1 Introduction

In contests, agents spend resources to increase the probability of obtaining a valuable prize. Contest models have been applied to describe a variety of economic situations, from rent seeking (Tullock, 1980; for a recent review see, e.g., Congleton et al., 2008) to warfare conflicts (e.g., Amegashie and Kutsoati, 2007; Chang et al., 2007), R&D competition (e.g., Dasgupta and Stiglitz, 1980; Taylor, 1995), sports (e.g., Szymanski, 2003; Kräkel, 2007; Berentsen et al., 2008), and intra-firm promotion tournaments (e.g., Lazear, 1999).

Empirical investigation of contest phenomena is challenging.[1] For example, in rent seeking the bidders' valuations of the rent are, in most cases, unmeasurable. Similarly, it is impossible to measure prize valuations by athletes or the value of winning a war to a nation. In R&D competition, the value of the prize (patent) includes the stream of the firm's future profits which is typically unavailable to researchers (Fonseca, 2009). In addition to data availability problems, contests in the field often involve nonrandom selection of participants. In these circumstances, controlled laboratory experiments can serve as a natural alternative research methodology for theory testing.

The existing experimental literature on contests focuses mainly on the case of complete information about players' abilities (e.g., costs of effort or prize valuations). The early experiments in this framework include those on simultaneous-move symmetric and asymmetric contests by Millner and Pratt (1989, 1991), Shorgen and Baik (1991), Schotter and Weigelt (1992), Davis and Reilly (1998), Potters et al. (1998), Anderson and Stafford (2003), Schmidt et al. (2006). More recently, a number of experimental studies looked at more complicated contest structures such as contests with an uncertain prize (Öncüler and Croson, 2005), sequential-move contests (Wiemann et al., 2000; Fonseca, 2009), multi-stage contests with elimination (Parco et al., 2005; Amegashie et al., 2007; Amaldoss and Rapoport, 2009; Sheremeta, 2010), contests with carry-overs (Schmitt et al., 2004), and multi-battle contests (Ryvkin, 2009).

In many applications, however, the assumption of complete information about rivals' abilities is not reasonable. For example, in R&D competition it is unlikely that firms perfectly know their rivals' cost structure. Similarly, different lobbyists may have unobserved differences in their lobbying abilities. While the theoretical literature on contests of complete information is well-established (for the most general results on one-shot simultaneous-move contests see, e.g., Malueg and Yates, 2006; Cornes and Hartley, 2009), there has been an increased interest recently in the theory of contests of incomplete information. Examples include Hurley and Shogren (1998a,b), Malueg and Yates (2004), Sui (2009), Harstad (1995), Schoonbeek and Winkel (2006), Pogrebna (2008), Wärnerid (2003). Fey (2008) and Ryvkin (2010) analyzed theoretically bidding in symmetric contests of incomplete information using a formulation similar to independent private value auctions (Klemperer, 2004). Specifically, Fey (2008) established the existence of a smooth symmetric equilibrium bidding function in the Tullock (1980) contest model of two players with private uniformly distributed marginal costs of effort. Ryvkin (2010) showed the existence also holds for an arbitrary number of players, arbitrary distributions of costs and more general contest success functions.

[1]The classic examples of empirical studies of tournaments and contests include Ehrenberg and Bognanno (1990), Knoeber and Thurman (1994), and Bognanno (2001).

In this paper, we study experimentally bidding behavior in symmetric simultaneous-move contests of incomplete information. To the best of our knowledge, such contests have not yet been explored experimentally, and, unlike in the case of complete information, theoretical predictions for bidding in such contests have not previously been tested. We consider symmetric contests with continuously distributed private marginal costs of effort and use the recent results of Fey (2008) and Ryvkin (2010) as a theoretical benchmark.

Using a 2×2 between-subjects design, we compare bidding in symmetric contests of complete and incomplete information for groups of two and four players. We compare experimental bidding behavior to the theoretical predictions of Fey (2008) and Ryvkin (2010). Additionally, we test the comparative statics results of Ryvkin (2010) who showed that there is a qualitative difference between contests of two and more than two players in relation to how bidding depends on the information condition.

Similar to most of the prior experimental studies on contests, we find significant overbidding in all of our treatments. The comparative statics across the information conditions, however, are in good agreement with the theoretical predictions. Our findings fill in the gap in the literature by providing an experimental test of a theory of bidding in contests of incomplete information. This class of contest models is relevant in many applications of contests where the assumption of complete information is too stylized. Our results show that bidding in contests is in many ways robust to variations in information conditions. At the same time, the subtle qualitative difference between contests of two and more than two players in the comparative statics between the two information conditions is captured by the data.

In a recent study of two-stage contests, Sheremeta (2010) proposes a way to operationalize the "nonmonetary utility of winning," which is often cited as a reason for overbidding in contests and auctions. Sheremeta (2010) measures the nonmonetary utility of winning by letting subjects bid in a contest with zero prize. In the present paper, we use a similar approach but make a step further trying to verify to what extent bidding for zero actually elicits subjects' willingness to win. We find no evidence that in our setting it does.

The rest of the paper is organized as follows. Section 2 summarizes the model and theoretical predictions relevant for our study. Section 3 describes the experimental design, procedures, and hypotheses. Section 4 presents the results, and Section 5 contains a discussion and concluding remarks.

2 Theory and hypotheses

There are n players indexed by $i = 1, \dots, n$. Each player i is characterized by constant marginal cost of effort $c_i > 0$. The players participate in a contest by simultaneously submitting effort levels $e_i \geq 0$. The probability of player i winning conditional on all players' efforts is $p_i = e_i / \sum_j e_j$ (it is assumed that $p_i = 1/n$ for all i if $\sum_j e_j = 0$). The winner receives a prize normalized to one while other players receive nothing.

In what follows, we consider two symmetric settings of this game differing by the type of information each player i has about her rivals' marginal costs c_j, $j \neq i$.

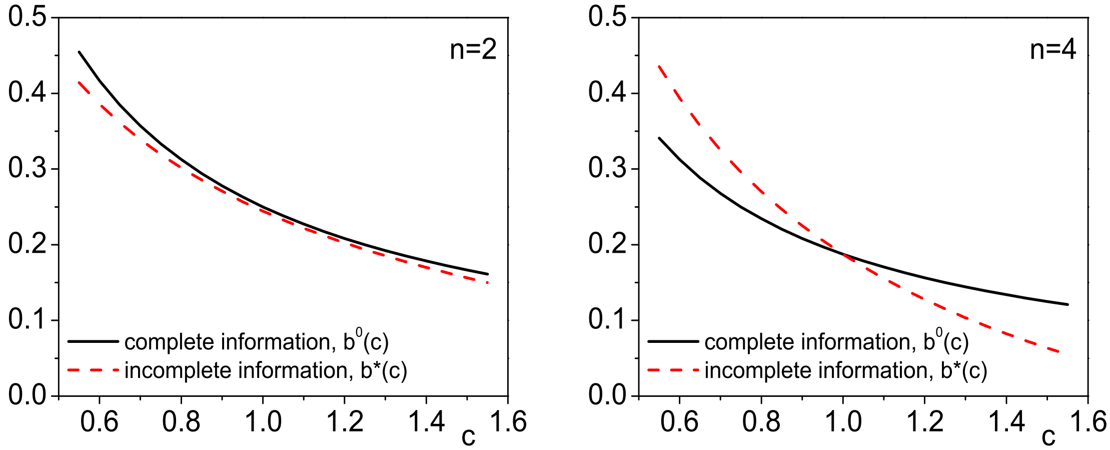

Figure 1: The symmetric equilibrium bidding functions under complete and incomplete information for contests with $n = 2$ (left) and $n = 4$ (right). The curves for the case of incomplete information are obtained numerically by iteratively finding a fixed point of the best response operator (see Ryvkin 2010 for details).

2.1 Complete information

Suppose that all players have the same marginal cost of effort $c_1 = \ldots = c_n = c$, and c is common knowledge. Assuming risk neutrality, the resulting game has a unique symmetric Nash equilibrium, with equilibrium effort $e^0 = (n-1)/cn^2$. We will refer to $b^0(c) = (n-1)/cn^2$ as the equilibrium *bidding function* under complete information.

2.2 Incomplete information

Suppose now that marginal costs are private information. Nature moves first by drawing c_i, $i = 1, \ldots, n$, from a commonly known uniform distribution on the interval $[\underline{c}, \overline{c}]$, with $\overline{c} > \underline{c} > 0$. Then each player i observes her own cost c_i and submits effort e_i. In a symmetric equilibrium, each player chooses effort $e_i^* = b^*(c_i)$, where $b^*(c)$ is the symmetric equilibrium bidding function under incomplete information. Under risk neutrality, the existence of equilibrium was established by Fey (2008) for $n = 2$, and by Ryvkin (2010) for an arbitrary $n \geq 2$.[2]

In the experiment, we choose the number of players $n = 2$ and $n = 4$, and marginal costs distributed uniformly between 0.55 and 1.55.[3] The resulting equilibrium bidding functions, $b^*(c)$, calculated numerically, are shown, together with the complete information bidding functions, $b^0(c)$, in Figure 1.

As seen from Figure 1, the bidding functions under complete and incomplete informa-

[2]Fey (2008) proved the existence of symmetric equilibrium for $n = 2$, uniformly distributed marginal costs and linear contest technology, i.e., the contest success function of the form $p_i = e_i / \sum_j e_j$. Ryvkin (2010) extended the proof of Fey (2008) to allow for $n \geq 2$, arbitrary distributions of marginal costs and contest success functions of the form $p_i = h(e_i)/\sum_j h(e_j)$, where $h(\cdot)$ is a strictly increasing concave function.

[3]Costs c_i could take on one of the ten values 0.6, 0.7, ..., 1.5, with equal probability 0.1.

Treatment	n	Information	Sessions	Subjects
C2	2	complete	2	46
I2	2	incomplete	2	40
C4	4	complete	2	36
I4	4	incomplete	2	44

Table 1: Summary of experimental sessions and treatments.

tion are qualitatively similar. As expected, they are smooth and decrease monotonically with c. There is an important difference between the left and right panels of Figure 1, however. For $n = 2$, the equilibrium bidding function under incomplete information, $b^*(c)$, is everywhere below $b^0(c)$.[4] However, the difference between the two bidding functions is very small. In contrast, for $n = 4$ the two bidding functions are substantially different, especially near the ends of the interval of costs. More importantly, the bidding functions cross: $b^*(c)$ is *above* $b^0(c)$ for $c < 1$ and below $b^0(c)$ for $c > 1$.

2.3 Hypotheses

Following the theory presented above, we formulate the following main hypotheses to be tested experimentally.

Hypothesis 1 *For $n = 2$, bids under complete information are not significantly different from bids under incomplete information for all values of c considered.*

Hypothesis 2 *For $n = 4$, bids under complete information are higher than under incomplete information for $c > 1$, and lower than under incomplete information for $c < 1$.*

3 Experimental design and procedures

3.1 Design

The experiment follows a 2×2 design by varying the number of players in a group ($n = 2, 4$) and information structure (Complete, Incomplete), resulting in four treatments. In what follows, we denote the treatments as C2, C4, I2, and I4. A total of 166 subjects participated in the experiment. Eight sessions were conducted, two sessions per treatment, as summarized in Table 1. Each session consisted of the following four parts.

3.1.1 Part 1

In Part 1, subjects are matched randomly in groups of n and go through 60 decision rounds of the corresponding contest game, with random re-matching after each round. In each decision round subjects are endowed with 120 experimental points (the exchange rate is US$0.01 for one point). They can allocate this endowment between two options:

[4] This is a general result for $n = 2$, see Ryvkin (2010).

keep $(120 - e_i)$ and invest into a project (e_i). One project in the group is then randomly chosen to be successful, with the probability of success of player i's project calculated as $p_i = e_i / \sum_j e_j$ (or $p_i = 1/n$ if all subjects invest zero). If player i's project is successful she receives a prize of $V = 120$ points that round. All players pay the cost of investment equal to $c_i e_i$.[5]

In the complete information treatments, all subjects in the group have the same cost c, which is reported to them at the beginning of each round. It is drawn randomly with equal probability among 10 values, $0.6, 0.7, \ldots, 1.5$, independent across groups and across rounds. In the incomplete information treatments, each subject only sees her own cost c_i. It is drawn randomly with equal probability among the same 10 values, independent across groups, rounds, and subjects.

After making their decisions, subjects are informed whether their project was successful and shown their payoff that round. At the end of 60 rounds, six rounds are chosen randomly to base subjects' actual earnings on.

3.1.2 Part 2

In Part 2, subjects go through five decision rounds in the same setting as in part 1, with one exception: in case subject i's project is successful, she does not receive any prize, i.e., $V = 0$. This game is similar to the "bidding for zero" game of Sheremeta (2010) who used it to capture subjects' "nonmonetary utility of winning" in contests of complete information. One of the five decision rounds is chosen randomly in the end to base subjects' actual earnings on.

At this point, subjects are shown the results of Parts 1 and 2. Specifically, subjects are shown the six rounds from Part 1 and the one round from Part 2 that have been chosen randomly to base their earnings on, reminded about their payoffs in those rounds, and shown their total earnings in Parts 1 and 2.

3.1.3 Part 3

In Part 3, subjects' risk attitudes are measured using the method of Holt and Laury (2002). Subjects independently go through a sequence of 10 choices between two lotteries, $A = (\$1.60, \$2.00; p, 1-p)$ and $B = (\$0.10, \$3.85; p, 1-p)$, with probability p changing from 0.9 to 0 in decrements of 0.1. After all 10 choices have been made, one of them is randomly chosen to be actually played. Lottery A is regarded as the "safe" option, while lottery B is the "risky" option. A subject's risk aversion can be proxied, for example, by the number of safe options she chooses.

[5] The parameters have been chosen so that the equilibrium bids are well below the endowment (equal to the prize in our setting) for all values of c_i, cf. Fig. 1 where prize is normalized to one and all equilibrium bids are well below one. If $c_i > 1$, it is possible for a subject to have a negative round payoff. It happened 160 times out of the total of 9,960 observations in Part 1. Negative payoffs in paying rounds reduced subjects' total earnings, but in the end none of the subjects had negative total earnings in Part 1.

3.1.4 Part 4

In Part 4, subjects perform a simple number addition task (see, e.g., Niederle and Vesterlund, 2007; Brueggen and Strobel, 2007). Within three minutes, subjects are asked to solve randomly generated number addition problems. Each problem is finding the sum of five two-digit numbers without using a calculator. Upon completion of the task, subjects are asked how they prefer to be paid. One option is to be paid \$0.10 per each correctly solved problem. The other option is to be randomly matched with three other subjects and be paid \$0.40 cents per each correctly solved problem if one's score is the highest among the four subjects, and zero otherwise, with ties broken randomly. Subjects are also asked to answer two self-assessment questions: (i) How many problems do you think you solved correctly? (ii) Among the [number of subjects in the session] participants of the experiment, what do you think your rank is (with rank 1 corresponding to the highest score, and rank [number of subjects in the session] to the lowest score)?

The goal of Part 4 is to provide a set of independent controls for the nonmonetary utility of winning. Subjects make decisions in an independent setting, and we measure their willingness to enter tournaments along with the major factors affecting tournament entry identified by Niederle and Vesterlund (2007).

3.2 Procedures

The experimental sessions were conducted at the XS/FS laboratory at Florida State University. Subjects were recruited using ORSEE (Greiner, 2004) and made decisions at separated computer terminals. The experiment was programmed in z-Tree (Fischbacher, 2007). Each subject participated in only one session. Sessions lasted approximately 90 minutes, with average earnings of around \$22 per subject including a \$10 show-up fee. Instructions were read aloud, with a printed copy distributed to subjects (see Appendix). At the beginning of each session, subjects were informed that there are several parts in the experiment but they did not know about the number of parts or about the nature of each part until it was announced.

4 Experimental results

4.1 Data and variables

Under the random re-matching protocol, each subject went through 60 decision rounds in Part 1, and five decision rounds in Part 2. Let B_{it} denote the bid of subject i in round t. For convenience, and for direct comparison to theory, we rescale all bids in the experiment by the value of the winner's prize in Part 1, $V = 120$ (which is also the size of the endowment), and use $b_{it} = B_{it}/120$ to denote the rescaled bids.

In Part 3 of the experiment, subjects went through 10 rounds of choices between two lotteries in the Holt and Laury (2002) risk aversion measurement instrument. We use RA_i, the number of times subject i chose lottery A (the "safe" option), as a measure of subject i's risk aversion.

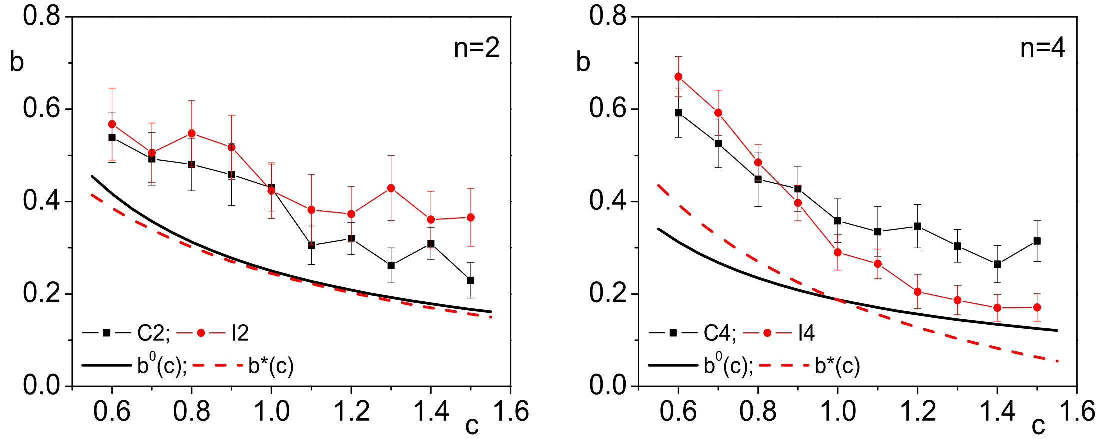

Figure 2: The observed average bidding functions for each treatment, with error bars (errors are cluster-robust at the subject level). The theoretical bidding functions are shown with the solid and dashed lines.

In Part 4 of the experiment, subjects were solving number addition problems for three minutes. $SumcorReal_i$ is the number of problems subject i solved correctly. Subjects were then asked to predict how many problems they solved correctly ($SumcorEst_i$) and what their ranking was among all the participants in the session ($RankEst_i$). For comparability across sessions with different numbers of subjects, we rescaled each subject's predicted rank into fractional percentile, $PtileEst_i = (m_i - RankEst_i)/(m_i - 1)$, where m_i is the number of subjects in subject i's session.

Subjects were also asked whether they prefer to be paid at a piece rate or submit their performance to a tournament with payoffs calibrated so that the expected tournament payoff of a subject whose performance is at the average level is the same as her piece-rate payoff. Variable $TournEntry_i$ is a dummy equal one if subject i chose to participate in the tournament.

We additionally recorded each subject's gender as a dummy variable $Female_i$ equal one if subject i is a female.

4.2 Aggregate results

4.2.1 Bidding

Part 1 of the experiment allows us to observe, on average, 276, 240, 216 and 264 bids per value of cost parameter c_i in treatments C2, I2, C4 and I4, respectively. We, therefore, can construct the average empirical bidding functions. These are shown for each treatment, together with the theoretically predicted bidding functions, in Figure 2. The error bars in Figure 2 represent subject-level clustered sample standard errors.

As seen from Figure 2, there is a significant overdissipation of effort as compared to the theoretical predictions.

Result 1 *On average, there is a significant overbidding in all treatments.*

Comparing the treatments with complete and incomplete information, it is seen from Figure 2 that the theoretically predicted comparative statics for $n = 2$ cannot be rejected for nearly all values of c_i: average bids are not statistically different between the two information conditions for all values of c_i except for $c_i = 1.3$ and $c_i = 1.5$. For $n = 4$, we observe that, as predicted, subjects bid lower in I4 than in C4 when costs are high. When costs are low, subjects bid slightly higher in I4 than in C4, but the difference is not statistically significant.

Result 2 *For $n = 2$, average bids are the same under complete and under incomplete information for all but two values of costs considered. For $n = 4$, average bids are higher under complete information for high costs, while for low costs there is no statistically significant difference in average bids between the information conditions.*

Thus, Hypothesis 1 is mainly supported by the aggregate data, whereas Hypothesis 2 is partially supported. The comparative statics of bids have consequences for the comparison of aggregate effort between the information conditions.

Result 3 *For $n = 2$, total expended effort is not statistically different under the two information conditions. For $n = 4$, total expended effort is higher under complete information.*

We conclude that although the point predictions are substantially below the observed bids, the comparative statics across the information conditions are in partial agreement with the theory. Most importantly, the qualitative difference between the cases of $n = 2$ and $n = 4$ predicted by theory is observable experimentally.

4.2.2 Bidding for zero prize

In Part 2 of the experiment, subjects go through five rounds of the same contest game as in Part 1 but the value of the winner's prize is zero. To minimize subjects' confusion, the instructions contain the following statement: "Note that the only way to guarantee yourself a payoff of 120 in this part of the experiment is to invest zero into the project. If you invest any positive amount, your payoff is guaranteed to be below 120." Still, out of the total of 830 bids in Part 2, 128 (15.4%) are not zero. It is worth noting that the minimal positive bid of one point is the second most popular bid after zero; the total number of zero and one point bids is 91.5%.[6]

Table 2 shows a more detailed distribution of bids in Part 2 for each treatment. Clearly, zero is the dominant bid, and the bid of one point is the second most popular with the exception of treatment I4. Overall, there appears to be fewer nonzero bids than reported by Sheremeta (2010), most likely due to the disambiguating statement about their effect on payoffs.

In Part 2, subjects bid for zero prize in five rounds. Table 3 shows the percentage of zero and zero or one point bids by round. Interestingly, there appears to be no significant effect of experience on bidding for zero, at least at the aggregate level (averaged across subjects).

[6]It can be argued that some subjects do not bid zero even if they want to becasue they are "afraid of the boundaries." On the other hand, bidding one point may indicate the desire to "win on the cheap."

	Treatments			
Bids	C2	I2	C4	I4
0	80.4	85.5	90.0	83.6
1	9.1	7.5	5.6	5.0
2-30	0.4	2.5	1.7	8.6
31-60	2.6	2.5	1.7	0.9
61-90	4.3	1.0	1.1	0.9
91-120	3.0	1.0	0	0.9

Table 2: The distribution of bids in Part 2 (bidding for zero), in percentage points, by treatment. Percentages may not exactly sum up to 100 due to rounding.

	Treatments							
Rounds	C2		I2		C4		I4	
	0	0 or 1	0	0 or 1	0	0 or 1	0	0 or 1
1	80.4	87.0	87.5	90.0	86.1	94.4	84.1	86.4
2	78.3	89.1	90.0	95.0	94.4	94.4	81.8	88.6
3	78.3	89.1	87.5	95.0	86.1	100	81.8	88.6
4	82.6	91.3	82.5	95.0	91.7	94.4	86.4	90.9
5	82.6	91.3	80.0	90.0	91.7	94.4	84.1	90.9

Table 3: The percentage of zero and zero or one point bids by round in Part 2 (bidding for zero), by treatment.

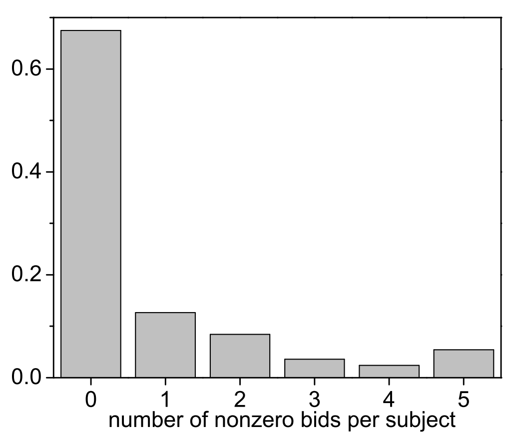

Figure 3: The distribution of the number of nonzero bids per subject in Part 2 (bidding for zero), all treatments combined. The total number of subjects is 166.

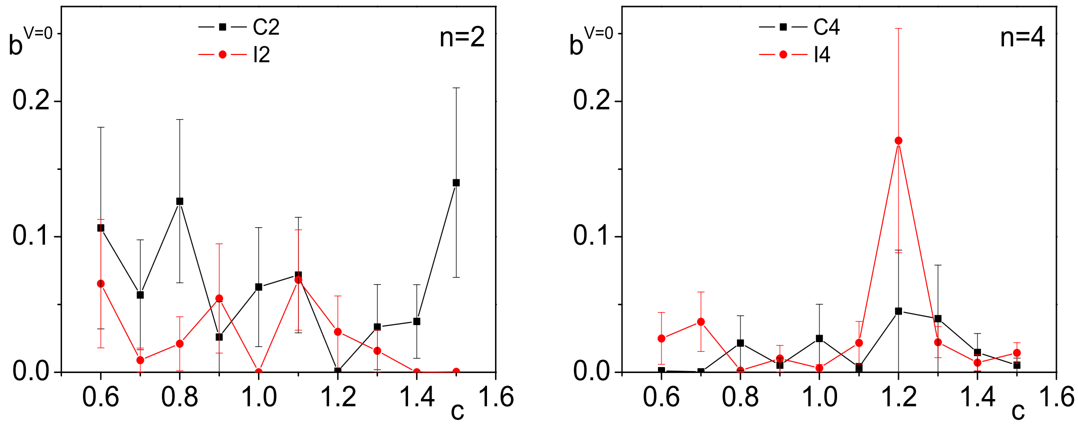

Figure 4: The observed average bidding functions in Part 2 (bidding for zero), by treatment. The error bars show sample standard errors clustered at the subject level.

Figure 3 shows the distribution of the number of nonzero bids per subject for 166 subjects in all four treatments combined. The distribution has a skewed U-shape, with a relatively strong peak at five nonzero bids. Thus, some subjects are persistent in their bidding above zero.

Finally, it is of interest to explore how bidding for zero depends on the cost parameter c_i. Similar to Figure 2, we formed average bidding functions for each treatment in Part 2 of the experiment. These are shown in Figure 4. Assuming, as in Sheremeta (2010), that bidding for zero prize is a measure of how much subjects value winning, the bidding functions in Figure 4 represent the demand for winning. Interestingly, as seen from Figure 4, the demand for winning is flat (this is also confirmed through regression analysis at the individual level, see below). Thus, either winning is not a normal good, or, which we believe is more likely, bidding for zero prize does not really measure demand for winning.

4.2.3 Risk aversion

Figure 5 shows the distribution of the RA variable (the number of times a subject chose the safer lottery A in the Holt and Laury 2002 instrument). The distribution is based on data from 166 subjects in the experiment with all four treatments combined. There is no statistically significant difference in average RA between treatments. As seen from Figure 5, most subjects fall somewhere in the middle of the range, but the instrument provides a substantial individual variation. The mean value of RA in the sample is 5.6, and standard deviation is 2.5.

4.2.4 Number addition task

Figure 6 shows the distribution of the $SumcorReal$ variable (the number of correctly solved addition problems in Part 4 of the experiment). The distribution is based on data from 166 subjects with all four treatments combined. There is no statistically significant difference in average $SumcorReal$ across treatments. Most subjects solved between four

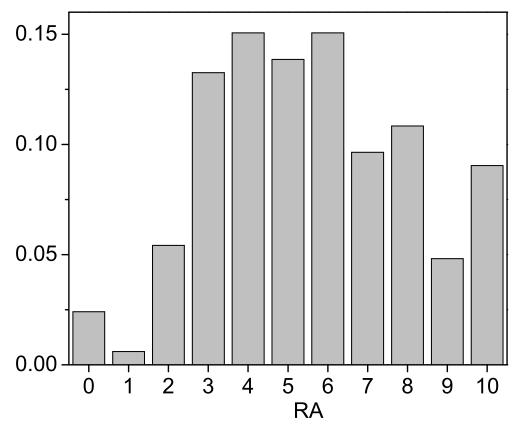

Figure 5: The distribution of the *RA* variable (the number of times a subject chose the safer lottery *A* in the Holt and Laury 2002 instrument), all treatments combined. The total number of subjects is 166.

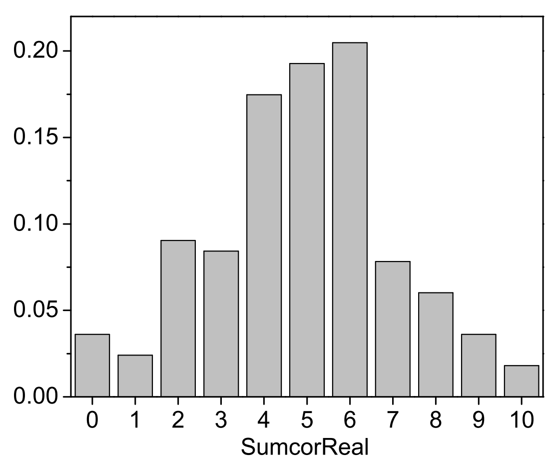

Figure 6: The distribution of the *SumcorReal* variable (the number of correctly solved summation problems in Part 4), all treatments combined. The total number of subjects is 166.

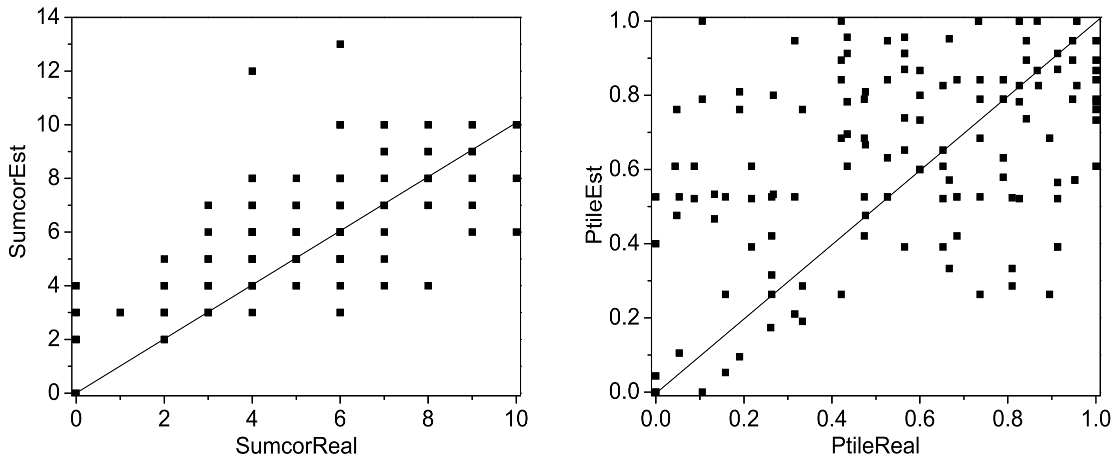

Figure 7: *Left*: The estimated number of correctly solved summation problems versus the actual number of correctly solved summation problems, 146 subjects, all four treatments combined (the number of points is fewer than 146 due to many coinciding data points). *Right*: Estimated session percentile versus actual session percentile, 146 subjects, all four treatments combined. The solid lines show the 45 degree lines.

and six problems, some solved zero, and none solved more than 10 problems during the allocated three minutes. There is a substantial variation in *SumcorReal* across subjects, with mean 4.9 and standard deviation 2.2.

4.2.5 Self-assessment

Upon completion of the number addition task, subjects were asked self-assessment questions about their absolute and relative performance.[7] Both serve as measures of subjects' overconfidence.[8]

Figure 7 shows the scatter plots of answers to the absolute and relative performance assessment questions against the real performance. The left graph in Figure 7 shows the estimated number of correctly solved summation problem plotted versus the actual number of correctly solved problems, while the right graph shows estimated performance percentile versus actual percentile. Both graphs exhibit the usual miscalibration patterns, with most subjects exhibiting overconfidence and top performers exhibiting some underconfidence. Calibration in absolute performance judgements appears to be relatively good. For relative performance, however, overconfidence at the low end of the performance distribution is substantial.

[7] The self-assessment questions were not asked in one of the two sessions of treatment I4, hence the total number of subjects who responded to the self-assessment questions is 146.

[8] Truthful answers were not incentivized in any way, therefore, the interpretation of the results from the self-assessment questions may be limited. We note, however, that overconfidence is not the focus of the present study, and we only use the measures of overconfidence as controls to explain variation in behavior across subjects. Thus, even though, arguably, the overall level of overconfidence can be reduced by monetary incentives, this effect is unimportant for our purposes.

4.2.6 Tournament entry

In addition to the self-assessment questions, subjects were asked whether they prefer to be paid for their performance on the number addition task at a piece rate of \$0.10 per correctly solved problem or to submit their performance to a tournament. For the tournament, each subject was randomly matched with three other subjects, and if her performance was the highest among the four people she received \$0.40 per correctly solved problem. Ties were broken randomly. The average tournament entry rate for 166 subjects in all treatments combined was 0.66, with standard deviation 0.48. There are no statistically significant differences in average entry rates between treatments.

Given the payment scheme, it is optimal for a risk-neutral subject to enter the tournament if she believes that her performance is above average. The hypothesis of average entry rate equal to 0.5 is rejected at less than 1% significant level. We conclude that subjects' entry decisions are, on average, consistent with overconfidence.[9]

4.3 The determinants of bidding

In this section, we focus on the bidding behavior in Part 1 of the experiment. The aggregate results on bidding presented in Section 4.2.1 only tell part of the story. To gain a better understanding of behavior and its deviations from the theoretical predictions, we use regression analysis at the individual level.

A nontrivial part of bids occur at the two boundaries. Specifically, in treatment C2 15.3% of rescaled bids are 0 and 6.9% of rescaled bids are 1; in treatments I2, C4, and I4 these are 9.5% and 8.4%, 25.1% and 12.7%, and 23.6% and 10.6%, respectively. We, therefore, use two-sided tobit models to estimate bidding equations.

Table 4 reports the results of two-sided tobit regressions with errors clustered at the subject level.[10] We report the results of three different specification, (1), (2) and (3). Within each specification, we ran the regression separately for $n = 2$ and $n = 4$ merging data from the two information conditions.

In specifications (1) and (2), we only used the first 55 rounds of bidding data. The last five rounds of data (rounds 56 through 60) have been used to generate control variable $Bida56\text{-}60_i$, the average bid of subject i in these rounds. Other variables in Table 4 not described previously are $Period$ controlling for the presence of overall trend in bids; $1/c$ – the inverse of the cost parameter; $Private$ – a dummy variable equal one in the treatments with incomplete information (I2 and I4); $Bid0_i$ – the average bid of subject i in Part 2 of the experiment (bidding for zero prize); $PtileReal_i$ – the actual percentile of subject i's performance on the number addition task in Part 4. We also include interaction variables $c * Private$ and $Period * Private$ to control for possible differences in the slope of the bidding function and time trending between the information conditions.

The difference between specifications (1) and (2) is in the inclusion of variable $Bida$56-60 in the latter. Since the coefficient estimates on all variables with the exception of $Bid0$ and $Bida$56-60 are not significantly different between specifications (1) and (2), we will

[9] As noted above, overconfidence is not the focus of this study. We will use variation in tournament entry decisions as a control for other purposes.

[10] Random effects tobit produces very similar results.

b_{it}	(1) $n=2$	(1) $n=4$	(2) $n=2$	(2) $n=4$	(3) $n=2$	(3) $n=4$
$Period$	-0.004***	-0.006***	-0.004***	-0.006***	-0.004***	-0.006***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
$1/c$	3.686***	4.83**	3.832***	4.804***	3.752***	4.684***
	(0.738)	(0.889)	(0.712)	(0.877)	(0.730)	(0.930)
$Private$	-0.016	0.235	-0.045	0.273*	-0.044	0.364*
	(0.141)	(0.161)	(0.135)	(0.155)	(0.139)	(0.202)
$c * Private$	0.002	-0.034**	0.004	-0.031**	0.002	-0.037**
	(0.012)	(0.013)	(0.011)	(0.013)	(0.012)	(0.017)
$Period * Private$	0.001	0.001	0.001	0.002	0.001	-0.001
	(0.001)	(0.002)	(0.001)	(0.002)	(0.001)	(0.002)
$Bid0$	0.551***	0.949***	0.087	0.238		
	(0.128)	(0.353)	(0.114)	(0.267)		
$Bida56$-60			0.781***	0.935***		
			(0.065)	(0.121)		
$Female$	0.051	0.087	0.014	-0.061	0.093**	0.172**
	(0.046)	(0.067)	(0.031)	(0.054)	(0.044)	(0.088)
RA	0.013	0.005	0.008	0.004	-0.012	-0.007
	(0.009)	(0.011)	(0.006)	(0.009)	(0.009)	(0.014)
$TournEntry$					0.018	-0.133
					(0.047)	(0.094)
$SumcorReal$					0.006	0.089
					(0.027)	(0.079)
$SumcorEst$					-0.003	-0.025
					(0.013)	(0.03)
$PtileReal$					-0.033	-0.466
					(0.206)	(0.625)
$PtileEst$					0.251**	0.04
					(0.108)	(0.215)
Constant	0.143	-0.035	-0.212**	-0.233**	0.052	0.078
	(0.1)	(0.115)	(0.096)	(0.119)	(0.124)	(0.165)
Observations	4730	4400	4730	4400	5160	3600
Clusters (subjects)	86	80	86	80	86	60
Time periods	55	55	55	55	60	60

Table 4: The results of two-sided tobit regressions of bids in Part 1. Standard errors are clustered by subject. Significance levels: *** - 1%, ** - 5%, * - 10%.

first describe the results regarding all other variables and then go back to discussing those two.

The negative and statistically significant time trend is present in all specifications. Thus, over time subjects learn to play closer to equilibrium although do not quite get there even by round 60. As expected, the inverse cost parameter, $1/c$, has a positive and highly significant effect, i.e., subjects exhibit a well-behaved downward-sloping demand for winning the prize. The $Private$ condition is not significant, but the interaction $c * Private$ is negative and significant for $n = 4$. This result confirms the aggregate finding that bids are lower under incomplete information for higher costs.

Result 4 *The regression analysis of individual data confirms the aggregate Result 2.*

Variable $Bid0$ has a positive and highly significant effect in specification (1). This result is consistent with Sheremeta (2010) who reports the positive and significant effect of bidding for zero prize on bidding in general. Sheremeta (2010) concludes that bidding for zero explains overdissipation in contests as it measures the "nonmonetary utility of winning."

Specification (2) contains, in addition to $Bid0$, variable $Bida56\text{-}60_i$. Note that in rounds 56-60 the winner's prize is not zero and cost parameters change randomly from one round to the next, therefore, $Bida56\text{-}60_i$ is a "fixed effect" of sorts and measures subject i's bidding propensity in general. As seen from the results of specification (2), it explains bidding in rounds 1-55 as well as $Bid0$. Moreover, when both $Bid0$ and $Bida56$-60 are included, the effect of $Bid0$ becomes insignificant. We conclude that $Bid0$ does not capture any new effects beyond general bidding propensity.[11]

Part 4 of the experiment was designed to measure subjects' willingness to participate in a tournament in a different context. If a subject experiences a higher nonmonetary utility from winning she should also be more likely to enter tournaments in general, other things being equal, because it is the only way to win. The determinants of tournament entry have been explored previously, see, e.g., Niederle and Vesterlund (2007). Conditioning on subjects' performance, the factors identified as the determinants of tournament entry are gender, risk aversion and overconfidence. Specification (3) includes a subject's decision to enter the tournament ($TournEntry$) and the aforementioned variables from Part 4 of the experiment as additional determinants of bidding in Part 1.

As seen from the results for specification (3), tournament entry decision in Part 4 is not statistically significant for bidding in Part 1. It appears that, controlling for other factors, women bid more than men. This result is preserved if variable $TournEntry$ is excluded from specification (3), and the coefficient estimates change only mildly. In line with the results of Niederle and Vesterlund (2007), the probit regression of $TournEntry$ on $Female$ and all other factors gives a negative and significant coefficient estimate on $Female$. The important result is that controlling for all other factors that determine

[11] It can still be argued at this point that $Bid0$ contains the nonmonetary utility of winning. Indeed, if this utility is part of regular bids, then it is also part of bids in periods 56-60, and it is not surprising that $Bida56$-60 is strongly correlated with $Bid0$. It is surprising, however, that controlling for $Bida56$-60, there is no additional effect of $Bid0$ coming exclusively from bidding for zero. We return to this issue later.

tournament entry the latter does not have a component responsible for overbidding in contests.

Result 5 *Willingness to enter tournaments (and hence, willingness to win) does not explain overbidding in contests.*

It still might be that bidding for zero is determined by the same variables as tournament entry, in which case it can be argued that it can serve as a gross measure of willingness to win. To check this, we ran a two-sided tobit regression of bids in Part 2 of the experiment on the same variables as in specification (3) in Table 4, including and not including variable $TournEntry$. None of the variables comes out as significant, with the exception of $PtileReal$ at 10% for $n = 2$ in the specification with $TournEntry$ included.

Two results (or, rather, "nonresults") are worth emphasizing. First, the coefficient estimate on variable $1/c$ is not significant. Thus, subjects do not appear to exhibit a normal downward-sloping demand for winning the contest with zero prize. Second, none of the variables identified as the determinants of tournament entry has an effect on bidding for zero. The following two results summarize these findings.

Result 6 *The hypothesis that the demand for winning the zero prize is flat cannot be rejected.*

Result 7 *Tournament entry decision and the variables affecting tournament entry have no effect on bidding for zero.*

Results 6 and 7, in conjunction with the insignificant coefficient estimate on $Bid0$ in specification (2) in Table 4, indicate strongly that in our experiment bidding for zero is not a reliable measure of nonmonetary utility of winning.

5 Discussion and conclusions

In this paper, we present the results of an experimental study of bidding in symmetric contests of incomplete information. The incomplete information condition is relevant for many applications of contest models, such as rent seeking or R&D competition. At the same time, most of the theoretical models and all experimental studies of contests focused on the case of complete information. The results of this study, therefore, inform on how behavior under the more realistic conditions may be different from what was observed previously.

Theoretically, the investigation of imperfectly discriminating contests of incomplete information is complicated by the presence of two levels of uncertainty in such settings. First is the uncertainty associated with imperfect discrimination: unlike in auctions, winning is not guaranteed for the highest bidder. On top of that, the second layer of uncertainty is associated with private information about players' abilities. The recent theoretical results of Fey (2008) and Ryvkin (2010) on the existence and properties of equilibrium bidding functions in imperfectly discriminating contests of incomplete information show, however, that the introduction of unobserved heterogeneity does not lead to major qualitative changes in symmetric equilibrium bidding functions.

At the same time, somewhat more subtle differences are predicted between the two information conditions. It was known previously that in asymmetric contests of complete information there are qualitative differences in responses of players to changes in other players' abilities between contests of two and more than two players (Stein, 2002). These differences, when averaged over the realizations of unobserved abilities, reveal themselves also in symmetric contests of incomplete information (Ryvkin, 2010). Specifically, equilibrium bids are always lower under incomplete information than under complete information for $n = 2$, while this is no longer the case in general for $n > 2$.

We conduct a direct comparison of bidding under complete and incomplete information for contests with different group sizes in a 2×2 between-subjects design involving two group sizes ($n = 2, 4$) and two information conditions (complete, incomplete). We find that, as predicted, average bidding functions under incomplete information are not qualitatively different from those under complete information. This is not unexpected, as both types of bidding reflect the normal downward-sloping demand for winning the prize. For contests of two players, we find that for almost all values of the marginal cost parameter the bidding functions under two information conditions are not statistically different. Thus, remarkably, bidding in two-player contests is robust to the introduction of unobserved heterogeneity. This result is important in view of the many applications where information is indeed incomplete. We conclude that the previous experimental findings on two-player contests are likely to hold if the information conditions change.

For contests of four players, we find that although for relatively low costs there is no statistical difference in bidding between the information conditions, the difference arises for higher costs. Specifically, players bid more conservatively under incomplete information, as predicted by theory. The intuition is straightforward: high-cost players under incomplete information are more likely to face opponents whose costs are lower, hence they should reduce their bids. The same intuition, with the opposite outcome, holds for low-cost players, but the experimental results do not support it.

Although the comparative statics of equilibrium bidding are in a reasonable agreement with the theory, the levels of bids are substantially higher than predicted. Overbidding in contests is certainly not a new result, but here we find that it is also robust to changes in information conditions. We also find that overbidding in our setting decreases with experience, albeit very slowly. This is in contrast with some other studies that found an increasing or flat time dependence of average bids (e.g., Millner and Pratt 1989, 1991; Sheremeta, 2010). We believe that the differences in learning patterns stem from the differences in the information available to subjects. For example, Sheremeta (2010) who observed bids increase over time informed subjects about their rivals' bids after every round. This way, bids could be rising due to learning by imitation. In our experiment, subjects were only informed about their own bid and payoff after each round. It turns out this restriction on learning improves efficiency over time.

A number of authors suggest that overbidding in contests and auctions is due to the "nonmonetary utility of winning" (Schmitt et al., 2004; Parco et al., 2005; Goeree et al., 2002; Amaldoss and Rapoport, 2009; Sheremeta, 2010). Sheremeta (2010) proposes a method to experimentally measure this utility by letting subjects bid in a one-shot contest with zero prize. In the present paper, we use a similar approach and let subjects bid for zero prize in five rounds of the same experimental treatment they went through bidding for a positive prize. We find that a small but significant part of our subjects bid nonzero amounts in these contests despite the explicit disambiguating statement in the instructions that they are guaranteed to earn less money by doing so. We find that bidding for zero is a strong explanator of bidding in the regular contest and, as such, automatically accounts for a significant part of overbidding.[12] We go a step further, however, and try to assess whether bidding in a contest with zero prize indeed measures the nonmonetary utility of winning.

First, in our design subjects bid for zero prize five times, with randomly changing marginal costs of bidding. If winning is a normal good (which it has to be assuming there is a nonzero monetary utility of winning), we should see lower bids as the cost increases. Instead, we observe no dependence of bids for zero prize on the marginal cost. Second, we try to relate subjects' bidding for zero to their behavior in a separate experimental treatment where they make decisions allowing us to explicitly measure their willingness to win. After a real-effort number addition task, subjects are asked to provide assessments of their own absolute and relative ability and also to choose whether to participate in a tournament where winning is determined by their relative performance on the task. Controlling for other factors known to determine tournament entry (gender, risk aversion, skill level, overconfidence), subjects who are more willing to enter tournaments should enjoy winning more. We find no evidence that bidding for zero is correlated with tournament entry.

So, what does bidding for zero actually measure? Unfortunately, we are unable to give a definite answer to this question. It is clearly correlated with the general bidding propensity: subjects who bid more for zero tend to bid more in regular contests. However, this correlation does not really explain anything because both types of bidding are likely affected by the same unobserved factors. Our attempt to capture these factors through additional subject characteristics – gender, risk aversion, overconfidence, ability to sum numbers, and willingness to enter tournaments – shows that women tend to bid higher than men, and overconfidence in one's relative standing is associated with higher bids for $n = 2$ (but not $n = 4$). One possible ex post explanation is that bidding for zero, at least in our setting, captures subjects who simply choose to ignore instructions. The origins of overbidding in contests, and how to reliably measure the nonmonetary utility of winning, are still open questions for future research.

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[12]Sheremeta (2010) uses experimentally observed bids in contests with zero prize to recover the corresponding effective "prize" subjects receive for winning. Thus recovered prize is then added to the prize in the regular contest, and the new predicted bids turn out to be close to the observed bids. In our case, the observed bids are still significantly higher.

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A Experimental instructions

Introduction

Thank you for participating in today's experiment. I will read through the script so that all sessions of this experiment receive the same information. You will receive $10 show-up fee plus whatever earnings you will make during the experiment. Your earnings may depend on your own decisions and the decisions of other participants.

At the end of the experiment your earnings will be added to your $10 participation fee. You will be given a check for the total amount. The payment will be anonymous. No other participant will be informed about your payment.

Please remain quiet and do not communicate with other participants during the entire experiment. Raise your hand if you have any questions. One of us will come to you to answer them.

The experiment consists of several parts. The instructions for each part will be given separately at the beginning of that part.

Part I

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

Matching. In this part of the experiment you will be randomly divided into groups of 4 participants. After each round, groups will be randomly re-shuffled, i.e., you will be randomly re-matched with three different participants. In any given period, any person in the room has equal chances to be in the same group with you.[13]

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 (0 and 120 inclusive) into a project. Your project can be either successful or unsuccessful. If your project is successful, you will receive 120 points of revenue that round. If your project is unsuccessful, you will not receive any revenue that round.

Likelihood of success. Each round, only one project in your group will be successful. The probability (likelihood) that your project is successful is determined as follows. First, the total investment of all members of your group is computed. Then, your probability of success is computed as the share of your investment in the total investment.

For example, suppose group members choose to invest 25, 50, 75, and 100. Then total investment is 25 + 50 + 75 + 100 = 250, and the probabilities of success are as follows:

Group member	investment	probability of success
1	25	25/250 = 10%
2	50	50/250 = 20%
3	75	75/250 = 30%
4	100	100/250 = 40%

[13] This paragraph is appropriately modified depending on group size.

Are there any questions?

Cost of investment. Your cost of investment is equal to your Unit Cost times your investment. For example, if your Unit Cost is 1, and your investment is 20, your cost this round is $1 * 20 = 20$. If your Unit Cost is 0.6 and your investment is 20, your cost this round is $0.6 * 20 = 12$.

Are there any questions?

Unit Cost. Recall that cost of investment = (unit cost)*investment. Your unit cost of investment will be **your private information**. Other participants in your group will not know your unit cost. Unit cost may change from one round to the next, and it may be different for different members of your group. In each round, it will be selected randomly to be one of the following numbers: 0.6, 0.7, 0.8, 0.9, 1, 1.1, 1.2, 1.3, 1.4, 1.5, with each of these 10 values equally likely. You will see your unit cost, but not the unit costs of other members of your group, before you make your investment decision.[14]

Payoff in a given round. Once the probability of project success of each member of your group is calculated, the computer randomly chooses one group member whose project is successful, in accordance with the probabilities calculated. That is, for example, if one member's probability of success is 10of success is 40member's project.

Your payoff in a given round is determined as follows:

If your project is successful:	If your project is unsuccessful:
+120 (endowment)	+120 (endowment)
+120 (revenue)	+0 (no revenue)
- (unit cost)*investment	- (unit cost)*investment
240 - (unit cost)*investment	120 - (unit cost)*investment

Are there any questions?

How your earnings from this part of the experiment are determined. You will go through 60 decision rounds. After that, six of these rounds will be chosen randomly (with all rounds being equally likely to be chosen) to base your earnings on. At the end of the experiment, you will be informed about the rounds chosen, your earnings in those rounds, and the total earnings.

Are there any questions?

To repeat, in each decision round in this part of the experiment you will be randomly matched with 3 other participants. You will be informed about your unit cost of investment and asked to choose an investment level between 0 and 120. After that the successful project and payoffs in your group will be determined. You will be randomly re-matched with 3 other participants, and so on.

Are there any questions?

You will now start the actual decision rounds. Please do not communicate with other participants or look at their monitors. If you have a question or problem, from this point on please raise your hand and one of us will assist you in private. Please remember to click CONTINUE to proceed.

[14]This paragraph is appropriately modified depending on the information condition.

Part II

In this part of the experiment, you will go through 5 decision rounds in the same setting as before. The only difference is that in case your project is successful you will receive *zero revenue*. The only reward to you is that your project is the successful project in your group.

Thus, payoff in each round will be determined as follows:

If your project is successful:	If your project is unsuccessful:
+120 (endowment)	+120 (endowment)
+0 (revenue)	+0 (no revenue)
- (unit cost)*investment	- (unit cost)*investment
120 - (unit cost)*investment	120 - (unit cost)*investment

Note that the only way to guarantee yourself a payoff of 120 in this part of the experiment is to invest zero into the project. If you invest any positive amount, your payoff is guaranteed to be below 120.

One of the 5 decision rounds will be chosen randomly to base your actual earnings on.

Are there any questions?

Part III

In each round of this series you will be asked to make a choice between two lotteries that will be labeled A and B. There will be a total of 10 rounds and after you have made your choice for all 10 rounds, one of those rounds will be randomly chosen to be played. Lottery A will always give you the chance of winning a prize of $2.00or1.60$, while lottery B will give you the chance of winning \$3.85 or \$0.10. Each decision round will involve changing the probabilities of your winning the prizes. For example in round 1, your decision will be represented on the screen in front of you:

Your decision is between these two lotteries:

Lottery A: A random number will be drawn between 1 and 100. You will win
\$1.60 if the number is between 1-90 (90% chance)
\$2.00 if the number is between 91 and 100 (10% chance)

Lottery B: A random number will be drawn between 1 and 100. You will win
\$0.10 if the number is between 1 and 90 (90% chance)
\$3.85 if the number is between 91 and 100 (10% chance)

If you were to choose lottery B and this turns out to be the round actually played, then the computer will generate a random integer between 1 and 100 with all numbers being equally likely. If the number drawn is between 1 and 90, then you would win \$0.10 while if the number is between 91 and 100, then you would win \$3.85. Had you chosen lottery A then if the number drawn were between 1 and 90 you would win \$1.60 while a number between 91 and 100 would earn you \$2.00.

All of the other 9 choices will be represented in a similar manner. Each will give you the probability of winning each prize as well as translate that probability into the numerical range the random number has to be in for you to win that prize.

At the end of the 10 choice rounds, you will be asked to press a button that will allow the computer to determine your payment. When you do so, the computer will randomly pick one of the 10 rounds to base your payment on, remind you of the choice you made in that round and draw the random number between 1 and 100 to determine your earnings.

Are there any questions before you begin making your decisions?

We ask that you follow the rules of the experiment and in particular we again ask that you do not talk with other participants or look at their screens during the experiment. Anyone who violates the rules may be asked to leave the experiment with only the $10.00 show-up fee.

You will now start the sequence of 10 choices. You will be able to go through the choices at your own pace, but we will not be able to continue the experiment until everyone has completed this series.

Part IV

In this part of the experiment you can solve simple number addition problems and earn some money. These problems are generated randomly by the computer. Each problem looks something like this:

12 + 34 + 56 + 82 + 30 = ?

You should find the sum without using a calculator, enter your answer into the input box and click SUBMIT. Then you will be given the next problem.

You will have 3 minutes to solve as many such problems as possible. You will be able to receive 10 cents for each <u>correctly</u> solved problem.

Are there any questions?

Questions after Part IV (before the results of Part IV are shown)
1. How many summation problems do you think you solved correctly?
2. Out of the [...] participants of the experiment, what do you think your ranking is (with 1 corresponding to the highest score and [...] corresponding to the lowest score)?
3. Please select how you would like to be paid [radio buttons with options (a) and (b)]. Option (a) will pay you $0.10 per each correctly solved problem. If you choose option (b), you will be randomly matched with three other participants. If the number of problems you solved correctly is greater than that of the other three people, you will receive $0.40 per each correctly solved problem. Otherwise you receive nothing. Ties will be broken randomly.