TOURNAMENTS, RISK PERCEPTIONS, AND FAIRNESS [1]

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Introduction

It is well known among contract theorists that relative performance incentive schemes, which reward agents based on how well they perform relative to other agents, is justified in environments where common shocks are large. By using relative performance contracts, principals can exploit the information contained in common shocks which would provide them with "…a richer information base on which to write contracts…" (Holmstrom and Tirole, p. 96). Agricultural economists have also used this insight when discussing relative performance contracts in agriculture where common shocks can be large (Hueth and Ligon; Tsoulouhas and Vukina; Wu and Roe, among others). A special class of relative performance contracts called tournaments have been used in some agricultural sub sectors, particularly in poultry.

Despite the fact that tournaments are, in principle, a legitimate economic device for incentive provision and risk management in environments with large common shocks, many farmers and farm advocacy groups often voice strong opposition to tournaments. This is true despite the fact that tournaments can be beneficial to agents (farmers) when common shocks are large (Levy and Vukina; Tsoulouhas and Vukina; Wu and Roe). This leads to a key puzzle which is: why are complaints about tournaments widespread even in industries (e.g. broilers) with large common shocks?[3] For example, a casual perusal of the National Contract Growers Association website[4] will reveal grower complaints about "unfair practices" by large integrators and some of these unfair practices include the use of tournament contracts. Moreover, concerns about tournaments have spread to the policy level. The Producer Protection Act of 2000, a

[3] Levy and Vukina determine empirically that the size of the common shock variance in the broiler industry exceeds the sum of the estimated variances of all other shocks, including idiosyncratic shocks and ability shocks.
[4] http://www.web-span.com/~pga/

model state legislation proposed by Iowa Attorney General Tom Miller and 16 other state attorney generals, proposes to ban "unfair practices" such as the use of tournament contracts.[5]

One plausible explanation for why growers are often strongly opposed to tournaments is that there may be opportunistic behavior on the part of processors who control both the inputs (e.g. quality of the chicks, feed, medicine, etc.) and the performance measurement systems used to determine compensation. As such, it is possible for processors to manipulate the distribution of inputs, which can affect performance, and/or falsify grower rankings to discriminate against certain growers. If these allegations are true, then it is not tournaments per se, but opportunistic behavior that is creating problems for growers. Nonetheless, growers may focus on tournaments since they are the vehicle through which opportunism is expressed. In this case, policy makers ought to focus on disciplining opportunism rather than impose a ban on tournaments as the latter would only remove an important economic instrument without addressing the core problem, which is opportunism.[6] On the other hand, if growers dislike tournaments even in the absence of opportunism, then it may be useful for policy makers to understand why.

The purpose of this paper is to explore the source of discontent with tournaments using data from an economic experiment where opportunism is absent. We are interested in addressing two key questions. First, we want to know which incentive contract is preferred by human subjects as expressed by their willingness to pay to perform under one type of contract versus to another. Second, we are interested in determining the key drivers of subjects' willingness to pay using both actual outcomes and perceived outcomes through a post experiment questionnaire. To answer these questions, we designed a tournament game similar to that of Bull, Schotter and

[5] The Producer Protection Act can be accessed online in its entirety at: http://www.state.ia.us/government/ag/agcontractingexplanation.htm

[6] Leegomonchai and Vukina (2003) provide perhaps the only empirical investigation of possible opportunism among agricultural contracting. They find no significant evidence that contractors discriminate across growers of differing ability when allocating inputs of differing quality.

Weigelt where subjects must make a costly decision which is positively correlated with their performance. Their compensation depends on how well they perform relative to other subjects. The same subjects then participate in a fixed performance standard contract session that differs from the tournament session only in that their payoffs are based on how they perform against some fixed performance standard rather than against the performance of other subjects. After these sessions, the subjects are asked to bid into additional sessions of both tournaments and fixed performance standard sessions. The bid data contain information about subjects' preferences for one type of contract versus the other and can shed light on whether agents dislike tournaments even in the absence of opportunism. At the conclusion of the sessions, we asked subjects to fill out an exit questionnaire which explored their attitudes and expectations, enabling us to gain additional insights into their bidding behavior.

Our results show that subjects generally preferred fixed performance contracts to tournaments. Moreover, the role of monetary incentives (e.g. profitability of a particular contract) is dominated by other considerations such as the fairness of contracts and perceptions of relative riskiness of contracts. Thus, even in the absence of opportunism, human subjects appear to express a preference for fixed performance standard contracts on the basis that they are more "fair" and perceived to be less risky.

While economists are quite familiar with the idea that risk or perceptions of risk can affect economic decisions, the relationship between fairness and behavior is less well understood. However, a series of recent papers in the economics literature have begun to integrate fairness concepts into economic models (E.g. Fehr and Schmidt; Bolton and Ockenfels). Other empirical or experimental studies have shown that, in some settings, subjects do not care solely about monetary payoffs (e.g. Roth; Camerer and Thaler), and that people may not operate solely on the basis of self interest (Rabin). Additionally, understanding the relationship between fairness and efficiency can have important consequences for contract design and policy prescription. Rabin points out that people want to be nice to those who treat them fairly and want to punish those who hurt them. This is related to the notion of *reciprocity* which has gained credibility in the contracting literature recently because it can act as an informal enforcement device which can enhance the value of contracting relationships (Fehr and Gachter). Along these lines, Fehr, Gachter, and Kirchsteiger, suggest that real contracts may be more equitable than those in theory where a principal extracts all surplus above an agent's reservation level. By giving the agent a positive share of the surplus, an agent may reciprocate by providing a level of effort that exceeds the level imposed by the incentive compatibility constraint. Anderhub, Gachter and Konigstein conduct contracting experiments, which show that when agents are given contracts that are perceived to be fair or equitable, agents reciprocate with effort levels that exceed the individually optimal effort level, and vice versa. This suggests that fairness may not just play a distributive role, but can be surplus enhancing as well. Therefore, a deeper understanding about the fairness of alternative contract structures can facilitate more effective policy and contract design.

Before proceeding further, we make a few comments about the relevance of experimental economics for understanding real world contracting issues. We refer to the argument put forth by Noussair and Plott, which is that experiments are not necessarily designed to replicate field situations and all institutional details. Instead, experiments are valuable in that they allow economists to examine general theories that should apply more broadly. If a theory does not apply even in simple, controlled environments, one has to question whether the theory is appropriate for explaining behavior or predicting responses in more complex environments.

Moreover, abstracting from reality is not unique to experiments; indeed, most economic studies which attempt to explain choice using simple, stylized models also base their conclusions on simplifying assumptions and abstractions. Additionally, experiments can provide researchers with an opportunity to test general theories and to inform our understanding by providing insights that are not obtainable through economic modeling alone.

Another question that may arise is whether our use of university students rather than actual farmers weakens the relevance of our results. For this particular research project, we regard our use of students as a strength not a weakness since growers' attitudes toward tournaments may be politicized by recent discussions about the "oppressive" nature of tournament contracts. Hence, we would not be surprised if actual farmers may be biased against tournaments. On the other hand, most university students are not familiar with the politics of contract legislation and might therefore respond in a less biased way.

Overview of the Experiments

Before describing the theory and the specific details of our experimental design, we will provide an overview of our experiments. The purpose of our experiments was to generate bid data for two types of contracts - tournaments and fixed performance standard contracts. As such, we wanted to ensure that our subjects gained actual experience with both types of contracts prior to bidding.

Over a period of about four months, we conducted seven experiments using undergraduate students at a major university in the Midwest. Each experiment included 12 participants whom we recruited via posters and/or email lists across several departments on campus. For each experiment, subjects arrived in a room and were randomly assigned to twelve chairs. For the tournament sessions, subjects were randomly paired together to form six pairs, but subjects were not informed of the identity of his/her pair member. The subjects were subsequently informed that they had the opportunity to earn money and that the amount they earn would be dependent on the decisions they made during the course of the experiment. Each experiment contained four sessions of ten rounds each, where the first session of the night was a tournament game (T), followed by a second session of a fixed performance standard game (F). Upon completion of the first two sessions, we informed the subjects that they had the opportunity to participate in another two sessions - another T session and another F session. We also informed the subjects that they would not gain automatic entry into the second half sessions; instead, they would gain entry based on an auction using their experimental earnings from the first two sessions. The purpose of the auction was to elicit the willingness to pay (WTP) data necessary to explore the objectives of this paper. The auction invited subjects to simultaneously submit sealed bids to enter each of the second half sessions. Each subject was free to bid any dollar amount to enter the second half T session and/or second half F session with the only constraint being that the sum of his/her WTP (bid) to enter the two second half sessions could not exceed his/her experimental earnings from the first half sessions. The subjects with the ten highest expressed WTP for the second half T session gained entry into the T session. Similarly, the ten subjects with the highest bid for the F session gained entry into the additional F session. We also decoupled bids submitted by the subjects from the dollar amount they actually had to pay to gain entry. That is, we required the participants who gained entry into a session to pay only the amount of the 10th place WTP for that session. This auction design decreased the probability that subjects would overbid so that they did not lose too much of their experimental earnings from the first half sessions.[7] There were no restrictions on whether each subject could

[7] In principle, however, our auction design can induce subjects to underbid. Nonetheless, underbidding would not

participate in both, neither, or only one of the second half sessions; it was purely determined by the WTP bids.

Once all four sessions were completed, subjects were asked to fill out an exit questionnaire which contained four questions regarding subjects perceptions about which contract is more profitable, fair, risky and fun. Once these questionnaires were completed, the subjects were paid in cash for their performance for the evening and allowed to leave. The complete questionnaire is in Appendix A. Note that game "A" refers to the tournament and game "B" refers to the fixed performance contract. The answers from this questionnaire were also used in the statistical analysis and generated insights that could not be obtained from the WTP and earnings data alone.

Theory and Experimental Parameters

Now that we have provided an overview of our experiments, we will discuss some of the theory motivating our experiments as well as some of the specifics of our experimental design. Following the literature on experimental tournaments (e.g. Bull, Schotter and Weigelt; Schotter and Weigelt), the design was purposely made simple to ensure that subjects understood the rules of the game. However, it still captures the key features of a real world tournament, which is that each agent's payment depends on how well she performs relative to the other agents. More specifically, we implement two player tournaments where each subject can choose a costly, non-contractible effort denoted by, e_i, for i =1, 2. Performance for subject i is stochastically related to effort via the following production function:

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affect our results since our objective was not to elicit absolute, truthful WTP, but to gain insights into *relative* WTP between tournaments and fixed performance contracts. If actual underbidding occurred, we have no reason to believe that the bias would affect WTP for tournaments and WTP for fixed performance contracts differently, since the WTP for the two types of contracts were elicited at the same time, and under the same auction procedures. Moreover, the potential underbidding problem did not seem to translate into many zero bids. Only five of the subjects across all of our experiments bid zero for at least one of the contract - four for the tournament and one for the fixed performance contract.

(1) $y_i = e_i + u_C + u_i$ $i = 1, 2$

where y_i is performance, u_C is a common shock that affects both agents, and u_i is an idiosyncratic shock that is independently and identically distributed across agents. All random variables are normally distributed so that $u_C \sim N(0,\sigma_C^2)$, $u_i \sim N(0,\sigma)$, $Cov(u_C,u_i) = 0$, and $Cov(u_i,u_j) = 0$, $\forall\ i \neq j$.

The tournament rule is simple: for the two player tournament, if $y_i > y_j$, then player i receives a high payment denoted by R and player j receives a low payment, r, where $R > r$. On the other hand, if $y_j > y_i$, then player j gets R while player i gets r. Moreover, we assumed that both agents have effort cost functions that satisfy the assumptions: $c(0) = 0$, $c'(e_i) > 0$ and $c''(e_i) > 0\ \ \forall i$. We also adopt the cost structure used by Schotter and Weigelt in their experiments, which is of the form,

(2) $$c_i(e_i) = \frac{\alpha_i e_i^2}{k}$$

where $k > 0$. Schotter and Weigelt allowed for heterogeneity across agents by letting the parameter α_i vary across agents. For four of our seven experiments, we imposed homogeneity by letting $\alpha_i = 1$, across all agents and for three of the experiments, we let $\alpha_i = 1.5$ for half the agents and $\alpha_j = 1$ for the other half and then randomly matched a high cost agent with a low cost agent. These variations allowed us to explore how WTP might be affected by heterogeneity in effort costs.

Since tournaments imply that agent i receives a high payment R if $y_i > y_j$ and a low payment r if $y_i < y_j$[8], the probability of agent i receiving the high payment is Prob($u_i - u_j > e_j - e_i$) where $u_i - u_j \sim N(0, 2\sigma^2)$ so that the risk agent i faces is double the

[8] In the case of a tie, we flipped a coin to determine the winner in our experiments.

idiosyncratic variance. Tournaments are beneficial when the common shock variance exceeds the idiosyncratic shock variance; in other words, when the common shock variance makes up more than half of the total variance, then we have $\sigma_C^2 + \sigma^2 > 2\sigma^2$ so that the agent faces less risk under T since σ_C^2 would be eliminated. If we let Prob($u_i - u_j > e_j - e_i$) = 1 - $F(e_j - e_i)$ where *F*(●) is the cumulative density function of $u_i - u_j$, agent *i's* objective function becomes,

(3) $$E(\pi_i^T) = \left[1 - F(e_j - e_i)\right]R + F(e_j - e_i)r - \frac{\alpha_i e_i^2}{k}$$

which, after some algebra, can be written as:

(4) $$E(\pi_i^T) = r + \left[1 - F(e_j - e_i)\right]\left[R - r\right] - \frac{\alpha_i e_i^2}{k}$$

Similarly, agent *j's* objective function is:

(5) $$E(\pi_j^T) = r + F(e_j - e_i)\left[R - r\right] - \frac{\alpha_j e_j^2}{k}$$

We should also point out that, while we assumed that agents are risk neutral to be consistent with previous studies, our experimental subjects may exhibit non-neutral risk preferences. Therefore, we treat the following theoretical predictions as benchmarks rather than exact predictions. We could have attempted to induce risk preferences in our experiments using techniques developed by Berg et al, but such procedures may not be very reliable.

Returning to the model, the two agents essentially play a game where effort choices are their strategies and their payoffs are given by (4) and (5). For homogeneous cost functions, where $\alpha_i = \alpha_j = 1$, the solution is straightforward to derive. The first order conditions with respect to effort are,

(6) $$\frac{\partial E(\pi_i^T)}{\partial e_i} = f(e_j - e_i)\left[R - r\right] - \frac{2e_i}{k} = 0$$

(7) $$\frac{\partial E(\pi_j^T)}{\partial e_j} = f(e_j - e_i)\left[R - r\right] - \frac{2e_j}{k} = 0$$

where $f(\bullet)$ is the density function. Conditions (6) and (7) suggest that,

(8) $$\frac{2e_i}{k} = f(e_j - e_i)\left[R - r\right] = \frac{2e_j}{k}$$

so that $e_i = e_j = e^*$ which is a symmetric Nash equilibrium. It is clear that the density function $f(\bullet)$ will be evaluated at zero so that $f(0) = \frac{1}{\sqrt{2\pi(2\sigma^2)}}$ giving us,

(9) $$e_i = e_j = e^* = \frac{k\left[R - r\right]}{2\sqrt{4\pi\sigma^2}}$$

Following convention in the literature, we restrict agents' effort choices to be in the set of integers from [0, 100].[9] We also chose parameters to ensure interior solutions.

The heterogeneous cost game was more difficult to solve since equilibrium effort levels would not be equal between the high cost and low cost agents so that the normal distribution would not reduce to the simple form, $f(0) = \frac{1}{\sqrt{2\pi(2\sigma^2)}}$. Thus, we had to use numerical methods to solve the first order conditions for equilibrium effort levels. See Table 1 for a list of equilibrium effort levels under different cost conditions and parameters. Details of the other parameters in the table will be discussed shortly.

Turning now to the fixed performance contract (F), we focus attention on F contracts with binary payoffs so that an agent *i* receives the high payoff *R* if his output exceeds some fixed standard y^* and *r* otherwise. The probability that agent *i* receives the high payoff is

[9]Bull, Schotter and Weigelt, and Schotter and Weigelt impose the same restriction. We tried to maintain consistency with other studies in much of our experimental setup so that we have some basis for comparison when assessing final results.

$\Pr ob(y_i > y^*)$, which is equivalent to Prob($u_C + u_i > y^* - e_i$), where $u_C + u_i \sim N(0,\ \sigma_C^2 + \sigma^2)$.

Letting $G(\bullet)$ be the cumulative density function of $u_C + u_i$, we have Prob($u_C + u_i > y^* - e_i$) = 1 - $G(y^* - e_i)$. Agent *i's* objective function is,

$$(10) \qquad E(\pi_i^F) = r + \left[1 - G(y^* - e_i)\right]\left[R - r\right] - \frac{\alpha_i e_i^2}{k}$$

with first order condition:

$$(11) \qquad \frac{\partial E(\pi_i^F)}{\partial e_i} = g(y^* - e_i)\left[R - r\right] - \frac{2\alpha_i e_i}{k} = 0$$

Because $g(\bullet)$ is a normal density function, solving (11) for e_i is complicated. We therefore used numerical methods to generate solutions. See Table 2 for the optimal effort levels under different parameters.

To explain some of the numbers in Tables 1 and 2, we turn to a brief discussion of our choice of experimental parameters. One of our objectives was to maintain some consistency with prior experimental studies on tournaments. Therefore, like Bull, Schotter, and Weigelt, the parameter k in the effort cost function was chosen to be 10,000. Another objective of ours was to explore how variations in the common shock variance might affect behavior. To achieve this, we held constant the total variance (sum of the variances for the common and idiosyncratic shocks) at 500 while varying the relative size of the variance of the common shock across different experiments.

In choosing the payments R and r for the tournament, we had to consider a couple of factors. First, the ability to implement any effort level involved choosing the spread between R and r to ensure incentive compatibility as dictated by equations (6), (7) and (11). Second, the choice of r can be used to determine ex ante expected payoffs for the agents. These are the same sorts of constraints that a real world processor would have to face in designing contracts for growers. The average effort level we chose to implement was $e^* = 37$ as this did not appear to be an obvious number that subjects can focus on thereby biasing the results. We therefore had to choose the spread $R - r$ to implement an effort level of 37, on average. For the symmetric cost experiments, this meant that the equilibrium strategies for both agents in a tournament was 37. For asymmetric cost tournaments, the two agents in a tournament had different Nash equilibrium effort levels, but the average of the two was 37. The low payment r was used to determine the ex ante expected payoffs for agents. While we did not know the actual reservation utilities of our subjects, we did want to ensure an expected payoff of a minimum of \$18.90 per experiment as dictated by our experimental budget. Furthermore, this is about the going rate for a two-hour student experiment on the campus that hosted the experiment. Each experiment consisted of four ten round sessions for a total of forty rounds of play. Thus, the per-round “participation constraint” involved dividing 18.90 by 40 to get .4725.

Once we knew what effort level to implement and the participation constraints to satisfy, we were able to pin down optimal values of R and r. For example, with a target effort level of 37, a cost parameter of k =10,000, and assuming that the variance of the idiosyncratic shock is half the total variance of 500 (i.e. $\sigma^2 = 250$ or $\sigma = 15.8$), the optimal payment spread is $R - r = .41$ for the symmetric cost experiments. To pin down r, we can use the per-round participation constraint,

(12) $$r + F(0)\left[R - r\right] - \frac{e_i^2}{k} = r + \frac{1}{2}\left[.41\right] - \frac{37^2}{10000} \geq 0.4725$$

Solving for r yields $r \approx .40$.[10] These parameters correspond with the numbers for Experiment 1 in Table 1. Parameters for the other experiments, where we varied the relative sizes of the common and idiosyncratic shocks and/or the parameter α, were generated in a similar fashion and displayed in the remaining rows of Table 1.

In calibrating parameters for the fixed performance contract, we had two goals in mind. First, we wanted the average optimal effort level to be 37 as under the tournaments. This allowed us to study how effective fixed standard contracts were relative to tournaments in achieving the same performance objectives. Second, we wanted to maintain the reservation utility of .4725 per round in calibrating parameters for the fixed performance standard contract so that, ex ante, risk neutral subjects would have no reason to prefer one type of contract over the other.

Note that the incentive compatibility constraint for the agent is given by (11) so that, given a choice of $e_i = 37$, we can solve for the payment spread.[11] However, we had to first choose a fixed standard y^*, which output must exceed in order for the agent to receive the high payment R. An obvious choice was $y^* = 37$, but we avoided this choice because we did not want to provide our subjects with a focal point so that they might naturally gravitate toward the optimal solution of 37. Instead, we chose $y^* = 41$ and then adjusted our payment spread to ensure that 37 was the optimal choice. Since $y^* > 37$, $g(\bullet)$ in (11) does not simplify into an easily manageable form as in the symmetric cost tournament model. This forced us to numerically solve for the optimal wage spread which was $R - r = .42$ for the symmetric cost experiments (Experiments 1,2, 3, and 7). Additionally, the value of r that would result in an

[10] We say "approximately" 40 because our numerical calculations had minor rounding errors. For example, effort was actually 36.83 for an idiosyncratic variance of 250 and a pay spread of .41. The expected payoffs were also slightly different from .4725 due to minor approximation errors but the payoff did not deviate by more than 0.001.
[11] We also evaluated the second order conditions at 37 to ensure that we are at a maximum.

expected payoff of .4725 per round to satisfy the participation constraint was r = .43. Parameters for the asymmetric cost experiments (Experiments 4, 5, and 6) were generated in a similar fashion and also displayed in Table 2.

With experimental parameters in hand, we can now provide more details about our experiments. For Experiment 1, each agent's output is the sum of an effort integer from 0 to 100, an idiosyncratic shock, u_i , distributed $u_i \sim N(0, 250)$, and an aggregate shock, u_C, distributed $u_C \sim N(0, 250)$, to get $y_i = e_i + u_C + u_i$. The output for agent j is similarly defined. We approximated a normal distribution with mean 0 and variance 250 using 300 pennies in a bucket where each penny was marked with an outcome for the random shocks. The outcomes were represented by integers and the frequency for each outcome was determined by approximating the number of outcomes out of 300 that might occur under a normal distribution.[12] Distributions for other values of σ_C^2 and σ_2 were approximated using the same method. For the T sessions, if $y_i > y_j$, then agent i gets R = .81 and agent j gets r =.40, and if $y_i < y_j$, then agent i gets r =.40 and agent j gets .81. The F sessions were identical to the T sessions, except that each subject played against a fixed standard of $y^* = 41$, rather than against a pair member.

Earlier, we mentioned that each experiment involved four sessions and an auction. Prior to the auction, each subject performed under both a T session and an F session. Then the auction took place and the ten highest bidders for T were subsequently allowed to play in another T session. The ten highest bidders for F were allowed to play in another F session. In each session, the subjects played ten identical rounds of the contracting game, where in each round,

[12] The exact method that we used was to calculate the probability mass function in Excel for a normal distribution with mean zero, and standard deviation 15.8. We then multiplied the probability for each outcome by 300 and rounded it to the nearest integer. The resulting integer represented the frequency for that particular outcome.

each subject was asked to choose a "decision number" (effort) from 0 to 100. The higher the decision number, the higher the cost of that decision to the subject as dictated by the cost function (2) with k= 10,000 and α_i = 1 or 1.5 depending on the experiment. After the decision numbers were chosen, the subject would enter these numbers into their worksheets and an administrator would record the decisions in a computer. Subsequently, one subject would draw a "common shock" number from a bucket with frequencies that approximated a normal distribution and all subjects added this number to their decision numbers. Then each subject drew a number from another bucket with frequencies approximating another normal distribution, and then this individual number was added to his/her decision number and the common shock number. Copies of the probability distributions for both the idiosyncratic and common shocks were given to subjects prior to the beginning of the experiment and explained in detail so these distributions were common knowledge.[13] The sum of the decision number, the common shock, and the idiosyncratic shock would be "performance" as given in equation (1). In the tournament sessions, the administrator would compare outputs of pair members and the pair member with the higher output would receive the high payment R while the other pair member got r (as outlined in the theoretical section). For the F sessions, the administrator compared output to the fixed standard of 41. Each subject only knew her payment received and not the difference in output.[14] Each subject would record the payment in the worksheet and subtract the decision cost to get net earnings for that round. After the round ended, the next round began and the entire

[13] We discussed how these distributions were constructed earlier.
[14] This is consistent with the way many comparative performance contracts work where growers/workers are informed about their rankings but are not provided detailed information about competitors' performance.

process was repeated. At the end of the tenth round, the subjects calculated their payoffs for the ten rounds.[15]

All subjects received cost sheets that mimicked their cost functions, knew the distribution of the numbers in the buckets, and were informed of all other experimental parameters, including opponents' cost functions. Only the identity of the pair members was not common knowledge.   A session typically lasted between 20-25 minutes; several non-paying, practice rounds were played before each session to ensure that subjects understood the experiment. Complete instructions for the experiments are available upon request.

Hypothesis and Results

In this study, we were primarily interested in addressing the question of whether subjects prefer fixed performance standard contracts (F) to tournament contracts (T) as expressed by their willingness to pay generated from the auction. We also explored some of the key drivers of WTP using both actual outcomes from the experiments as well as answers from the post experiment questionnaire, which elicited subjects' motives and perceptions. We constructed our experiments to yield the same expected profits to the agents under both types of contracts. However, ex post, earnings differed across agents so that some agents earned more under T sessions whereas others earned more under F sessions. This difference in net payoffs provides monetary incentives that can affect agents' bidding behavior for the two contracts. However, even if subjects earned more under one type of contract versus another, they may *expect* future outcomes to differ; thus, the post experiment questionnaire allowed us to control for expectations. In analyzing our data, we organize our analysis around the following hypothesis:

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[15] While the tournament was repeated over 10 rounds, the theory is based on a static model. Such repetition is common in experimental practice because subjects make complex decisions. Moreover, the only subgame perfect Nash equilibrium to a finitely repeated game involves the choice of the Nash equilibrium decision level to the one-shot game. Thus, predictions concerning equilibrium play were independent of finite repetition (Bull, Schotter, and Weigelt).

Hypothesis: Subjects will have higher willingness to pay for contracts under which they earned or expect to earn more money.

This is a simple prediction that is consistent with standard rational choice theory which assumes that agents seek to maximize their own monetary payoffs.

We begin by examining descriptive statistics to get an overview of the data. Table 3 presents mean bid size (WTP), mean earnings per round for the subjects, along with other interesting statistics. While there were seven experiments of 12 subjects each, we ended up with 79 usable observations since 5 of the subjects were repeat participants. One can see immediately that the mean bid size was higher for F contracts rather than T contracts. Moreover, 36 subjects bid more for F than T. This appears to be consistent with earnings as subjects earned more per round under F than T. Since there were ten rounds in a session, on average, subjects earned $3.90 per T session and $4.60 per T session. However, if we examine the final row, we see that not all of our subjects behaved in a way consistent with rational choice theory. In particular, 74% of subjects that earned *more* under T did *not* bid more for tournaments and 46% of subjects that earned more under F did not bid more for F. This suggests that many subjects did not behave in accordance with rational choice theory. While this appears perplexing, it is not inconsistent with earlier experimental findings in other contexts. Rabin points out that experimental research has shown people to deviate from self interest for a number of reasons, among which is the concern for fairness and/or because they don't always accurately assess their own experienced well being from past decisions.

Another possibility is that our subjects are rational but their *expectations* about the relative profitability of the two types of contracts may differ from what they actually earned. For example, a subject who earned more under a T session may still bid more to participate in a future F session if she *expects* to earn more under F in the future. In other words, the subject may have attributed the larger earnings from the T session to luck. To analyze this possibility, we relate bidding behavior to Question 1 in our Exit Questionnaire. This question asks "If each game were played many times under the same conditions as today, which one would earn you more money?" The subject was asked to answer this question on a 1-5 scale where "1" would indicate that the subject believes that the T session would be most profitable whereas a "5" would indicate that the F session would be most profitable. We can use the results from this question to determine whether subjects are rational when using a forward looking criterion.

Figure 1 provides a graphical representation of subjects' bidding behavior conditional on their responses to Question 1. That is, we partition subjects into those that responded to question 1 by choosing an answer that is less than or equal to 3 (either "1", "2", or "3"), and those that chose an answer that is greater than 3 (either "4" or "5"). Those subjects who responded to the question with an answer that was greater than "3" expect F to be more profitable, while those subjects who responded with an answer that was "3" or lower expect T to be at least as profitable as F. One can see that, for those that expect F sessions to be more profitable (responses > 3), more than 50% expressed a higher WTP for F sessions as expressed by their higher bids for this contract. Nonetheless, a non-trivial fraction (nearly 50%) still did not bid more F sessions which implies that there is still some "irrational" behavior amongst these subjects. Turning to the bidding behavior of subjects that expect T sessions to be at least as profitable as F session (responses ≤ 3), we can see that close to 70% of these subjects bid at least as much for T sessions. Nonetheless, a non-trivial fraction (slightly more than 30%) still bid more for F sessions indicating a certain amount of "irrationality". Thus, we can still conclude that a non-trivial fraction of subjects behaved in a way that contradicts our main hypothesis.

What else might affect WTP for the two types of contracts? Recent economic studies have shown that other motivators besides monetary incentives, such as fairness, can play a significant role in influencing behavior in experiments (e.g. Andreoni, Castillo, and Petrie; Bolton and Ockenfels; Fehr and Schmidt; Rabin, among others). Our post experiment questionnaire allows us to directly examine subjects' perceptions of which contract is more fair. Question 2 asks "Which game did you think was more fair to all the participants involved?" While economists have provided different definitions of fairness, we avoided imposing any particular definition in constructing our questionnaire because we did not want to constrain the way subjects responded to the question. Most people have an intuitive notion of what fairness is even if no precise definition is given. The downside of not providing a precise definition is that the responses do not allow us to address questions about any specific definition of fairness found in the literature, such as inequality aversion, reciprocity, etc. However, our main interest was not to test any specific theory of fairness, but rather to understand whether subjects are motivated by general fairness considerations that could cause their behavior to deviate from pure self interest.

Figure 2 provides a graphical view of subjects' responses to Question 2. One can see immediately that responses tend to be in favor of F contracts. Indeed, over 50% of our 79 subjects strongly believed that F sessions were more fair as evidenced by their "5" answers to the question. Slightly more than 20% believed that the contracts were equally fair and a very small fraction felt that tournaments were more fair (answers of either "1" or "2"). The data from Question 2 can help us gain additional insights about the behavior of those subjects who do not behave in a way that is consistent with our main hypothesis. In particular, we were interested in examining the group of subjects that behaved in a strongly irrational way. By strongly irrational, we mean those subjects who either, (1) responded to Question 1 with an answer of "3" or lower (expect T to be at least as profitable as F) and yet bid strictly more for F than for T, or (2) responded to Question 1 with an answer greater than "3" (expect F to be more profitable) and yet bid strictly more for T than for F. There were sixteen such subjects among the 79 and we provide a graphical overview of their bidding behavior conditional on their responses to the fairness question in Figure 3. Note that five of the sixteen subjects answered Question 2 with "3" or lower indicating that these five felt that T is at least as fair as F, whereas eleven of the sixteen responded with an answer greater than "3" indicating that these eleven believed F was fairer.

One can see from Figure 3 that four of the five subjects that responded with an answer less than or equal to "3" (T at least as fair as F) bid more for T. Seven of the eleven subjects that responded with an answer greater than "3" (F more fair) bid more for F. Thus, it appears that the majority of these sixteen subjects behaved in a way that indicates that they have preferences for fairness even if their responses to monetary incentives alone make them appear "irrational."

So far, we have not considered the potential impact of risk aversion on subjects' preferences for the two types of contracts. If risk preferences matter, then concluding that subjects do not behave rationally on the basis of them not bidding higher for contracts that yield higher monetary or expected monetary payoffs may be short sighted. Unfortunately, subjects' risk preferences are largely unobservable and methods for inducing preferences (e.g. Berg, et. al.) are somewhat controversial (Selten, Sadrieh, and Abbink) and may not be very reliable. Alternatively, we can impose a risk structure on our subjects but this would be a rather arbitrary and may lead to additional questions about whether our risk assumptions are valid. However, while we cannot predict exactly how risk aversion, if any, might affect behavior, our experimental setup does allow us to indirectly assess how sensitive the bids were to changes in the risk environment. Moreover, Question 3 of the exit questionnaire directly asked subjects about their perceptions of the relative riskiness of the two contracts. Thus, we have both an objective, as well as a subjective method of assessing how risk preferences may impact our results. We will discuss each method in turn beginning with the objective method first.

Recall from our theoretical section that agents face less risk under T than under F when $\sigma_C^2 > \sigma^2$ since total risk under T is $2\sigma^2$ whereas total risk under F is $\sigma_C^2 + \sigma^2$. Hence, F was at least as risky as T in experiments 1, 3, 4, and 5, whereas F was less risky than T in experiments 2, 6, and 7. If risk preferences of subjects are non-neutral enough to affect their bidding behavior, we ought to observe a significant difference in the distribution of bids between the two sets of experiments. We conducted the Wilcoxon Rank-Sum test for unmatched data to test whether there is a significant difference in the distribution of bids across the two sets of experiments (those for which F was at least as risky as T and those where F was less risky). The p-value for the F bids was 0.49 so that we cannot reject the null that there is no difference in the distribution of F bids across the two sets of experiments. The p-value for the T bids was 0.03 so that we can reject the null at the 5% level that the distribution of T bids was the same across the two sets of experiments. We also defined a variable $\Delta bid = bid(F) - bid(T)$, where $bid(F)$ is the bid for the F session by subject *i,* and $bid(T)$ is the bid for the T session by subject *i.* This variable measures the difference in bid size for each contract by each individual. Applying the Rank Sum test to this variable yielded a p-value of 0.25 which does not allow us to reject the null that the distribution of this variable is the same across the two sets of experiments. Thus, there appears to be mixed evidence as to whether a change in common shock affected bidding behavior.

Question 3 of the post experiment questionnaire gave us a subjective means of examining the impact of risk. This question asks subjects the following question: "Which game did you think was more risky?" Of the 79 subjects, 66 felt that T sessions were more risky whereas 13 felt that F was more risky. Figure 4 relates bidding behavior to perceptions of the relative riskiness of the two contracts. Of the 66 subjects that felt that T sessions were at least as risky as F, approximately 50% bid higher for the F, whereas only a little over 10% bid more for T. Of the 13 subjects that felt that F sessions were more risky, approximately 38% bid more for T, whereas only about 23% bid more for F. At a broad level, these results seem to be intuitively plausible as subjects tend to bid more for the contract that they perceived to be less risky although this relationship wasn't perfect.

So far, we have related bidding behavior to individual variables such as actual earnings per round, expected profitability, perceptions of fairness and perceptions of risk. In doing so, we did not examine how each of these variables affected bidding behavior holding the other variables constant. Thus, it would be useful to conduct a regression analysis that allows us to address *ceteris paribus* questions about the impact of each variable on bidding behavior. We estimated several different regressions that were variations of the following general form:

(13) $$\Delta bid = \beta_0 + \beta_1 profit + \beta_2 fairness + \beta_3 risk + \beta_4 fun + \beta_5 \sigma_C + \beta_6 \cos t + \varepsilon$$

where Δbid was defined earlier and represents the difference in bids for F and T for each subject. If $\Delta bid > 0$ for subject *i,* then this means that subject *i* bid more for F than for T and vice versa. The variable, *profit* indicates some measure of monetary payoffs to the subjects for each contract. We use both a backward looking measure and a forward looking measure. The backward looking measure is captured by a variable $\Delta netpayoff = payoffs(F) - payoffs(T)$ where *payoffs(F)* measures the average pre-auction net earnings per round for a subject under contract F, and *payoffs(T)* measures the average pre-auction net earnings per round for the same subject under contract T. The forward looking criterion is captured by Question 1 of the post experiment questionnaire that we discussed earlier. Regressions (1) and (2) in Table 5 used the forward looking measure whereas regressions (3) and (4) used the backwards looking measure. The *fairness* and *risk* variables are captured by Questions 2 and 3 of the post experiment questionnaire which were also discussed earlier. The *fun* variable is strictly a control variable since it is possible for subjects to bid a certain way if they felt that certain sessions were more entertaining than other sessions. *Fun* was captured by Question 4 of the post experiment questionnaire and is measured on the same scale as the other questions. That is, if a subject strongly believed the F session to be more fun, she circled "5" and if she strongly believed T sessions to be more fun, she circled "1". The σ_C variable simply represents the common shock standard deviation which varied across experiments. Finally, *cost* was represented by two different types of dummy variables. The first cost dummy was the *Symmetric Cost* dummy which took a value of "1" if an observation came from experiment 1, 2, 3, or 7 and "0" otherwise. The second dummy we defined was the *Asymmetric Cost-High Cost* dummy which took a value of "1" if the observation came from an asymmetric cost experiment (experiments 4, 5, or 6) AND the associated subject had the high effort cost function ($\alpha_i = 1.5$). These dummies were used as controls since the subjects bidding behavior may be influenced by whether they were advantaged, disadvantaged, or neither during the experiments. Regressions (1) and (3) in Table 5 used the *Symmetric Cost* dummy whereas regressions (2) and (4) used the *Asymmetric Cost-High Cost dummy.*

Although we estimated four different variations of (13), the results from Table 5 indicate that there were no differences in qualitative conclusions and only minor differences in quantitative conclusions across the four regressions. Surprisingly, neither the backward or forward looking measures of profitability were significant in any of the regressions which contradicts our main hypothesis that subjects should have higher WTP for contracts under which they earned or expect to earn more money. Instead, the dominant drivers of bidding behavior appear to be perceptions of fairness and relative riskiness of the two contracts. The coefficients for both of these variables were significantly different than zero at either the 5% or 10% levels depending on the regression. No other variables were significantly different from zero even at the 10% level of significance.

While our results are surprising, it is not inconsistent with some empirical findings in the economic literature, which have shown that fairness considerations can compete with monetary incentives. For example, Kahneman, Knetsch, and Thaler show that some firms may be reluctant to fully exploit their monopoly pricing power because buyers may have strong feelings about the fairness of pricing practices. Guth, Kliemt, and Ockenfels look at the tradeoff between fairness and efficiency in two sided gift giving games. They find that efficiency concerns can be dominated by fairness concerns.

At this point, it may be useful for us to provide a discussion about why subjects may perceive one type of contract to be more fair than the other. As was illustrated in Figure 2, an overwhelming majority of subjects believed F contracts to be at least as fair as T contracts. We offer some possible explanations for why this might be the case. First, Konigstein, Kovacs, and Zala-Mezo suggest that perceptions of fairness may be related to the way outcomes differ from a reference point. Reference points can come from many sources including previous own outcomes. In this regard, tournaments may be perceived as unfair because subjects may experience high payments in one round but low payments in other rounds even if their performance and/or effort does not vary much. When this is combined with Rabin's discussion about how people tend to be significantly more averse to losses (relative to their reference point) than gains of the same size, it's possible that subjects may feel cheated by tournaments. This supposition is also consistent with grower complaints in the real world. Tsoulouhas and Vukina provide the following quote from a GIPSA report, " ….consecutive flocks grown by the same grower having similar production costs could receive substantially different payment amounts because of the results of other growers in the settlement group. Growers have expressed exasperation over this form of settlement because they have no way of estimating in advance how much to expect in payment." Another possibility is that tournaments naturally implement unequal distribution of payments. That is, tournaments, by design, do not allow both pair members to receive the high payment in any given round regardless of performance so that there will be inequality in the distribution of wealth. In other words, since there is always one "winner" and one "loser", tournaments implement social inequality. If people are inequality averse (Fehr and Schmidt) then tournaments might be perceived as unfair.

Conclusion

We conclude this paper by discussing some of the main implications of our findings. First, our results suggest that risk perceptions matter for the way people evaluate their economic alternatives. This finding simply confirms conventional wisdom in economics that risk can affect economic decisions. Second, and more surprising, is our finding that subjects' perception of fairness is significantly correlated with their WTP for different types of contracts. Economists have paid relatively little attention to fairness until recently, but our results suggest that it may be a significant motivator of people's choices, and may, in some contexts, be a more significant driver of behavior than monetary incentives.

On a policy level, our findings can inform our understanding of why growers may dislike tournaments. Even in the absence of opportunism, it is possible that humans might perceive tournaments to be "unfair." This finding is important because fairness and efficiency may be inseparable in contracting relationships. Indeed, recent research in the economics literature has shown that fairness and efficiency are not mutually exclusive nor are they necessarily substitutes. Experimental research has also shown that a perceived lack of fairness can stifle cooperation and other surplus enhancing activities by contracting parties. Additionally, our research may be of interest to contract designers (e.g. processors) because if fairness enters growers' utility functions, processors may have to pay a "fairness premium" in order to meet growers' reservation utilities.

Our findings provide a springboard for future research. A question that may be of interest to both contract designers and policy makers is: if tournaments are perceived to be less fair, would the use of tournament contracts still be efficiency enhancing in environments with large common shocks? When contracts are perceived as unfair, cooperation and positive reciprocity may be stifled which can reduce the surplus available for sharing between contracting parties. Thus, there may be a tradeoff between the positive effects of tournaments (elimination of common shocks) and the negative effects (reduced cooperation and positive reciprocity). Understanding these tradeoffs may provide another perspective on whether proposals to restrict the use of tournaments contracts would be welfare enhancing.

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Table 1: Experimental Parameters for the Tournament Sessions.

	$e_i^* (e_j^*)$	R	r	σ_C^2	σ^2	*Effort Cost*
Experiment 1	37(37)	\$0.81	\$0.40	250	250	Symmetric $\alpha_i = \alpha_j = 1$
Experiment 2	37(37)	\$0.88	\$0.33	50	450	Symmetric $\alpha_i = \alpha_j = 1$
Experiment 3	37(37)	\$0.77	\$0.45	350	150	Symmetric $\alpha_i = \alpha_j = 1$
Experiment 4	44(30)	\$0.95	\$0.33	250	250	Asymmetric $\alpha_i = 1$ $\alpha_j = 1.5$
Experiment 5	44(30)	\$0.90	\$0.35	350	150	Asymmetric $\alpha_i = 1$ $\alpha_j = 1.5$
Experiment 6	44(30)	\$0.99	\$0.29	150	350	Asymmetric $\alpha_i = 1$ $\alpha_j = 1.5$
Experiment 7	37(37)	\$0.909	\$0.31	0	500	Symmetric $\alpha_i = \alpha_j = 1$

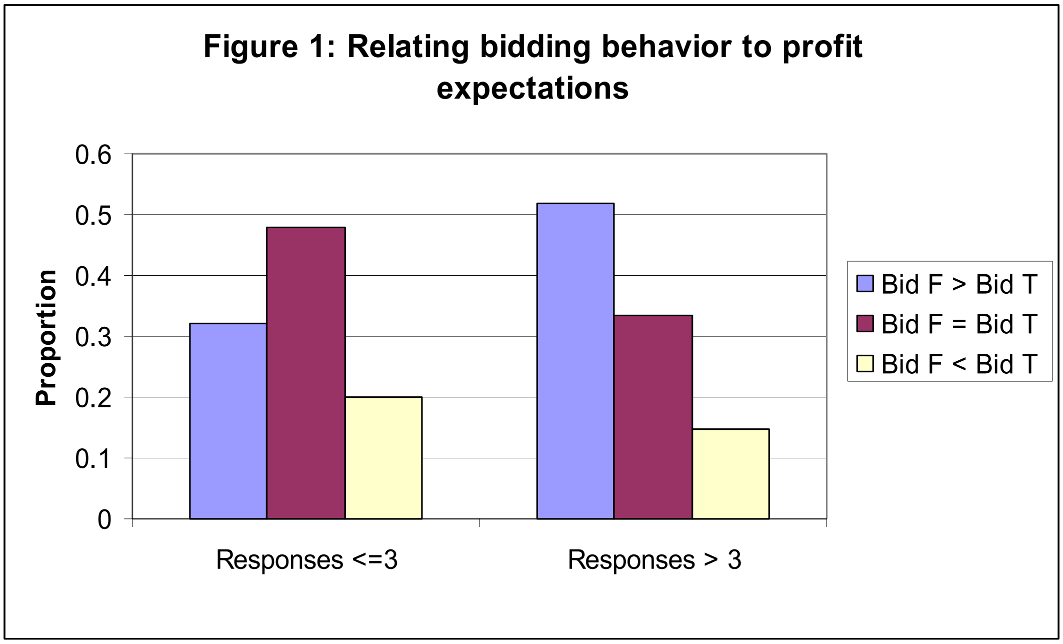
Table 2: Experimental Parameters for the Fixed Performance Standard Sessions.

	$e_i^*(e_j^*)$	R	r	σ_C^2	σ^2	*Effort Cost*
Experiment 1	37(37)	\$0.85	\$0.43	250	250	Symmetric $\alpha_i = \alpha_j = 1$
Experiment 2	37(37)	\$0.85	\$0.43	50	450	Symmetric $\alpha_i = \alpha_j = 1$
Experiment 3	37(37)	\$0.85	\$0.43	350	150	Symmetric $\alpha_i = \alpha_j = 1$
Experiment 4	44(30)	\$0.95	\$0.40	250	250	Asymmetric $\alpha_i = 1$ $\alpha_j = 1.5$
Experiment 5	44(30)	\$0.95	\$0.40	350	150	Asymmetric $\alpha_i = 1$ $\alpha_j = 1.5$
Experiment 6	44(30)	\$0.95	\$0.40	150	350	Asymmetric $\alpha_i = 1$ $\alpha_j = 1.5$
Experiment 7	37(37)	\$0.85	\$0.43	0	500	Symmetric $\alpha_i = \alpha_j = 1$

Table 3: Descriptive Statistics (n = 79 subjects)

	Tournament Contract (T)	*Fixed Performance Standard Contract (F)*
Mean bid size (standard deviation)	$2.90 ($1.66)	$3.38 ($1.54)
Mean earnings per round (standard deviation)	$0.39 ($0.14)	$0.46 ($0.08)
# that bid higher for contract*	13	36
# that bid "0" for contract	4	1
% that did *not* bid higher for the contract despite earning more under it.	74% (17/23)	46% (6/13)

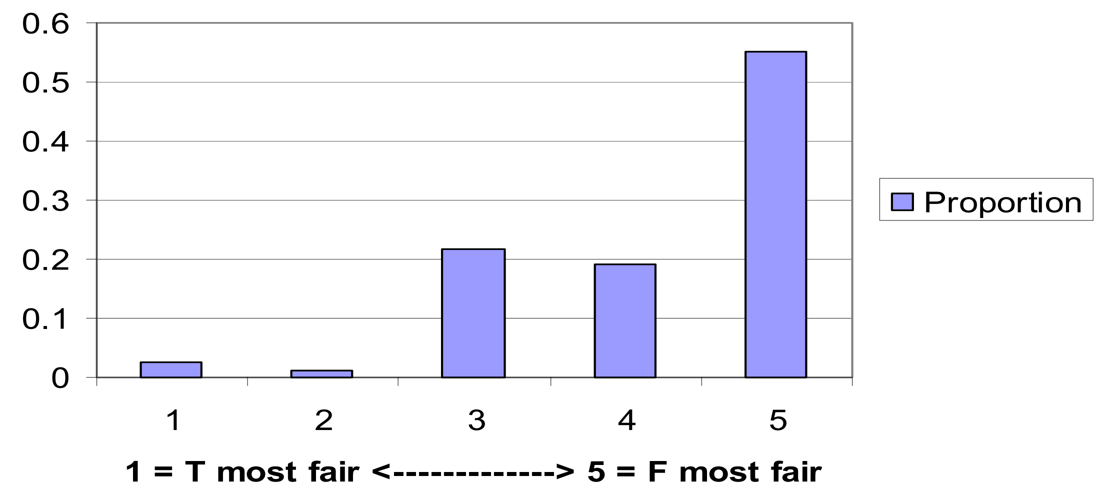
Note 1: that there were 10 rounds per session and each subject could participate in up to four sessions (two tournaments and two fixed performance standard) per experiment. Thus, if a subject earns the average per round under each contract, he/she would make $3.90 for a tournament session and $4.60 per fixed performance standard session.
*30 subjects bid the same amount for the contracts.



Note 1: Responses ranged from 1 – 5. A “1” implies subjects strongly expect tournaments to be more profitable. A “5” implies that subjects strongly expect fixed performance contracts to be more profitable. Anything greater than “3” implies subjects believe fixed performance contracts to be more profitable. Any response of “3” or less means the subjects expect tournaments to be at least as profitable as fixed performance standard contracts.

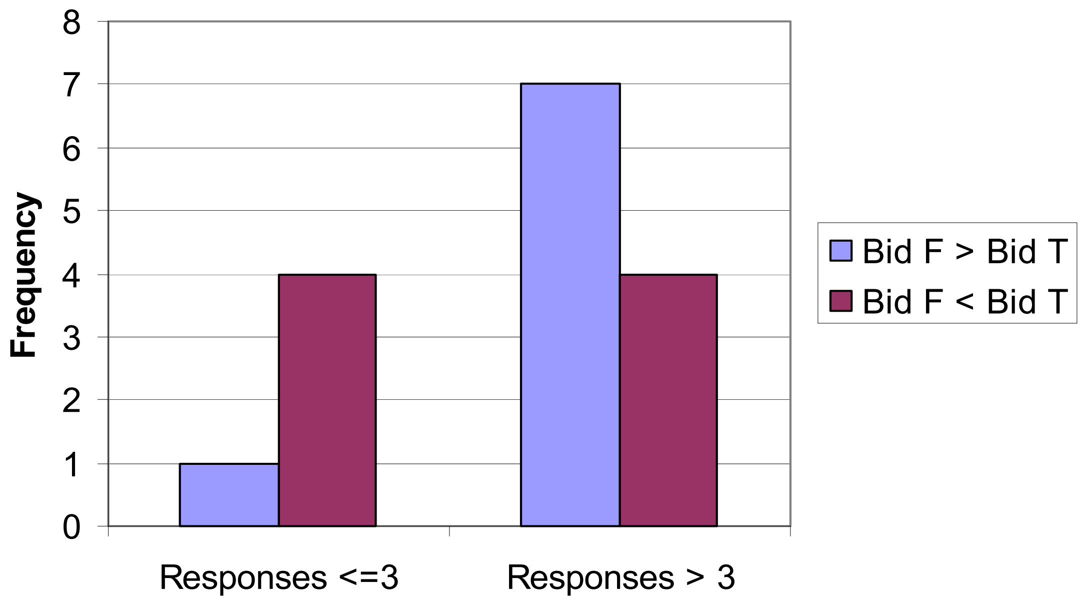
Note 2: 25 subjects fell into the “responses ≤ 3” category and 54 subjects fell into the “response > 3” category.

Figure 2: Subjects' perceptions of the relative fairness of contracts

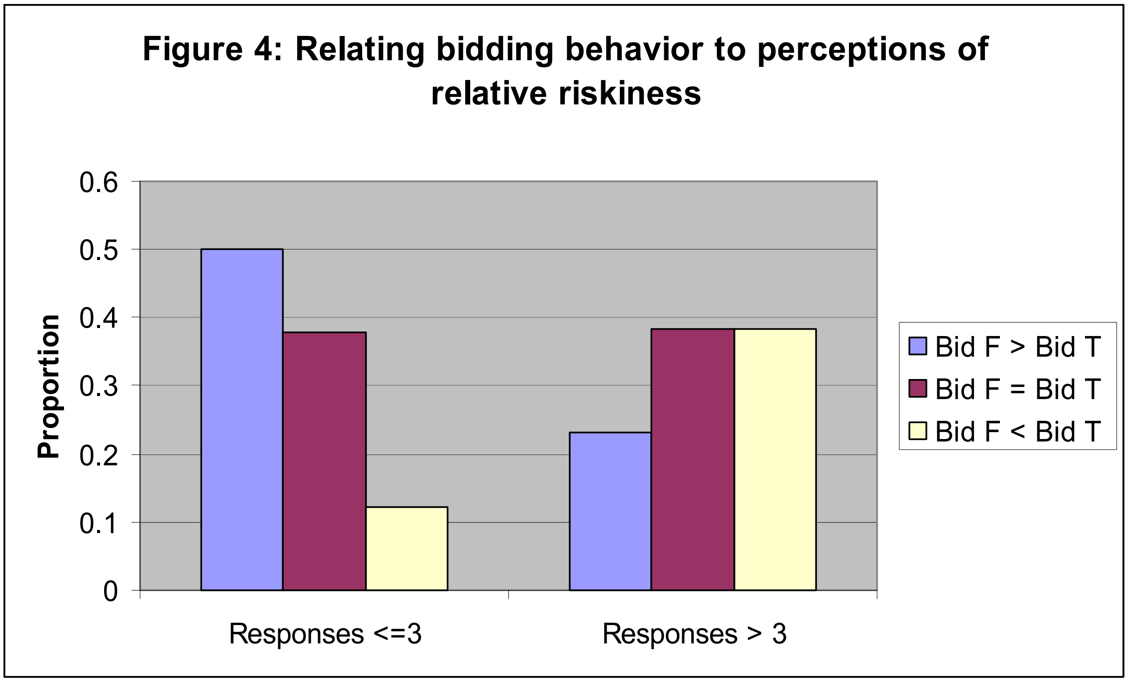


Note: Responses ranged from 1 – 5. A "1" implies subjects feel strongly that tournaments are more fair. A "5" implies that subjects strongly feel that fixed performance standard contracts are more fair. Anything greater than "3" implies that subjects believe fixed performance contracts to be more fair. Anything less than or equal to "3" implies that subjects perceive tournaments to be at least as fair as fixed performance standard contracts.

Figure 3: Bidding behavior of the 16 irrational subjects conditional on their perceptions of fairness



Note: Of the 16 subjects that did not behave in a "rational" way, five of the subjects responded to Question by indicating that they felt tournaments were at least as fair as fixed performance standard contracts (a response of "3" or less) and eleven of the subjects indicated that fixed performance contracts were more fair (a response greater than "3"). The above table examines the bidding behavior of these two groups of responders.



Note 1: Responses ranged from 1 – 5. A “1” implies subjects strongly expect tournaments to be more risky. A “5” implies that subjects strongly expect fixed performance contracts to be more risky. Anything greater than “3” implies subjects believe fixed performance contracts to be more risky. Any response of “3” or less means the subjects expect tournaments to be at least as risky as fixed performance standard contracts.
Note 2: 66 subjects fell into the “responses ≤ 3” category and 13 subjects fell into the “response > 3” category.

Table 5: Regression Estimates – Dependent variable is Δbid (in dollars)

Variables	*(1)*	*(2)*	*(3)*	*(4)*
$\Delta netpayoffs$	0.74 (1.64)	0.80 (1.66)	--	--
Q1 – Expected profitability	--	--	0.04 (0.18)	0.04 (0.19)
Q2 – Fairness perceptions	0.37* (0.195)	0.40** (0.188)	0.38* (0.196)	0.40** (0.197)
Q3 – Risk perceptions	-0.32** (0.155)	-0.31** (0.153)	-0.31* (0.167)	-0.31* (0.168)
Q4 – Fun	0.19 (0.136)	0.19 (0.136)	0.19 (0.15)	0.20 (0.15)
σ_C	0.05 (0.03)	0.05 (0.03)	0.05 (0.03)	0.05 (0.03)
Symmetric Cost Dummy	-0.11 (0.42)	--	-0.19 (0.40)	--
Asymmetric Cost - High Cost Dummy	--	0.07 (0.46)	--	0.09 (0.46)
Constant	-1.73 (1.27)	-1.91* (1.09)	-1.82 (1.43)	-2.05 (1.32)
F-statistic	3.70**	3.68**	3.66**	3.62**
R-square	0.24	0.24	0.24	0.23

Note 1: Standard errors are contained in the parentheses.
Note 2: Significant at the 5% level **
Note 3: Significant at the 10% level *

Appendix A

Exit Questionnaire

Chair #: ______________________

- In Game A your payment depended on how your score in points compared to your pair member.
- In Game B your payment depended on how your score compared to a fixed standard.

	Game A		About the Same		Game B
1. If each game were played many times under the same conditions as today, which one would earn you more money?	1	2	3	4	5
2. Which game did you think was more fair to all the participants involved?	1	2	3	4	5
3. Which game did you think was more risky?	1	2	3	4	5
4. Which game did you think was more fun to play?	1	2	3	4	5