Contests with a stochastic number of players: Experimental evidence

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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), the unique symmetric equilibrium investment level in this setting can exhibit non-monotonicity 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. In stark contrast to the experimental literature on contests with certain group size, where overbidding relative to equilibrium is widely documented, we find remarkable agreement between the observed average investment and the equilibrium investment levels in all but one treatment.

Keywords: contest, stochastic number of players, experiment

JEL classification codes: C72, C91, D72, D82

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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.\(^1\) While in most contest models it is assumed that the number of contestants is common knowledge, it is often the case that when a contest participant has to decide on how much to invest in the contest she is not aware of how many other competitors she will face. In games with a very large number of players, group size uncertainty is both an appealing and realistic feature. However, even in contests with few potential players, the typical assumption that group size is commonly known can be too strong. Examples of these smaller environments include lobbyists competing in a rent-seeking contest, firms engaged in an R&D race, or job candidates applying for a position.

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 spending derived by Lim and Matros (2009) for Tullock (1980) lottery contests with a stochastic number of players.

Lim and Matros (2009) study lottery contests in which the number of players is a binomial random variable. In their model, there is a fixed pool of \(n\) potential players, each with independent probability \(q \in (0, 1]\) of participating. There is a unique symmetric equilibrium that exhibits two key features. First, for any fixed number of potential players \(n > 2\), individual equilibrium spending is single-peaked in the probability of participation \(q\). Second, for any two values of \(n\), the individual equilibrium spending 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 spending. 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 spending is very low. As \(n\) increases, group sizes larger than one become more likely and the equilibrium spending 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 spending as in standard contests where the number of players is known, i.e., the spending 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 spending is increasing in \(n\) for the low value of \(q\) and decreasing in \(n\) for the high value of \(q\). For each \(n\), we also vary the participation probability within subjects, accounting for possible order effects.

To the best of our knowledge, this is the first experimental study of contests with a stochastic number of players. Our experimental design has two important novel features.

\(^1\)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).
First, instead of informing subjects about the participation probability, $q$, we inform them directly about the probabilities, $q_m$, of different realizations of random group size $m \in \{1, \ldots, n\}$. We believe this information is easier for subjects to understand and 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). Second, 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 study is related to the earlier theoretical literature on contests with a stochastic number of participants (Münster, 2006; Myerson and Wärneryd, 2006; Fu, Jiao and Lu, 2011; Kahana and Klunover, 2015, 2016) and endogenous entry (Fu and Lu, 2010). Morgan, Orzen and Sefton (2012) explored endogenous entry in contests experimentally and found, in line with the theoretical predictions, that individual spending increases with the value of the prize. Games with population uncertainty have also been studied in other environments, including auctions\(^2\) as well as voting and public goods settings modeled as Poisson games.\(^3\)

We find remarkable agreement between the theory and our experimental data, especially in terms of the point predictions. Our main results are presented based on subject behavior in an initial 30-period sequence of decisions with a fixed treatment. With the exception of the treatment where $n = 6$ and $q = 0.8$, average investment is extremely close to the risk-neutral Nash equilibrium level, in stark contrast with the literature on lottery contests with certain group size, where overbidding has been widely documented (Sheremeta, 2013). Overall, we find strong support for at least two out of the four comparative static predictions and three out of the four point predictions.

Furthermore, in a second 30-period sequence of decisions, subject behavior provides even stronger support for the theory. All four of the comparative static predictions (with respect to maximal group size and participation probability) are confirmed by the data. In fact, we find no evidence of overbidding in terms of average investment for any of the treatments, and even find significant underbidding in the treatment with $n = 3$ and $q = 0.2$. Using the within-subject variation in our experiment, we also find broad support for the comparative static predictions with respect to $q$, except when $n = 6$ and the subjects experienced $q = 0.8$ before $q = 0.2$.

We also explore the possibility that overbidding in the experiment can be explained,


in part, by bounded rationality. To do so, at the end of the experiment, we asked subjects
to make an investment decision in a control task where the strategic uncertainty regarding
other potential players’ investments is removed. We compute the theoretical best response
for each subject and compare their responses in order to provide a basic measure of their
ability to best respond. We find that subjects who tended to overbid in the control task
(i.e. without strategic uncertainty) also overbid more (relative to the Nash equilibrium)
in the main part of the experiment. Furthermore, the tendency to overbid in the control
task is significantly higher in the treatment where \( n = 6 \) and \( q = 0.8 \), which is also the
only treatment where we found overbidding in the initial 30-period sequence of decisions.

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 and
discussed in Section 4, and Section 5 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 \( i \in N = \{1, \ldots, n\} \), 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
\( (x_i, x_M) \in R_+ \times R_+^{|M|} \), 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

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

The winner of the contest receives a prize \( V > 0 \). Thus, the expected payoff of a participat-
ing 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 Nash
equilibrium, with individual investments given by the following proposition.

**Proposition 1 (Lim and Matros (2009))** The contest with a random group size has
a unique symmetric pure-strategy Nash equilibrium where each player’s investment is

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

Equation (2) implies nontrivial comparative statics of individual equilibrium invest-
ment 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 non-monotonic.
in $q$, with a single peak $q^*(n) \in (0, 1)$ 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 $q$ 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 spending that is similar to the effect of an increase in group size in a standard contest with certain 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 Proposition 1, the equilibrium individual investments, $x^*(n, q)$, under these parameters are

$$
  x^*(3, 0.2) = 10.67, \quad x^*(3, 0.8) = 26.66,
  x^*(6, 0.2) = 19.03, \quad x^*(6, 0.8) = 19.49.
$$

(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 spending 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 174 subjects (59.2% 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. Eight sessions were conducted, and each subject participated in only one
session. A session lasted approximately 70 minutes, with subjects earning $26.52, on average, including a $7.00 show-up fee. 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 A.\footnote{All other instructions not reproduced in Appendix A are straightforward and available from the authors upon request.}

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).\footnote{In each case, subjects were presented with a list of 20 choices between a sure amount of money and a lottery with two outcomes. The probabilities of the lottery outcomes 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 lottery to preferring a sure amount. Our measures for risk aversion (RA), ambiguity aversion (AA) and loss aversion (LA) were constructed using the row numbers where subjects switched. Although we control for these measures when examining the dynamics of individual behavior in the main part of the experiment (see Section 4.4), they provide little explanatory power and do not affect the main findings.} 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.

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.\footnote{When subjects were making decisions in Part 2 they were not yet informed about the existence of or instructions for Part 3.} Thus, comparisons of behavior in Part 2 across treatments will allow us to test for treatment effects between subjects, whereas comparisons between Part 2 and Part 3 will provide within-subject reactions to changes

<table>
<thead>
<tr>
<th>Treatment</th>
<th>$n$</th>
<th>$q$ (Part 2)</th>
<th>$q$ (Part 3)</th>
<th>Sessions</th>
<th>Subjects</th>
<th>Groups</th>
</tr>
</thead>
<tbody>
<tr>
<td>3LH</td>
<td>3</td>
<td>0.2</td>
<td>0.8</td>
<td>2</td>
<td>48</td>
<td>8</td>
</tr>
<tr>
<td>3HL</td>
<td>3</td>
<td>0.8</td>
<td>0.2</td>
<td>2</td>
<td>42</td>
<td>7</td>
</tr>
<tr>
<td>6LH</td>
<td>6</td>
<td>0.2</td>
<td>0.8</td>
<td>2</td>
<td>42</td>
<td>7</td>
</tr>
<tr>
<td>6HL</td>
<td>6</td>
<td>0.8</td>
<td>0.2</td>
<td>2</td>
<td>42</td>
<td>7</td>
</tr>
</tbody>
</table>

Table 1: Summary of experimental treatments.
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.

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, $q_m = \binom{n-1}{m-1}q^{m-1}(1-q)^{n-m}$, for each possible group size $m \in \{1,\ldots,n\}$. These probabilities are shown in Table 2 for each combination of treatment parameters $n$ and $q$. 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.

### Table 2: Probabilities, $q_m$, for each possible group size $m \in \{1,\ldots,n\}$ for the different values of $n$ and $q$

<table>
<thead>
<tr>
<th>Group Size</th>
<th>$q = 0.2$</th>
<th>$q = 0.8$</th>
</tr>
</thead>
<tbody>
<tr>
<td>$m$</td>
<td>$n = 3$</td>
<td>$n = 6$</td>
</tr>
<tr>
<td>1</td>
<td>0.64</td>
<td>0.32768</td>
</tr>
<tr>
<td>2</td>
<td>0.32</td>
<td>0.4096</td>
</tr>
<tr>
<td>3</td>
<td>0.04</td>
<td>0.2048</td>
</tr>
<tr>
<td>4</td>
<td>0.0512</td>
<td>0.2048</td>
</tr>
<tr>
<td>5</td>
<td>0.0064</td>
<td>0.4096</td>
</tr>
<tr>
<td>6</td>
<td>0.00032</td>
<td>0.32768</td>
</tr>
</tbody>
</table>
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. In this part, subjects made two investment decisions: one in a setting similar to Part 2 and the other in a setting similar to Part 3. However, at the time of making their investment decision, subjects could now see the investment decisions that would be implemented for the \( n - 1 \) other members who may be potentially selected as members of their contest group. Subjects were told that these were the decisions made by other participants at an earlier point in the experiment.\(^7\) Subjects were still unaware of the actual size of their group at the time of making their investment decisions, and, therefore, did not know which of the \( n - 1 \) investments would be used to calculate the probability of project success. Thus, when making their decisions subjects faced all the features of the original game with the exception of strategic uncertainty. The goal of this part was to assess the ability of experienced subjects to determine best responses to others’ investments. One of the two decisions from Part 4 was randomly selected and used to calculate actual earnings that were added to the final earnings at the exchange rate of $1=100 points. At the conclusion of the experiment, subjects were paid anonymously 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 from their matching group without replacement, to calculate their probability of success in the current round.\(^8\) As a result,

\(^7\)The revealed investment decisions were taken from round 20 of Part 2 and round 20 of Part 3. We believe decisions in that round are more indicative of the behavior of informed, experienced subjects, as they have had ample time to learn about the contest environment.

\(^8\)Note that, in this way, player \( i \)'s payoff is unaffected by the payoff calculation for another player \( j \), even if \( i \)'s investment was used to determine player \( j \)'s probability of success.
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, we report summary statistics and examine the
comparative static predictions described in Section 2 using only between-subject compar-
isons of the data from the first 30 rounds of contests (Part 2 of the experiment). We
also compare average investments with the Nash equilibrium point predictions for each
treatment. Second, for robustness, we conduct the same analysis using the data from the
second 30 rounds of contests (Part 3 of the experiment).\textsuperscript{9} Third, using the within-subject
analysis, we examine the effect of changing the participation probability \(q\), for each maxi-
mal group size. We then analyze the dynamics of individual behavior in Part 2 over time,
controlling for various individual and demographic characteristics. Finally, we examine
subjects’ decisions in Part 4 of the experiment, in the absence of strategic uncertainty.
We compute a measure of their abilities to best respond and test whether this measure
can be used to explain some of the observed variation in individual behavior.
Throughout this Section, all standard errors are clustered at the matching group level
and matching groups are used as the unit of analysis for non-parametric tests.

4.1 Average investment in Part 2

For the analysis of Part 2, we will refer to treatments \(nLH\) as \(nLow\) and to treatments \(nHL\)
as \(nHigh\), where Low and High refer to the participation probability \((q = 0.2, q = 0.8)\)
used in Part 2 only. Figure 1 shows the average individual investment across all 30 rounds
for each treatment, along with the corresponding Nash equilibrium (NE) predictions. As
seen from the figure, in all treatments except \(6High\), the average investment converges
closely to the NE predictions by round 15. This agreement with the point predictions is
in stark contrast to the widely documented phenomenon of overbidding in lottery contest
experiments with certain group size (Sheremeta, 2013).

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, the
table also provides the NE predictions. The \(t\)-tests comparing the average investments in
rounds 16-30 to the NE predictions produce the two-sided \(p\) values of 0.976, 0.870, 0.575
and 0.001 for treatments \(3Low\), \(3High\), \(6Low\) and \(6High\), respectively.

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

\textsuperscript{9}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
results of comparisons of Part 3 decisions across different treatments should be interpreted with caution.
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 1** (a) Average investment agrees with NE predictions in all treatments except 6High.

(b) Investment increases in \( n \), as predicted, for low \( q \), but does not change, contrary to the prediction (reduction), for high \( q \).

(c) 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.

Recall that the reduction of equilibrium investment with $n$ when $q$ is high is driven primarily by the standard group size effect in contests where group size is certain. As group size increases, each player’s probability of winning, and hence expected marginal revenue of investment, goes down. However, the documented behavior in experimental contests with certain group size does not support this prediction. The effect of group size on contest spending was first explored by Anderson and Stafford (2003), who 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.

In Section 4.5, we explore the possibility that behavior in the treatment with $n = 6$ and $q = 0.8$ may also be explained by some degree of bounded rationality on the part of the subjects. In particular, we compare the subjects’ abilities to best respond when strategic uncertainty is removed across treatments, and show that the tendency to overbid relative to the best response is significantly higher for the treatment with $n = 6$ and $q = 0.8$.

One of the standard approaches explaining overbidding in contests 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. Parameter $\lambda$ of the QRE determines the sharpness of this dependence, with $\lambda = 0$ corresponding to completely random behavior (the uniform distribution over the strategy space) and $\lambda \to \infty$ to the Nash equilibrium where each player chooses best response with probability one. We computed the QRE for various values of $\lambda$ in our setting and found, similar to the QRE for contests with certain group size, that the ranking of average QRE effort by treatment is the same as the ranking of NE effort, overbidding is similar across treatments, and average effort converges to the NE from above as $\lambda$ increases. Thus,

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Ave. investment: 1-30</th>
<th>Ave. investment: 16-30</th>
<th>NE investment</th>
</tr>
</thead>
<tbody>
<tr>
<td>3Low</td>
<td>13.05 (1.16)</td>
<td>10.71 (1.22)</td>
<td>10.67</td>
</tr>
<tr>
<td>3High</td>
<td>29.95 (3.29)</td>
<td>27.32 (3.86)</td>
<td>26.66</td>
</tr>
<tr>
<td>6Low</td>
<td>21.09 (1.98)</td>
<td>17.32 (2.89)</td>
<td>19.03</td>
</tr>
<tr>
<td>6High</td>
<td>34.34 (1.66)</td>
<td>31.01 (1.66)</td>
<td>19.49</td>
</tr>
</tbody>
</table>

Table 3: Average individual investment in Part 2, by treatment, with robust standard errors in parentheses, clustered by matching group.
QRE cannot explain why we observe overbidding in 6High but not in the other three treatments.  

4.2 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 2, 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 4.

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, $p = 0.037$). 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

---

10Details are available from the authors upon request.
significant decrease in average investment, from 28.86 to 20.83 (Wilcoxon rank-sum test, $p = 0.002$). 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, $p = 0.41$), which is consistent with the theoretical prediction.

In both Figure 2 and Table 4, we provide Nash equilibrium predictions for the purposes of comparison. As for the analysis of Part 2, we find little evidence in support of overbidding relative to the Nash equilibrium 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 Nash equilibrium prediction. The $t$-tests comparing the average investments in rounds 31-60 to the NE predictions produce the two-sided $p$ values of 0.415, 0.337 and 0.468 for treatments 3High, 6Low and 6High. For the 3Low treatment, the two-sided $p$ value is 0.000, but indicates significant underbidding relative to the Nash equilibrium prediction.

**Result 2** Using the sequence of decisions made in Part 3 of the experiment,

(a) Average investment agrees with NE predictions in all treatments except 3Low, where we find significant underbidding.

(b) Investment increases in $n$, as predicted, for low $q$, and decreases in $n$, as predicted, for high $q$.

(c) Investment increases in $q$, as predicted, for low $n$, and does not change with $q$, as predicted, for high $n$.
### 4.3 Within-subject responses to changes in \( q \)

Using the within-subject variation, we can also examine the comparative statics predictions with respect to the participation probability, \( q \). Theory predicts that for \( n = 3 \), an increase in \( q \) from low (0.2) to high (0.8) should lead to an increase in individual investment (from 10.67 to 26.66). On the other hand, for \( n = 6 \), the theory predicts no difference in individual investment for the different participation probabilities.

As expected, for \( n = 3 \), we find that average investment is significantly higher for \( q = 0.8 \) than for \( q = 0.2 \), regardless of whether subjects experienced \( q = 0.2 \) first (Sign-rank test, \( p = 0.000 \)) or \( q = 0.8 \) first (Sign-rank test, \( p = 0.000 \)). Similarly, as expected for \( n = 6 \), we find no difference between average investment when the subjects experienced \( q = 0.2 \) before experiencing \( q = 0.8 \) (Sign-rank test, \( p = 0.441 \)). However, when subjects experienced \( q = 0.8 \) before \( q = 0.2 \), we find significantly lower average investment for \( q = 0.2 \) (Sign-rank test, \( p = 0.000 \)), consistent with the observed overbidding for the 6High treatment in Part 2 of the experiment.

Regression-based tests paint a similar picture, as shown in Table 5. For \( n = 3 \), the coefficient on Part 3 indicator shows that investment levels are significantly higher for \( q = 0.8 \), regardless of which \( q \) the subjects faced first. On the other hand, for \( n = 6 \), investment levels are higher (at the 5% level of significance) for the high participation probability when subjects faced \( q = 0.8 \) before \( q = 0.2 \), but not different when they faced \( q = 0.2 \) first.

### 4.4 Dynamics of individual investment

In this section, we explore the dynamics of individual investment in Part 2 of the experiment. Table 6 reports the OLS estimates from several regression models. In all specifications, the dependent variable is the individual investment made by subject \( i \) in round \( t \). Model (1) provides baseline evidence of the persistence in individual investment by including the lagged value of individual investment as an explanatory variable. As seen from the coefficient estimate on \( \text{effort}_{it-1} \), individual effort is fairly persistent; that is, most subjects establish relatively fixed effort “types.”

<table>
<thead>
<tr>
<th>( \text{effort}_{it} )</th>
<th>3LH</th>
<th>3HL</th>
<th>6LH</th>
<th>6HL</th>
</tr>
</thead>
<tbody>
<tr>
<td>Part 3</td>
<td>15.82***</td>
<td>-25.55***</td>
<td>-0.260</td>
<td>-10.88**</td>
</tr>
<tr>
<td></td>
<td>(2.890)</td>
<td>(3.600)</td>
<td>(1.810)</td>
<td>(3.674)</td>
</tr>
<tr>
<td>Constant</td>
<td>13.05***</td>
<td>29.95***</td>
<td>21.09***</td>
<td>34.34***</td>
</tr>
<tr>
<td></td>
<td>(1.158)</td>
<td>(3.287)</td>
<td>(1.985)</td>
<td>(1.662)</td>
</tr>
</tbody>
</table>

\( N \)

|     | 2880 | 2520 | 2520 | 2520 |

Table 5: Within-subject effect of the participation probability, \( q \), on individual investment, \( \text{effort}_{it} \). Standard errors in parentheses are clustered by matching group. Significance levels: * \( p < 0.10 \), ** \( p < 0.05 \), *** \( p < 0.01 \).
Table 6: OLS regression results for the dynamics of individual investment, \( \text{effort}_{it} \), in the first 30 rounds of contests. Standard errors in parentheses are clustered by matching group. Significance levels: * \( p < 0.10 \), ** \( p < 0.05 \), *** \( p < 0.01 \).

We also explore the possibility that subjects might adjust their investment decisions in reaction to the realized outcomes in past periods. The most straightforward approach
is to estimate the effects of three different factors that correspond to the previous period’s outcome observed by the subject, cf. Model (2). The binary variable \( \text{win}_{it-1} \) takes the value 1 if the subject’s project was successful in the previous period, and 0 otherwise, while \( \text{groupsize}_{it-1} \) represents the realized size of the subject’s group in the previous period. The third variable, \( \text{pwin}_{it-1} \), is the probability that subject \( i \)’s project was successful in the previous period, given her own investment, the realized size of her group, and the investments chosen by the other members of her group (if there were any). None of the variables produces a statistically significant effect in Model (2). Note, however, that the three factors are likely correlated with one another. For Model (3), we select \( \text{pwin}_{it-1} \) as the explanatory variable to represent the combined information conveyed by the lagged outcome, lagged group size, and lagged average of others’ investments.\(^{11}\) We also include the interaction term between \( \text{pwin}_{it-1} \) and period. The coefficient estimates for Model (3) suggest that the lagged probability of success has a significantly negative effect on individual investment. However, the effect diminishes over time, and becomes insignificant after 14 periods.

In Model (4), we also interact \( \text{pwin}_{it-1} \) with the treatment dummies to see if there are any significant differences in the effects of the lagged probability of success across treatments. Tests on the coefficients reveal that the negative effect of \( \text{pwin}_{it-1} \) is only significant for the treatments where \( n \) is low (\( n = 3 \)), and even then, the effects become insignificant after approximately 10 periods. We check the robustness of these results in Model (5), which uses only the data for investment by subjects who were not the only members of their group in the previous period.\(^{12}\) With this restriction, the effect of \( \text{pwin}_{it-1} \) is only significant in the treatment with \( n = 3 \) and \( q = 0.8 \) (3High) and becomes insignificant after just a few periods. Thus, although it appears as though subjects in the treatments with low maximal group size (\( n = 3 \)) respond to previous period outcomes in the early rounds of the experiment, the effects diminish and become insignificant over time. Generally, the negative effect of \( \text{pwin}_{it-1} \) and its reduction over time are consistent with standard models of reinforcement learning (e.g., Roth and Erev, 1995); however, in our experiment reinforcement learning appears to play a relatively modest role.

We also explored specification (5) with added controls for gender and for the incentivized measures of risk aversion, loss aversion, and ambiguity aversion collected in Part 1 of the experiment. Our measure of ambiguity aversion has a significant, negative effect on individual investment (\( p = 0.038 \)), although nothing changes qualitatively for the coefficient estimates on the other explanatory variables. Accordingly, we omit the specification including these demographic controls from Table 6.

\(^{11}\)We also estimated a model controlling for the average investment of others in the previous period. However, the results are similar to what we find by including only \( \text{pwin}_{it-1} \), and so we do not report them here.

\(^{12}\)If a subject was the only member of her group in the previous period, then regardless of investment, her probability of success would have been equal to 100%.
Table 7: OLS regression results for overbid relative to theoretical best response in Part 4. Standard errors in parentheses are clustered by matching group. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

<table>
<thead>
<tr>
<th>overbidBR$_i$</th>
<th>Part 2</th>
<th>Part 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>(1)</td>
<td>(2)</td>
<td></td>
</tr>
<tr>
<td>3High</td>
<td>-3.398</td>
<td>16.826***</td>
</tr>
<tr>
<td></td>
<td>(5.601)</td>
<td>(4.361)</td>
</tr>
<tr>
<td>6Low</td>
<td>2.308</td>
<td>14.938***</td>
</tr>
<tr>
<td></td>
<td>(4.724)</td>
<td>(1.631)</td>
</tr>
<tr>
<td>6High</td>
<td>16.510**</td>
<td>13.933**</td>
</tr>
<tr>
<td></td>
<td>(6.748)</td>
<td>(5.784)</td>
</tr>
<tr>
<td>Constant</td>
<td>9.197***</td>
<td>-1.396**</td>
</tr>
<tr>
<td></td>
<td>(2.507)</td>
<td>(0.670)</td>
</tr>
</tbody>
</table>

4.5 Best response ability without strategic uncertainty

In this section, we analyze subjects’ responses to the control task implemented as Part 4 of the experiment. For this task, we eliminated strategic uncertainty about the investments that would be used for any other potential group members, while retaining the uncertainty about the actual group size at the time of the investment decision. Subjects made two decisions – one for the parameters used in Part 2, and one for the parameters used in Part 3. For each subject, we computed the corresponding theoretical best response to the investments of the other potential group members. We then used the subjects’ effort choices in Part 4 to construct a measure of their best response ability, by calculating the amount by which they overbid, relative to the theoretical best response.

First, we focus on subjects’ tendency to overbid in the control tasks, for each treatment, separated by whether or not the treatment was implemented in Part 2 or Part 3. Table 7 reports the coefficient estimates for the effects of treatment on overbidding relative to the theoretical best response, which is denoted by overbidBR$_i$ for subject $i$. The first column reports the effects of the treatments using sessions in which they were implemented during Part 2, while the second column reports the effects using sessions in which they were implemented during Part 3. The results indicate that, for treatments implemented in Part 2, subjects displayed overbidding relative to the best response in all four treatments. However, the overbidding is significantly larger in the 6High treatment. This corresponds very nicely with our results on average investment in Part 2, where we found that average investment converged closely to the Nash equilibrium predictions in all treatments except 6High.

A possible explanation for the difference is that the 6High treatment generates group size probabilities that are heavily skewed towards groups of size 3 or greater. In this sense,
the environment is closer to the standard contest environment (with known group size of at least 2) than in the other treatments, where the prospect of being in a group of one (or two) is non-negligible. As a result, we might expect there to be more overbidding in the 6High treatment, just as there is consistent overbidding in the experimental literature on contests with known group size.

Shifting the focus to the second column in Table 7, we find that when subjects have already had the experience of playing a 30-period sequence of contests, the results are quite different. Specifically, the tendency to overbid relative to the theoretical best response is not significantly different for the 6High treatment, when subjects participated in 6High during Part 3 of the experiment. Instead, we find that the treatment effects are significant (and similar) for 3High, 6Low, and 6High, while subjects actually underbid relative to the theoretical best response in the 3Low treatment. This also mirrors the qualitative differences we observe in relation to average investment in Part 3 of the experiment, where we found no overbidding (relative to the Nash equilibrium) except in 3Low, where subjects exhibited significant underbidding. To summarize, we find that our measure of best response ability correlates nicely with the results presented in Section 4.1 and Section 4.2 regarding average investment.

Second, to provide more direct support, we also estimate the explanatory power of our best response measure for overbidding (relative to the Nash equilibrium) at the individual level in the main parts of the experiment, overbidBRi. The results are reported in Table 8. In Part 2 of the experiment, our measure of best response ability (overbidBRi) explains a significant amount of the individual overbidding in Part 2. Furthermore, the effect does not differ across treatments.

On the other hand, in Part 3, the best response measure has a slightly different effect, depending on the treatment. For instance, in the 3Low treatment, on average, overbidBRi is negative, which means that the positive, significant coefficient strongly explains the underbidding relative to Nash equilibrium (negative overbidi) observed in the 30-period sequence of contests with treatment 3Low in Part 3. The effect of overbidBRi is not significant for the 6High treatment, although it remains marginally significant for 3High and significant for 6Low.

5 Conclusions

In this paper, we provide 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. These comparative statics can be summarized as follows. First, when the participation probability q is low, increasing the maximal group size n increases equilibrium investment. In contrast, when q is high, an increase in n leads to a decrease in equilibrium investment. Second, an increase in the participation probability q, leads to higher equilibrium investment when n is low, but does not generate a significant difference when n is high.
Table 8: OLS regression results for overbidding relative to Nash equilibrium in Part 2 and Part 3. Standard errors in parentheses are clustered by matching group. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

<table>
<thead>
<tr>
<th></th>
<th>Part 2</th>
<th>Part 3</th>
</tr>
</thead>
<tbody>
<tr>
<td>overbid$_{it}$</td>
<td></td>
<td></td>
</tr>
<tr>
<td>overbidBR$_{it}$</td>
<td>0.234***</td>
<td>0.422***</td>
</tr>
<tr>
<td></td>
<td>(0.079)</td>
<td>(0.088)</td>
</tr>
<tr>
<td>overbidBR$_{it}$ × 3High</td>
<td>0.152</td>
<td>-0.224*</td>
</tr>
<tr>
<td></td>
<td>(0.139)</td>
<td>(0.119)</td>
</tr>
<tr>
<td>overbidBR$_{it}$ × 6Low</td>
<td>0.238</td>
<td>0.172</td>
</tr>
<tr>
<td></td>
<td>(0.200)</td>
<td>(0.127)</td>
</tr>
<tr>
<td>overbidBR$_{it}$ × 6High</td>
<td>-0.086</td>
<td>-0.344***</td>
</tr>
<tr>
<td></td>
<td>(0.111)</td>
<td>(0.122)</td>
</tr>
<tr>
<td>3High</td>
<td>0.826</td>
<td>4.833**</td>
</tr>
<tr>
<td></td>
<td>(2.132)</td>
<td>(1.815)</td>
</tr>
<tr>
<td>6Low</td>
<td>-3.591</td>
<td>2.061</td>
</tr>
<tr>
<td></td>
<td>(2.974)</td>
<td>(3.032)</td>
</tr>
<tr>
<td>6High</td>
<td>10.834***</td>
<td>6.042***</td>
</tr>
<tr>
<td></td>
<td>(3.056)</td>
<td>(1.977)</td>
</tr>
<tr>
<td>Constant</td>
<td>0.229</td>
<td>-5.678***</td>
</tr>
<tr>
<td></td>
<td>(1.140)</td>
<td>(0.725)</td>
</tr>
</tbody>
</table>

| $N$                  | 5220   | 5220   |
| $R^2$                | 0.146  | 0.141  |

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. However, when we examine the behavior of subjects in a second sequence of decisions, we obtain strong confirmation for all four comparative statics. In fact, when subjects have already experienced an initial sequence of decisions, we find no evidence of overbidding in any treatment, and even find significant underbidding in the treatment with $n = 3$ and $q = 0.2$.

One possible explanation for our findings is that subjects (without previous experience) interpret the $n = 6$, $q = 0.8$ treatment as most similar to a standard contest with certain group size. Existing results suggest significant overbidding in these contests, and even insensitivity of actual investment levels to variation in the known group size. In the other three treatments where we find close alignment between theory and average behavior, the possibility of being in a group of size one is more salient, and provides a countervailing influence on the tendency to overbid. In support of this argument, we also explore the possibility that bounded rationality can explain some of the treatment differ-
ences. Using a separate control task, we show that when strategic uncertainty is removed, subjects are significantly worse at determining the best response in the treatment with \( n = 6 \) and \( q = 0.8 \) than in the other three treatments. However, these differences do not emerge when subjects have past experience with the decision environment.

**Acknowledgments**

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**References**


Kahana, Nava, and Doron Klunover. 2016. “Complete rent dissipation when the number of rent seekers is uncertain.” Economics Letters, 141: 8–10.


A  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 3\textit{LH} 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 \textbf{Your Group} from the other participants in the experiment.

- The size of \textbf{Your Group} (including you) can be 1, 2, or 3.

- The program will randomly determine the size of \textbf{Your Group} according to the probabilities in Table 1. This table will also be visible on your screen.

- The other players in \textbf{Your Group} (if there are any) will be randomly chosen.
Table 1: Probabilities for the Size of Your Group

<table>
<thead>
<tr>
<th>Size of Your Group</th>
<th>1</th>
<th>2</th>
<th>3</th>
</tr>
</thead>
<tbody>
<tr>
<td>Probability</td>
<td>64.000%</td>
<td>32.000%</td>
<td>4.000%</td>
</tr>
</tbody>
</table>

(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:

\[
\text{Number of points you invested in your project} \div \text{Sum of the points invested in projects by all members of Your Group}
\]

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:

<table>
<thead>
<tr>
<th>If your project succeeds:</th>
<th>If your project fails:</th>
</tr>
</thead>
<tbody>
<tr>
<td>+120 (endowment)</td>
<td>+120 (endowment)</td>
</tr>
<tr>
<td>+120 (revenue)</td>
<td>+0 (no revenue)</td>
</tr>
<tr>
<td>− (points you invested)</td>
<td>− (points you invested)</td>
</tr>
<tr>
<td>[240 − \text{(points you invested)}]</td>
<td>[120 − \text{(points you invested)}]</td>
</tr>
</tbody>
</table>
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 succeed 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.