(1): 292–299.
- McAfee, R. Preston, and John McMillan.** 1987. "Auctions with a stochastic number of bidders." *Journal of Economic Theory*, 43(1): 1–19.
- McKelvey, Richard D., and Thomas R. Palfrey.** 1995. "Quantal response equilibria for normal form games." *Games and Economic Behavior*, 10(1): 6–38.
- Mohlin, Erik, Robert Östling, and Joseph Tao-yi Wang.** 2015. "Lowest unique bid auctions with population uncertainty." *Economics Letters*, 134: 53–57.
- Morgan, John, Henrik Orzen, and Martin Sefton.** 2012. "Endogenous entry in contests." *Economic Theory*, 51(2): 435–463.
- Münster, Johannes.** 2006. "Contests with an unknown number of contestants." *Public Choice*, 129(3-4): 353–368.
- Myerson, Roger B.** 1998. "Population uncertainty and Poisson games." *International Journal of Game Theory*, 27(3): 375–392.
- Myerson, Roger B.** 2000. "Large Poisson games." *Journal of Economic Theory*, 94(1): 7–45.

- Myerson, Roger B, and Karl Wärneryd.** 2006. "Population uncertainty in contests." *Economic Theory*, 27(2): 469–474.
- Palfrey, Thomas R., and Svetlana Pevnitskaya.** 2008. "Endogenous entry and self-selection in private value auctions: An experimental study." *Journal of Economic Behavior & Organization*, 66(3): 731–747.
- Parco, James E, Amnon Rapoport, and Wilfred Amaldoss.** 2005. "Two-stage contests with budget constraints: An experimental study." *Journal of Mathematical Psychology*, 49(4): 320–338.
- Pevnitskaya, Svetlana.** 2004. "Endogenous entry in first-price private value auctions: The self-selection effect." *Unpublished Manuscript*.
- Prendergast, Canice.** 1999. "The Provision of Incentives in Firms." *Journal of Economic Literature*, 37(1): 7–63.
- Roth, Alvin E., and Ido Erev.** 1995. "Learning in extensive-form games: Experimental data and simple dynamic models in the intermediate term." *Games and Economic Behavior*, 8(1): 164–212.
- Ryvkin, Dmitry, and Mikhail Drugov.** 2017. "Winner-take-all tournaments." *Working paper*. [http://myweb.fsu.edu/dryvkin/LR\\_stochastic\\_17.pdf](http://myweb.fsu.edu/dryvkin/LR_stochastic_17.pdf).
- Selten, Reinhard, and Rolf Stoecker.** 1986. "End behavior in sequences of finite Prisoner's Dilemma supergames A learning theory approach." *Journal of Economic Behavior & Organization*, 7(1): 47–70.
- Sheremeta, Roman M.** 2010. "Experimental comparison of multi-stage and one-stage contests." *Games and Economic Behavior*, 68(2): 731–747.
- Sheremeta, Roman M.** 2011. "Contest design: An experimental investigation." *Economic Inquiry*, 49(2): 573–590.
- Sheremeta, Roman M.** 2013. "Overbidding and heterogeneous behavior in contest experiments." *Journal of Economic Surveys*, 27(3): 491–514.
- Sheremeta, Roman M.** 2016. "Impulsive behavior in competition: Testing theories of overbidding in rent-seeking contests." *ESI Working Paper 16-21*. [http://digitalcommons.chapman.edu/esi\\_working\\_papers/199/](http://digitalcommons.chapman.edu/esi_working_papers/199/).
- Sutter, Matthias, Martin G. Kocher, Daniela Glätzle-Rützler, and Stefan T. Trautmann.** 2013. "Impatience and uncertainty: Experimental decisions predict adolescents' field behavior." *American Economic Review*, 103(1): 510.
- Szymanski, Stefan.** 2003. "The Economic Design of Sporting Contests." *Journal of Economic Literature*, 41(4): 1137–1187.

**Tullock, Gordon.** 1980. "Efficient rent seeking." *Toward a Theory of the Rent-Seeking Society*, ed. James M. Buchanan, Robert D. Tollison and Gordon Tullock, 97–112. College Station: Texas A&M University Press.

**Tversky, Amos, and Daniel Kahneman.** 1992. "Advances in prospect theory: Cumulative representation of uncertainty." *Journal of Risk and Uncertainty*, 5(4): 297–323.

# A Additional results

## A.1 Average investment in Part 3

In this section, we examine subjects' investment decisions in the second 30-period sequence of contests (Part 3 of the experiment). We refer to the treatments  $nLH$  as  $nHigh$ , and to the treatments  $nHL$  as  $nLow$ , to reflect the participation probabilities used in Part 3 of the experiment. In Figure A.1, we show that for the second sequence of 30 rounds, all of the comparative statics are supported. Summary statistics for each treatment are reported in Table A.1.

As we found for the sequence of decisions in Part 2 of the experiment, when the participation probability is low ( $q = 0.2$ ), increasing the maximal group size from  $n = 3$  to  $n = 6$  leads to a significant increase in average investment from 4.40 to 23.46 (Wilcoxon rank-sum test,  $p = 0.002$ ). Furthermore, when the participation probability is high ( $q = 0.8$ ), increasing the maximal group size from  $n = 3$  to  $n = 6$  leads, as predicted, to a significant decrease in average investment, from 28.86 to 20.83 (Wilcoxon rank-sum test,  $p = 0.037$ ). Both of these comparative statics are robust to considering only the last 15 rounds (46 - 60) of Part 3.

We also find strong confirmation for the comparative statics with respect to the participation probability  $q$ . When  $n = 3$ , increasing the participation probability from  $q = 0.2$  to  $q = 0.8$  leads to a significant increase in average investment from 4.40 to 28.86 (Wilcoxon rank-sum test,  $p = 0.001$ ). On the other hand, when  $n = 6$ , we find that there is no significant difference between average investment for  $q = 0.2$  and  $q = 0.8$  (Wilcoxon rank-sum test,  $p = 0.406$ ), which is consistent with the theoretical prediction.

In both Figure A.1 and Table A.1, we provide the NE predictions for the purposes of comparison. As for the analysis of Part 2, we find little evidence in support of overbidding relative to the NE investments, in stark contrast to the majority of the experimental contests literature. Recall that for the sequence of decisions in Part 2, the only case in which we observed any significant overbidding is the 6High treatment. In the sequence of decisions in Part 3, there is no significant overbidding in any treatment condition. In fact, in the 3Low treatment, we even find significant underbidding, relative to the NE prediction. The one-sample Wilcoxon signed-rank tests comparing the average investments in rounds 31-60 to the NE predictions produce the two-sided  $p$  values of 0.263, 0.398 and 0.128 for treatments 3High, 6Low and 6High. For the 3Low treatment, the two-sided  $p$  value is 0.018, but indicates significant *underbidding* relative to the NE prediction.

Comparing these findings in Part 3, with our results from Part 2, we see that the confirmation of all four comparative statics is driven by the significant order effects for the 6High treatment condition. In particular, the substantial overbidding in 6High is not observed after subjects have already experienced the 6Low treatment condition in Part 2. One possible explanation for this order effect is that subjects form payoff aspirations after experiencing treatment condition 6Low for 30 rounds, which causes them to curtail their investment levels in the second sequence of decisions (see, e.g., Huck et al., 2007).

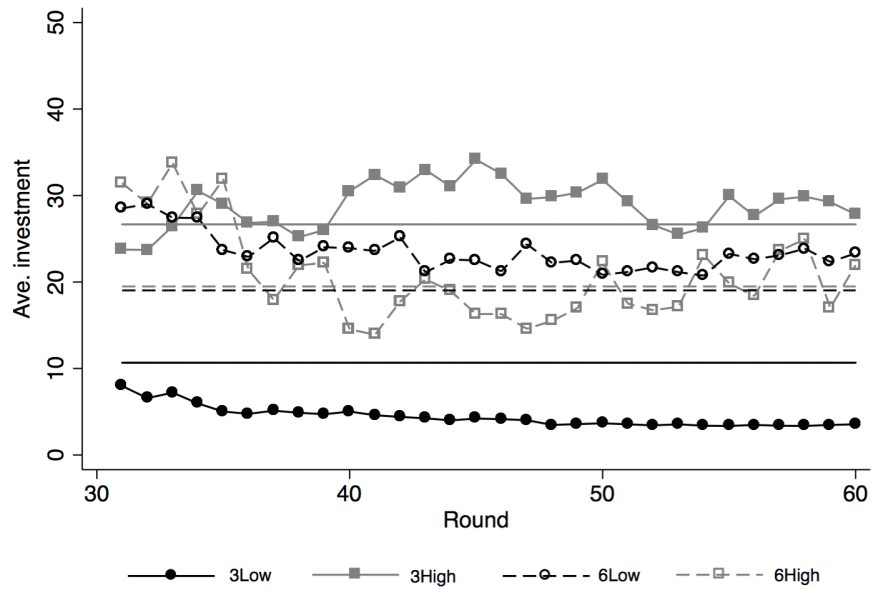


Figure A.1: Average investment by treatment: Periods 31 - 60.

Treatment	Ave. investment: 31-60	Ave. investment: 46-60	NE investment
3Low	4.40 (0.50)	3.56 (0.61)	10.67
3High	28.86 (2.54)	29.05 (2.84)	26.66
6Low	23.46 (4.25)	22.29 (3.95)	19.03
6High	20.83 (1.74)	19.06 (2.04)	19.49

Table A.1: Average individual investment in Part 3, by treatment, with robust standard errors in parentheses, clustered by matching group.



## A.2 QRE behavior in contests with stochastic group size

In this section, we summarize the implications of Quantal Response Equilibrium (QRE) behavior in our setting. For brevity, the theory is summarized for the contest with  $n = 3$  potential players, each with participation probability  $q$ . The expressions for  $n = 6$  are similar.

Let  $B$  be a discretized set of available investment choices, and let  $p(b)$  denote the symmetric QRE probability of investing  $b \in B$  by an active player. Then, the symmetric (logit) QRE with parameter  $\lambda$  can be obtained by solving the following system of equations:

$$p(b) = \frac{\exp[\lambda\pi_e(b)]}{\sum_{b' \in B} \exp[\lambda\pi_e(b')]}, \quad (\text{A.1})$$

$$\pi_e(b) = \sum_{i_2, i_3 \in \{0,1\}} \sum_{b_2, b_3 \in B} q^{i_2+i_3} (1-q)^{2-i_2-i_3} \frac{Vb}{b + i_2 b_2 + i_3 b_3} p(b_2) p(b_3) - b, \quad (\text{A.2})$$

where  $\pi_e(b)$  represents the equilibrium expected payoff of a player investing  $b$ , conditional on being active in the contest. The noise parameter  $\lambda$  captures the degree to which mistakes are made, with  $\lambda = 0$  corresponding to completely random behavior and  $\lambda \rightarrow \infty$  to the NE.

Figure A.2 displays average QRE investments  $b^{QRE} = \sum_{b' \in B} b' p(b')$  as a function of the noise parameter  $\lambda$  for each combination of participation probability  $q \in \{0.2, 0.8\}$  and maximum possible group size  $n \in \{3, 6\}$ . For each calculation, we normalized the prize to  $V = 1$  and used a discretized investment space  $B = \{0.00, 0.05, 0.10, \dots, 1.00\}$ . The corresponding set of QRE densities  $\{p(0.00), p(0.05), \dots, p(1.00)\}$  are obtained by iteratively and simultaneously solving the system of equations defined by Eq. (A.1) and Eq. (A.2). Consistent with other studies, we find that, in all treatments,  $b^{QRE}$  approaches the NE investment level from above. Furthermore, note that for each  $\lambda$ , the ranking of the QRE predictions matches that of the ranking of our NE predictions. As such, the comparative statics predictions between treatments are the same even when we allow for bounded rationality consistent with quantal response behavior.

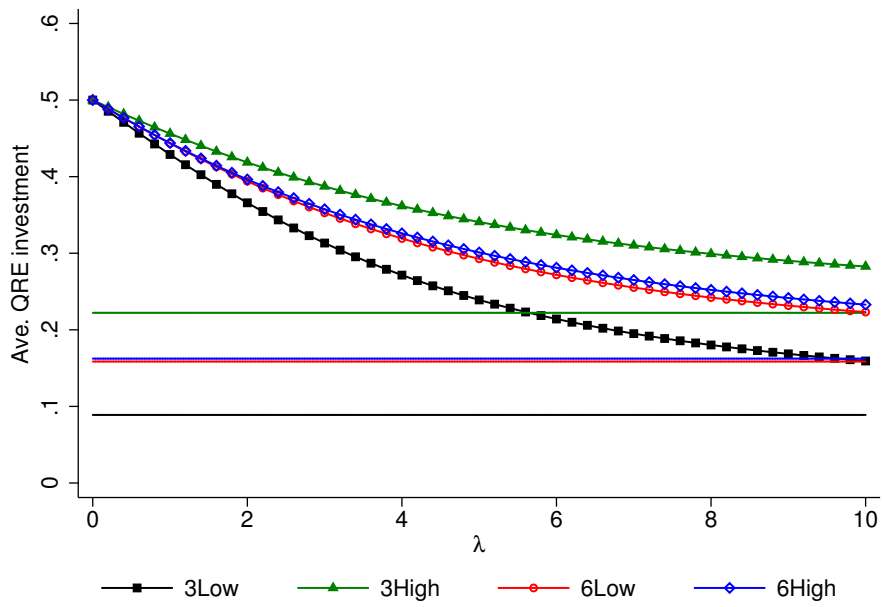


Figure A.2: Average QRE investment by treatment for various levels of noise,  $\lambda$ . The solid lines show the corresponding NE investments.

## B Experimental instructions

Each experimental session consisted of four parts. Following a brief introduction by the experimenter, instructions for part 1 were distributed, read aloud, and then subjects made decisions for that part. This procedure continued for all other parts. Below, we reproduce instructions for part 2 of the experimental session for treatment *3LH* and exclude instructions for the other three treatments, as well as part 3 instructions, due to the similarities. Instructions for part 1 – risk, ambiguity, and loss aversion elicitation – are standard and available upon request. For part 4, subjects were told that they would continue to make investment decisions in an environment similar to part 2 and part 3, with the exception that they will now be able to see investments made by other participants which were randomly selected from a previous round. Instructions for part 3 and 4 are also available upon request.

### Part 2

All amounts in this part of the experiment are expressed in points. The exchange rate is 100 points = \$1 or 1 point = \$0.01.

This part of the experiment consists of a sequence of decision rounds.

#### Endowment and investment

In each round, you will be given an endowment of 120 points. You can invest any integer number of points from 0 to 120 into a project. Any points you do not invest, you get to keep. The project can either succeed or fail. If your project succeeds, you will receive 120 points of revenue for the round. If your project fails, you will not receive any revenue for the round.

#### What is the likelihood that your project succeeds?

After you have made your investment decision, the outcome of your project will be determined. In each round, the probability that your project succeeds is determined as follows.

(1) First, the computer program randomly forms **Your Group** from the other participants in the experiment.

- The size of **Your Group** (including you) can be 1, 2, or 3.
- The program will randomly determine the size of **Your Group** according to the probabilities in Table 1. This table will also be visible on your screen.
- The other players in **Your Group** (if there are any) will be randomly chosen.

Table 1: Probabilities for the Size of **Your Group**

Size of <b>Your Group</b>	1	2	3
Probability	64.000%	32.000%	4.000%

(2) Second, the sum of the project investments made by all members of **Your Group** is computed. Then, **the probability that your project succeeds** is given by:

$$\frac{\text{Number of points you invested in your project}}{\text{Sum of the points invested in projects by all members of **Your Group**}}$$

For example, suppose you invested 10 points, and then the size of **Your Group** is randomly determined to be 2. If the randomly chosen other member of **Your Group** invested 20 points, then the probability that your project succeeds is

$$\frac{10}{10 + 20} = \frac{10}{30} = \frac{1}{3} = 33.33\%.$$

For another example, suppose you invested 10 points, and then the size of **Your Group** is randomly determined to be 3. If the two randomly chosen other members of **Your Group** invested 5 points and 25 points, then the probability that your project succeeds is

$$\frac{10}{10 + 5 + 25} = \frac{10}{40} = \frac{1}{4} = 25.00\%.$$

Lastly, if the size of **Your Group** is randomly determined to be 1, then your project **always** succeeds (the probability is 100%), regardless of how much you invested.

### Payoff in a given round

After determining the size of **Your Group** and the probability that your project succeeds, the software program will randomly determine whether your project succeeds or not, according to the calculated probability.

Then your **individual payoff** in the round is determined as follows:

If your project succeeds:	If your project fails:
+120 (endowment)	+120 (endowment)
+120 (revenue)	+0 (no revenue)
− (points you invested)	− (points you invested)
<hr style="width: 100%; border: 0; border-top: 1px solid black; margin-bottom: 5px;"/> 240 − <b>(points you invested)</b>	<hr style="width: 100%; border: 0; border-top: 1px solid black; margin-bottom: 5px;"/> 120 − <b>(points you invested)</b>

## How are your earnings from this part determined?

You will participate in a series of many decision rounds. At the end of the series, **five** of these rounds will be chosen randomly (with all rounds being equally likely to be chosen). At the end of the experiment, you will be informed about which five rounds were chosen and your payoff from each of those five rounds. Then your earnings from this part will be the sum of your payoffs from the five randomly selected rounds.

## Practice stage

Before the actual decision rounds begin, you will participate in an unpaid practice stage that is designed to help you better understand the rules just described to you. In the practice stage, you will not interact with anyone else, and no decisions you make will be shown to anyone else. You will not earn anything from this practice stage – it is only intended to help you understand the instructions.

As in the actual decision rounds, you can choose how many points to invest into your project. In addition, **for this practice stage only**, you can choose the project investments for the other two players who may be selected as members of **Your Group**. In the actual decision rounds, these project investments for the other members of **Your Group** will be the investments that were actually chosen by the other participants.

**Also for this practice stage only**, the computer will calculate the probability that your project succeeds for each of the possible group sizes. This allows you to see what would happen in each case, given the decisions you entered for yourself and the decisions you entered for others. In the actual decision rounds, only **one** of the possible group sizes will be randomly selected by the computer in each round, according to the probabilities shown in Table 1.

Please go ahead and make your decisions on the first practice screen and click CONTINUE when you are done.

At the top of the second practice screen, you can see a summary of the investment decisions you entered for yourself and for the other two players. Next to that, you can also see the probability table for the possible sizes of **Your Group**.

Below these, there are 3 panels. The left panel shows you what you would see if the computer randomly determined that the size of **Your Group** is **1**. The table in the panel shows you the Investment made by each Group Member in **Your Group** (which in this case, is only You). Below the table, the probability your project succeeds is calculated and labeled as ‘Your Probability of Success’. In this case, when the size of **Your Group** is **1**, the probability that your project succeeds is always 100%.

The middle panel shows you what you would see if the computer randomly determined that the size of **Your Group** is **2**. In the actual experiment, the other member of **Your Group** will be randomly selected from among the other participants. In this practice stage, it may be either of the hypothetical group members for whom you made the investment decisions. The table in the panel shows you the Investment made by each Group Member in **Your Group**. As in the left panel, below the table, the probability your project succeeds is calculated and labeled as ‘Your Probability of Success’. In this case, when the size of **Your Group** is **2**, the probability that your project succeeds is given by dividing your Investment by the sum of your Investment and the Investment of the other member of **Your Group**.

Finally, the right panel shows you what you would see if the computer randomly determined that the size of **Your Group** is **3**. In the actual experiment, the other members of **Your Group** will be randomly selected from among the other participants. In this practice stage, the other group members will be the hypothetical ones for whom you made the investment decisions. The table in the panel shows you the Investment made by each Group Member in **Your Group**. As in the other two panels, below the table, the probability your project succeeds is calculated and labeled as ‘Your Probability of Success’. In this case, when the size of **Your Group** is **3**, the probability that your projects succeeds is given by dividing your Investment by the sum of your Investment and the Investments of both other members of **Your Group**.

Remember, in the actual experiment, only one of the panels will be shown to you in each round, corresponding to the group size selected in that round by the computer, according to the probabilities shown in Table 1.

### Recap of this part

In each decision round, you will receive an endowment of 120 points. You must choose how many points to invest in your project. Any points that you do not invest, you can keep. *Remember that you must decide how much to invest in your project before the size of Your Group is determined.* After the investment decisions are made, the program will determine the size of **Your Group** and the probability your project succeeds, based on the investments made by you and the other members of **Your Group** (if any). If your project succeeds, you earn 120 points in revenue, but if it fails, you earn 0 revenue. There will be a large number of decision rounds, and at the end of the experiment, you will be paid your earnings from 5 randomly selected rounds.

In a moment, you will start on the actual decision rounds for this part. Please do not communicate with other participants or look at anyone else’s monitor. If you have a question or problem, from this point on, please simply raise your hand so that one of us can assist you in private. Please remember to click CONTINUE to proceed.