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## **An Experimental Investigation of Alternative Incentive Schemes with Heterogeneous Agents**

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**Abstract:** Experimental economics is used to compare tournaments (T) and fixed performance contracts (F) when agents have heterogeneous costs. Results suggest that: (1) There is no difference in average pooled effort across contracts, (2) high ability agents exert higher effort than low ability agents under both types of contracts, (3) average pooled earnings are affected by contract type, (4) high ability agents benefit from T whereas low ability agents are harmed by T, and (5) the *difference* in average pay between high and low ability agents is larger under T. Thus, T implement greater inequality.

**Keywords:** tournaments, agricultural contracts, experiments, contract regulation, common shocks, fixed performance contracts.

**JEL Classification:** D01, D81, D82



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# **An Experimental Investigation of Alternative Incentive Schemes with Heterogeneous Agents**

In recent years, policy makers and farmers have questioned the fairness of agricultural contracts. This has led to legislative proposals, such as the Producer Protection Act of 2000 (Iowa Attorney General's Office), which contains a clause forbidding "unfair practices" such as the use of tournaments, which determine compensation through a rank ordering of growers. In response, economists have recently begun to empirically investigate the efficiency and welfare effects of tournaments compared to possible alternatives such as fixed performance standards contracts, which determine compensation by requiring growers to meet absolute rather than relative performance criteria. For example, Levy and Vukina use field data on chicken broiler contracts and suggest that grower welfare may be higher under tournaments than fixed performance standard contracts given the risk environment commonly found in this industry. Wu and Roe (in press) use an experimental approach and find that agents tend to exert higher effort under fixed performance standard contracts than tournaments. Moreover, they find that agents are generally better off under fixed performance standard contracts, except when common shocks are large.

Like Wu and Roe's (WR) study, this paper analyzes experimental evidence about the behavioral and earnings effects of tournaments (T) and fixed performance (F) contracts. However, this study adds an additional dimension of realism to WR's study by allowing agents to have asymmetric effort cost functions. In particular, the tournaments pair high ability (low cost) agents with low ability (high cost) agents. The subjects also participate in a fixed performance standard experiment and face the same production decision and risk environment as they do under the tournaments. Comparison of subject decisions across the two types of contracts provides a clean test of whether effort and earnings are higher under tournaments. The

risk environment (e.g., the percent of total production risk attributable to common versus idiosyncratic shocks) was altered across three different experimental sessions in order to provide a between-subject test of the interaction between risk environment and contract type on subject effort and earnings. Our primary findings are:

1. There is no statistical difference in average pooled effort (effort averaged across both high and low ability agents) under T and F contracts. Moreover, the *difference* in average effort levels exerted by high and low ability agents is unaffected by the type of contract. Thus, a ban on tournaments may not impact agent performance.
2. On average, effort exerted by high ability agents is considerably higher than effort exerted by low ability agents under both contracts.
3. Average pooled payoffs are affected by the type of contract used. In addition, high ability agents generally benefit from T whereas low ability agents are harmed by T. Low ability agents are not negatively impacted by T only when common shocks dominate.
4. The *difference* in average pay between high and low ability agents is larger under T than F. Thus, there appears to be greater inequality under tournaments.

Overall, our results suggest that a ban on tournaments may not harm processors' ability to manage performance and can be beneficial to low ability agents by reducing inequality.

However, when common shocks are extremely important, it is unlikely that a ban would help low ability growers and would hurt high ability growers which may possibly reduce aggregate profits. These findings are important as they allow researchers and policy makers to understand the performance, efficiency and distributional consequences of a potential ban on tournaments.

Before proceeding further, we comment on the relevance of experimental economics for understanding real world contracting issues. Noussair and Plott argue that experiments need not

replicate field situations and all institutional details to retain relevance for policy analysis. 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 must 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 base conclusions on models that employ 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.

While our experimental subjects are university students rather than farmers, we believe that the results reveal useful insights about human behavior under tournaments and fixed performance standards contracts. Moreover, for this particular study, we regard the use of students as a strength and not a weakness because growers' attitudes toward tournaments may be politicized by recent discussions about the "oppressive" nature of tournament contracts while most university students are unfamiliar with the politics of contract legislation.

### **Overview of the Experiments**

Prior to describing our theory and providing the details of our experiments, we will provide an overview of our experiments. We conducted three experiments using undergraduate students at a major university in the Midwest. For each experiment, we recruited twelve subjects via posters and email lists across a number of departments on campus. These subjects arrived in a room and were randomly assigned to twelve chairs. For T sessions, subjects were randomly matched together to form pairs, but subjects were not informed of the identity of the other pair member. Each subject was informed about whether her effort cost function is "high" or "low". If a subject

is assigned a low effort cost function, then her pair member was assigned a high effort cost function so that an advantaged agent (low cost) was always paired with a disadvantaged agent (high cost). Every subject was informed about her cost function as well as her pair member's cost function so that all cost functions were common knowledge.

The rest of the procedures for the experiments followed those of WR. Every subject was told that she can earn money by making good decisions during the experiments. Each experiment contained four sessions of ten rounds each, where the first session of the night was a T-session, followed by an F-session. After the first two sessions were completed, we conducted another T and another F session. However, subjects did not gain automatic entry into the second half sessions; instead, they had to bid their way into these sessions through an auction using their experimental earnings from the first two sessions. The sum of a subject's bids could not exceed that subject's accumulated earnings from the initial sessions. The ten highest bidders for the T session got to participate in the post-auction tournament. Similarly, the ten highest bidders for the F session got to participate in the additional F session. To maximize subject take-home pay, we required that the ten highest bidders pay only the amount offered by the tenth place bidder.<sup>1</sup>

The auction helped us to eliminate unmotivated subjects as such subjects are less likely to bid high enough to gain entry. Unmotivated subjects may contaminate results with poor strategic decisions. Repeated sessions also helped to moderate learning effects.<sup>2</sup> By the time subjects reached the second half sessions, they had become "experienced" subjects, i.e., learning effects within these second half sessions should be minimal. Thus, we expect the post-auction results to be relatively free of learning effects and outliers caused by unmotivated subjects.

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<sup>1</sup>Lusk, Feldkamp, and Schroeder have shown that, in general, elicited valuations are quite sensitive to choice of auction mechanism used. Our auction design should, in principle, induce subjects to shade their bids below their true valuation. This design served our purpose of eliminating outlier subjects while not inducing subjects to give up too much of their earnings.

<sup>2</sup> See Friedman and Sunder (pages 98-99) for a discussion of learning effects.

Once all four sessions were completed, subjects filled out an exit questionnaire and were paid in cash for their performance for the evening. In the next section, we discuss the underlying theory motivating our experiments and the experimental parameters employed.

## Theory and Experimental Parameters

### *Tournaments*

The underlying theory motivating our experiments has been discussed in other papers on experimental tournaments (e.g. Bull, Schotter and Weigelt (BSW); Schotter and Weigelt (SW); Wu and Roe (in press & 2003), and is motivated by the work of Lazear and Rosen. We provide a brief discussion of the theory in this section.

Consider a two-player tournament where each subject chooses a costly, non-contractible effort denoted by,  $e_i$ , for  $i = 1, 2$ . Performance for subject  $i$  is stochastically related to effort via the production function:

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

where  $y_i$  is performance,  $u_C$  is a common shock, and  $u_i$  is an idiosyncratic shock that is independently and identically distributed across agents. Random variables are distributed as follows:  $u_C \square N(0, \mathbf{s}_C^2)$ ,  $u_i \square N(0, \mathbf{s}^2)$ ,  $Cov(u_C, u_i) = 0$ , and  $Cov(u_i, u_j) = 0, \forall i \neq j$ .

The tournament compensation rule is simple: if  $y_i > y_j$ , then player  $i$  receives a high payment denoted by  $R$  and player  $j$  receives a low payment,  $r$ , where  $R > r$ , and vice versa.<sup>3</sup> Moreover, both agents face effort-cost functions satisfying:  $c(0) = 0$ ,  $c'(e_i) > 0$  and  $c''(e_i) > 0$ .

We also adopt the cost structure used by SW in their experiments, which is of the form,

$$(2) \quad c_i(e_i) = \frac{\mathbf{a}_i e_i^2}{k}$$

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<sup>3</sup> In the case of a tie, the high cost agent was declared the winner.

where  $k > 0$ . SW allowed for cost (ability) heterogeneity by letting  $\mathbf{a}_i$  vary across agents. We let  $\mathbf{a}_i = 1.5$  for half the agents and  $\mathbf{a}_j = 1$  for the other half; a high-cost agent is always randomly matched with a low-cost agent.

Under T, the probability of agent  $i$  receiving the high payment is  $\text{Prob}(u_i - u_j > e_j - e_i)$  where  $u_i - u_j \sim N(0, 2\mathbf{s}^2)$ . When common shock variance is the majority of total variance, then  $\mathbf{s}_c^2 + \mathbf{s}^2 > 2\mathbf{s}^2$  and the *total* variance is lower under T than F because  $\mathbf{s}_c^2$  is eliminated under T in exchange for the smaller  $\mathbf{s}^2$  under F. If we let  $\text{Prob}(u_i - u_j > e_j - e_i) = 1 - F(e_j - e_i)$  where  $F(\bullet)$  is the CDF of  $u_i - u_j$ , agent  $i$ 's objective function becomes,

$$(3) \quad E(\mathbf{p}_i^T) = [1 - F(e_j - e_i)]R + F(e_j - e_i)r - \frac{\mathbf{a}_i e_i^2}{k}$$

which, after some algebra, can be written as:

$$(4) \quad E(\mathbf{p}_i^T) = r + [1 - F(e_j - e_i)][R - r] - \frac{\mathbf{a}_i e_i^2}{k}$$

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

$$(5) \quad E(\mathbf{p}_j^T) = r + F(e_j - e_i)[R - r] - \frac{\mathbf{a}_j e_j^2}{k}$$

To be consistent with previous experimental studies, we assume risk-neutral agents. However, later we will discuss how predictions might change under risk aversion.

The two agents essentially play a game where effort choices are strategies and payoffs are given by (4) and (5). First-order conditions are,

$$(6) \quad \frac{\partial E(\mathbf{p}_i^T)}{\partial e_i} = f(e_j - e_i)[R - r] - \frac{2\mathbf{a}_i e_i}{k} = 0$$

$$(7) \quad \frac{\partial E(\mathbf{p}_j^T)}{\partial e_j} = f(e_j - e_i)[R - r] - \frac{2\mathbf{a}_j e_j}{k} = 0$$

where  $f(\bullet)$  is the density function. If cost functions were identical ( $\mathbf{a}_i = \mathbf{a}_j = 1$ ) as in WR's study, the solution is straightforward. Conditions (6) and (7) suggest that,

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

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  $f(0) = \frac{1}{\sqrt{2p(2s^2)}}$  giving us,

$$(9) \quad e_i = e_j = e^* = \frac{k[R - r]}{2\sqrt{4ps^2}}$$

However, our study features equilibrium effort levels that differ between high and low cost

agents; hence, the normal distribution is not evaluated at  $f(0) = \frac{1}{\sqrt{2p(2s^2)}}$ . Numerical

solutions to the first-order conditions for these equilibrium effort levels are reported in Table 1.

Details of the other parameters in the table will be discussed shortly. Like previous studies, we restrict subjects' effort choices to be an integer from zero to 100<sup>4</sup> and chose parameters to ensure interior solutions.

#### *The Fixed Performance Standard Contract*

Under fixed performance contracts, agent  $i$  receives the high payoff  $R$  if output exceeds a fixed standard  $y^*$  and  $r$  otherwise. The probability that agent  $i$  receives the high payoff is  $\text{Prob}(y_i > y^*)$

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<sup>4</sup>WR 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.

=  $\text{Prob}(u_C + u_i > y^* - e_i)$ , where  $u_C + u_i \sim N(0, \mathbf{s}_C^2 + \mathbf{s}^2)$ . Letting  $G(\bullet)$  be the CDF of  $u_C + u_i$ , we have  $\text{Prob}(u_C + u_i > y^* - e_i) = 1 - G(y^* - e_i)$ . Agent  $i$ 's objective function is,

$$(10) \quad E(\mathbf{p}_i^F) = r + [1 - G(y^* - e_i)][R - r] - \frac{\mathbf{a}_i e_i^2}{k}$$

with a first-order condition of

$$(11) \quad \frac{\partial E(\mathbf{p}_i^F)}{\partial e_i} = g(y^* - e_i)[R - r] - \frac{2\mathbf{a}_i e_i}{k} = 0$$

Because  $g(\bullet)$  is a normal density function, solving (11) for  $e_i$  is complicated. Numerical solutions used as benchmarks for our experiments are listed in Table 2.

To explain some of the numbers in Tables 1 and 2, we briefly discuss our process of choosing experimental parameters. One objective of parameter choice was to maintain consistency with WR's study so that the impact of cost heterogeneity could be isolated. Therefore, like WR, the parameter  $k$  in the effort cost function was chosen to be 10,000. Another objective was to explore how alteration of common shock variance affects behavior. Total variance (sum of the variances for the common and idiosyncratic shocks) is held constant at 500 while the relative size of the variance of the common shock changed across experiments.

In choosing the payments  $R$  and  $r$  for the tournament, we followed the procedures of WR, modified only to adjust for asymmetric effort cost functions. 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  $r$  is used to determine expected payoffs for the agents.<sup>5</sup> The average effort level we target is  $e^* = 37$  as this did not appear to be an obvious number that subjects spuriously might choose, thereby biasing the result. We

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<sup>5</sup> In practice, processors would have a similar consideration when designing contracts because they must choose a base payment to ensure that growers are guaranteed a level of expected payoffs that exceed their reservation utilities.

therefore had to choose the spread  $R - r$  to implement an effort level of 37, on average. Because the effort cost functions differed across agents within a pair, high and low cost agents 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 payoffs for agents. For each experiment, we chose  $r$  to ensure that high cost agents receive an expected payoff of \$14.20 and low cost agents receive expected payoffs of \$23.50 for an average of \$18.90. This is about the going rate for a two-hour student experiment on the host campus. Each experiment consisted of four ten-round sessions for a total of forty rounds of play. Thus, the per-round expected payoffs for the high cost and low cost agents were \$0.355 and \$0.588, respectively, for an average of about \$0.473.

Once the targeted effort levels and expected payoffs were identified, we pin down optimal values of  $R$  and  $r$ . For example, with a target average 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.  $\sigma^2 = 250$ ), the optimal payment spread is  $R - r = 0.62$ . In principle, this spread should induce high cost subjects to choose  $e = 30$  and low cost subjects to choose  $e = 44$  for an average of  $e = 37$ . To pin down  $r$ , numeric simulations showed that  $r = 0.33$  would provide high and low cost agents with per round expected payoffs of approximately \$0.355 and \$0.588, respectively.<sup>6</sup> 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, were generated in a similar fashion.

In calibrating parameters for the fixed performance contract, we wanted the average effort level to be 37 as under the tournaments. This allowed us to study how effective fixed

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<sup>6</sup> We say “approximately” because our numerical calculations had minor rounding errors. For example, effort for a high cost agent was actually 29.98 for an idiosyncratic variance of 250 and a pay spread of 0.62. The expected payoffs were also slightly different from \$0.355 due to minor approximation errors but the payoff did not deviate by more than 0.001.

standard contracts were relative to tournaments in achieving the same performance objectives. Second, we wanted to maintain the same expected payoffs for the agents so that, on average, risk neutral agents should earn the same amount both contracts.

Note that the incentive compatibility constraint for the agent is given by (11) so that if we wanted an average effort level of  $e = 37$ , we can solve for the payment spread that would induce this average effort level.<sup>7</sup> 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. Our numeric simulations yielded an optimal wage spread of  $R - r = 0.55$ . While this spread produced an *average* of  $e = 37$ , we note that the high cost agents optimal effort is  $e = 27$  while the optimal low cost effort is  $e = 47$ . Hence the optimal effort gap in T sessions is “14” (44 minus 30) whereas it is “20” in F sessions (47 minus 27). This may provide some intuition about why processors may prefer tournaments in practice because, with heterogeneous agents, the effort gap between high ability and low ability subjects is greater under F contracts than under tournaments. Thus, performance may be less consistent under tournaments. The experimental data can be used to test these theoretical predictions. Additionally, the value of  $r$  that would result in an average expected payoff of \$0.473 per round to satisfy the “participation constraint” was  $r = 0.40$ . However, while this value of  $r$  is consistent with an expected per round payoff of .473 when we pool all the agents, the expected per round payoffs for the high and low cost agents are \$0.437 and \$0.51, respectively. Hence, the gap in expected payoffs between high and low cost

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<sup>7</sup> We also evaluated the second order conditions to ensure that we are at a maximum.

agents is lower under F than under T so that we might expect greater inequality under T.

Parameters for the F sessions are presented in Table 2.

### **Additional Details of the 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.<sup>8</sup> Distributions for other values of  $\mathbf{s}_C^2$  and  $\mathbf{s}^2$  (i.e. for experiments 2 and 3) were approximated using the same method. For T sessions, if  $y_i > y_j$ , then agent  $i$  gets  $R = 0.95$  and agent  $j$  gets  $r = 0.33$ , and if  $y_i < y_j$ , then agent  $i$  gets  $r = 0.33$  and agent  $j$  gets  $0.95$ . This rule applied for experiments 2 and 3 as well, except that the numeric values of  $R$  and  $r$  were different. Each low cost subject was matched with a high cost subject in a tournament round and both players were informed about both her cost function and her opponent's cost function. The F sessions were similar to the T sessions, except that each subject played against a fixed standard of  $y^* = 41$ , rather than against a pair member. These experimental procedures were essentially identical to the procedures of WR, except for the matching of a high ability subject with a low ability subject.

In each T and F session, subjects play ten identical rounds of the game. In each round subjects choose "decision numbers" (effort) from 0 to 100 and enter the decision numbers into

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<sup>8</sup> 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.

individually maintained worksheets as an experimenter records the decisions in a computer. Then one subject draws a “common shock” number from a bucket with frequencies that approximated a normal distribution and all subjects add this number to their decision numbers. Next, each subject draws a number from another bucket with frequencies approximating another normal distribution, and then this individual number was added to the 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. The sum of the decision number, the common shock, and the idiosyncratic shock is “performance” ( $y_i$ ).

In T sessions, the administrator would compare outputs of pair members and inform all subjects of the relevant payment received for the round ( $R$  or  $r$ ). For F sessions, output is compared to the fixed standard of 41. Each subject is informed only his/her own payment and not the difference in output.<sup>9</sup> Each subject records the payment in the worksheet and subtracts the decision cost to get net earnings for that round. This pattern is identically repeated in all 10 rounds. At the end of the tenth round, subjects calculated cumulative payoffs for the ten rounds and confirmed these figures with the computer maintained records kept by the administrator.<sup>10</sup>

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; two non-paying,

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<sup>9</sup> 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.

<sup>10</sup> 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).

practice rounds were played before each session to ensure that subjects understood the experiment. Complete instructions for the experiments are available upon request.

A potential criticism of our experimental design is that the order of our sessions is not counterbalanced; that is, the tournament is always conducted first and could give rise to order effects. This was done to minimize the potential for subjects to use the fixed standard of 41 as a focal point for choosing strategies in a tournament setting. WR discuss this issue more fully and reference some control experiments in which subjects only play F sessions. These control experiments did not involve tournaments so the F session results were free of “background context” from T sessions.<sup>11</sup> The F control results did not differ enough from the F sessions that were preceded by T sessions to suggest that the fundamental conclusions would be different had the order of T and F sessions been counterbalanced.

## **Hypotheses**

We focus on four hypotheses that allow us to infer the potential performance and earnings consequences of imposing different contracts and, in the process, understand the general economic forces that shape the controversial issues surrounding the use of tournament contracts in agriculture. Specifically, the following hypotheses are proposed:

***Hypothesis 1:*** *Pooled effort (low cost and high cost agents combined) will not differ across F and T contracts.*

***Hypothesis 2:*** *The difference in effort (effort gap) exerted by low- and high-cost agents is the same under F and T contracts.*

***Hypothesis 3:*** *Pooled net pay per round will not differ across F and T contracts.*

***Hypothesis 4:*** *The difference in expected net pay (pay gap) between low cost agents and high cost agents is the same under F and T contracts.*

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<sup>11</sup> See Tversky and Simonson for a discussion of background context.

Hypothesis 1 is derived from the fact that we chose our experimental parameters in order to implement an average pooled effort level of  $e = 37$ . The practical implication of this hypothesis might be that, because many real world processors do not offer separating contracts and often use a single contract for all growers, regardless of type, it may shed light on how average performance might be affected by a ban on tournaments. If this hypothesis cannot be rejected, then it might suggest that processors only interested in implementing some average level of performance would not be significantly affected by a ban on tournaments.

Testing hypothesis 2 can provide insights into how well processors' can maintain performance consistency when growers are heterogeneous. Inconsistent performance would harm processors' operations by creating greater performance variability which may affect processing costs and quality consistency. Recall that the numerical simulations predict that the low cost agents should exert higher effort under F relative to T. That is, given the parameters chosen to implement a pooled  $e = 37$ , risk-neutral low-cost agents are expected to optimize by choosing an effort level of 47 under F and 44 under T contracts. Risk-neutral high-cost agents who optimize should choose an effort level of 30 under T and 27 under F. If these predictions are confirmed by the data, it would suggest that a ban on T may lead to a greater performance gap between low and high cost agents.

Hypothesis 3 follows from the fact that we chose  $r$  across all of our experiments to provide our subjects with a pooled expected payment of \$0.473 per round. Under the null hypothesis, a ban on tournaments would neither harm nor help growers at the *aggregate* level, which would be consistent with our simulations.

Hypothesis 4 allows us to explore grower earnings when disaggregating by cost type. Our simulations suggest that net pay per round for low cost agents would be higher under T rather

than under F so that a ban on tournaments would *harm* high ability growers. However, low ability growers (high cost) would benefit from a ban. This implies that the average net pay gap between low and high cost growers would be *higher* under T contracts so there would be greater inequality under T. Thus, if the predictions made by our simulations are confirmed by data, then we would have evidence to suggest that, even if a ban would not affect welfare at the aggregate level, there will be winners and losers at the disaggregate level. Hypothesis 4 serves as the null hypothesis to the alternative hypothesis that our numerical simulations are correct.

## **Results**

### *Effort Levels*

We begin by examining summary statistics for pooled effort levels in Table 3. Note that we partition the data into pre- and post-auction sessions for both types of contracts so that we can get a glimpse of how learning effects and the presence of unmotivated subjects may have affected the results. We consider post-auction data to be more reliable as learning effects should be moderated and some unmotivated subjects should have been eliminated by the auction.

When we examine the combined data from all three experiments (row 1), we find average effort under T is 46.7 in pre-auction sessions and a remarkable 37.5 in post auction sessions, which is nearly identical to simulated levels. This suggests that learning effects and unmotivated outlier subjects may have affected results in pre-auction sessions. Consequently, whenever pre- and post-auction results conflict, we will give preference to post-auction results.

The remaining rows of Table 3 partition the data across experiments with different sized common shock standard deviations, though, as discussed earlier, contract parameters are adjusted to ensure the same optimal effort and payoffs across experiments. Thus, we should observe the same average effort across experiments. However, one can see that there was significant

variation in average effort across experiments. Focusing on post-auction sessions, we see a low of 29.09 ( $s_c \approx 18.7$ ) to a high of 46.2 ( $s_c \approx 15.8$ ).

Pooled average effort under F was slightly above the simulated optimal effort of 37. For pre-auction data, the average was 43.2 and for post-auction data, the average was 41.8. While average effort did vary across experiments, the variation was not great in the post-auction data, ranging from a low of 39.8 ( $s_c \approx 18.7$ ) to a high of 44 ( $s_c \approx 15.8$ ). The reduced variation in effort under F is not surprising because *total variance* did not change across F experiments.

Tables 4 lists summary statistics for low cost agents only and Table 5 presents the same for high cost agents. Numerical simulations (Tables 1 and 2) predict that low cost agents will exert slightly higher effort under F relative to T while high cost agents would exert slightly higher effort under T relative to F. Focusing our attention on post-auction data, we see that these predictions are partially fulfilled: *both* low cost and high cost agents appear to exert slightly higher effort under F, except in the  $s_c = 15.8$  experiment.

Because subjects played multiple rounds in each session, we had many repeated observations by subject. Hence, across-round observations will not be a part of a random sample, but it does allow us to use panel methods to control for unobservable heterogeneity, such as rate of learning, risk tolerances, among other factors, which may affect subjects' effort choice. We ran a random effects regression where the dependent variable is effort, and the explanatory variables were a tournament dummy (T), which equals "1" whenever data are generated under a tournament and "0" otherwise; a variable for the standard deviation of the common shock ( $\sigma_C$ ) which can take three values, 12.2, 15.8, and 18.7; an interaction term for the tournament dummy and  $\sigma_C$ ; a low cost dummy, which equals "1" whenever data are generated by a low cost agent

and “0” otherwise; and an interaction term for the tournament dummy and the low cost dummy.<sup>12</sup> Round-specific dummy variables were also included.

Table 6 contains the results for the effort random effects regressions. Hypothesis 1 predicts that pooled effort will not differ across the two types of contracts given the way we have chosen our parameters. Under the null, this would imply that a ban on tournaments should have no impact on a processor’s ability to implement desired performance. In order to test Hypothesis 1, note that under the null, the conditional mean of effort should be identical under T and F so that a formal test involves examining the joint significance of the tournament coefficient and any interaction term involving the tournament dummy. Table 6 reports that the Chi-square(3) statistics for this test are 2.27 and 1.52 for pre and post-auction data, respectively. We therefore cannot reject the null that the tournament dummy does not affect the conditional mean of effort. This suggests that average performance will not be impacted by the choice of contract.

While the failure to reject hypothesis 1 may suggest that processors might be indifferent to T and F, hypothesis 2 allows us to assess things in more detail by looking at the effort gap between the two types of agents. Hypothesis 2 suggests that the *difference* in average effort levels exerted by low and high cost agents should not be affected by contract type. The alternative hypothesis is that the effort gap will be larger under F, which comes from our numeric simulations. To understand how we might test hypothesis 2, note from Tables 1 and 2 that low cost agents are predicted to exert higher effort under both contracts. Also, under the alternative hypothesis, the predicted effort gap between high and low cost subjects is smaller under T contracts ( $44-30 < 47 - 27$ ). Hence, we expect the “low cost” dummy variable to be

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<sup>12</sup> We also conducted a Hausman test to compare fixed vs. random effects. The Hausman test yielded a Chi-square(12) test statistic of 7.88 with a p-value of 0.79 using pre-auction data, and a Chi-square(12) test statistics of 10.68 with a p-value of 0.56 using post-auction data. These results suggest that we could not reject the null of no systematic difference in coefficients. We therefore felt justified in using the random effects approach.

positive and the sum of the low cost dummy coefficient and the low cost $\times$ T interaction coefficient to be positive. However, under the alternative hypothesis, we expect the low cost $\times$ T interaction coefficient to be negative and significant.

The regression results reported in Table 6 suggests that we cannot reject hypothesis 2 because the low cost $\times$ T interaction coefficients for both the pre-auction and post-auction data are not significantly different from zero. We also estimated the sum of the low cost coefficient and the low cost $\times$ T interaction coefficient and found this sum to be positive and significantly different from zero for both the pre- and post-auction data. Hence, we see clearly that the data supports the prediction that low cost agents exert higher effort. Focusing on the post-auction data, which we deem more reliable, we see that this gap is, on average, 16.73, which is similar to our simulated predictions. That is, the predicted gap under F is 20 and the predicted gap under T is 14 so our estimated coefficient of 16.73 falls somewhere between these two predictions.

### *Agent Profits*

An important issue related to a tournament ban is how agent (grower) profits will be affected.

Our results can provide insights into how agent profits are affected by contract type.

Table 7 reports summary statistics of net pay per-round partitioned by pre- and post-auction results. Recall that our numerical simulations predicted that the pooled net pay (average net pay across both types of agents) would be \$0.473 under both F and T contracts. Examining row 1 of table 7, which reports average net pay across all three experiments, we see that average per-round net pay ranges from \$0.32 (pre-auction T) to \$0.44 (pre and post-auction F). Thus, the empirical results suggest that subjects earn less, on average, than what is predicted. Examination of the same data for the individual experiments reveals substantial variation in average net pay across experiments. For instance, in the pre-auction data for the experiment where  $s = 15.8$ , our

subjects earned only an average of \$0.22 per round under T contracts. In contrast, subjects earned an average of \$0.49 per-round in the post-auction F session.

Table 8 contains summary statistics for low cost subjects only. Recall that our simulations predict that low cost subjects would earn \$0.588 per round under T contracts and \$0.51 per round under F. We see that, for the most part, low cost subjects earned more under T contracts in the post-auction data but more under F in the pre-auction data. Because we consider the post-auction data to be more reliable, we give preference to these results whenever there is a conflict. The overall post-auction results (row 1) suggest that low cost subjects did indeed earn more under T contracts than F as predicted by the simulations. The difference in earnings ( $\$0.54 - \$0.46 = \$0.08$ ) was very close to the difference predicted in the simulations ( $\$0.588 - \$0.51 = \$0.078$ ). However, in absolute terms, the average earnings of the subjects fell short of the predicted earnings by about \$0.05 for both types of contracts.

Table 9 presents summary statistics for the high cost subjects only. Our simulations had predicted that high cost subjects would earn \$0.355 per round under T contracts and \$0.437 under F. Again, subjects seem to earn less than what was predicted as the overall post-auction earnings (row 1) were \$0.29 and \$0.41 for the T and F contracts, respectively. The difference in earnings ( $\$0.41 - \$0.29 = \$0.12$ ) appears to be slightly larger than the predicted difference ( $\$0.437 - \$0.355 = \$0.082$ ). Nonetheless, the empirical results are close enough to the predictions of the simulations that we feel that the theory is adequate at explaining behavior.

We now turn to a formal test of hypotheses 3 and 4. We first examine hypothesis 3 which essentially predicts that pooled average net pay will not be affected by contract type. This implies that the conditional mean of pooled net pay should be identical under T and F; that is, the tournament dummy and all interactions involving this dummy should have no significant impact

on pooled net pay. Table 10 reports the relevant regression results. The Chi-square(3) test statistics for this test are 44.22 and 23.05 for the pre- and post-auction data, respectively; we reject the null hypothesis that tournaments have no impact on the conditional mean of net pay. This suggests that a ban may potentially affect aggregate grower profits.

By looking at the incremental impact of tournaments, which involves examining the sign and significance of the linear combinations of all coefficients involving the tournament dummy, the direction of the effect on grower profits can be determined. To do this, let  $b_T$  denote the coefficient for the tournament dummy,  $b_{TC}$  be the coefficient for the interaction term between the tournament dummy and  $\mathbf{s}_C$ , and  $b_{TLC}$  denote the coefficient for the interaction term between the tournament dummy and the low cost dummy. The incremental impact of tournaments on net pay is  $b_T + b_{TC}\mathbf{s}_C + b_{TLC}lowcost$  where *lowcost* is the dummy variable signifying a low cost agent. Concentrating on post-auction data, we report the various incremental impacts of the tournament dummy evaluated at different levels of  $\mathbf{s}_C$  and for both low- and high-cost subjects (Table 11).

Tournaments have a different impact on high- and low-cost growers. The incremental impact on low-cost subjects is positive across all three values of  $\mathbf{s}_C$  and significantly different from zero at  $\mathbf{s}_C = 15.8$  and  $\mathbf{s}_C = 18.7$ . Also, the magnitude of the estimated incremental impact increases with  $\mathbf{s}_C$  suggesting that the beneficial effects of tournaments to low cost agents increase as the common shock becomes more important.

In contrast, the incremental impact of T on high cost agents appears to be negative across all values of  $\mathbf{s}_C$  and significantly different from zero at  $\mathbf{s}_C = 12.2$  and  $\mathbf{s}_C = 15.8$ . Notice, however, that as  $\mathbf{s}_C$  increases, tournaments appear to be less harmful to high cost agents so that tournaments appear to be most justified in environments where common risk is very important.

Our results suggest that a ban on tournaments may (a) hurt low cost growers, (b) benefit high cost growers, (c) and reduce aggregate profits for all growers when common risk is dominant. Point (c) is supported by the fact that, in the experiment where  $s_c$  is largest ( $s_c = 18.7$ ), the incremental effect of T is large and positive (0.147 per round) for low cost growers and not significantly different from zero for high cost growers. A ban in this situation would not impact high cost growers but would reduce profits of low cost growers. In contrast, when  $s_c = 12.2$ , a ban could possibly improve aggregate grower profits because it would have little impact on low cost growers and eliminate the negative effects of T on high cost growers.

The final hypothesis to be tested is hypothesis 4, which states that the net pay gap between low- and high-cost subjects is identical under T and F contracts. The alternative hypothesis is that the net pay gap is greater under T contracts, which comes from our simulations. The simulations predict that the gap is only \$0.074 (\$0.51-\$0.437) under F and \$0.233 (\$0.588-\$0.355) under T contracts. Because the low cost agents are predicted to earn more under both types of contracts, we expect the low cost dummy coefficient to be positive and significant. Also, under the null hypothesis, the pay gap should be the same under T and F so we expect the low cost  $\times$  T interaction coefficient to not differ significantly from zero. However, under the alternative hypothesis, this coefficient should be positive and significantly different from zero. The results reported in Table 10 show that this coefficient is positive (0.19) and significantly different from zero at the 5 percent level in the post-auction data. Thus, hypothesis 4 is rejected. Indeed, given that the coefficient is positive in both the pre- and post-auction data, the pay gap increases under tournaments suggesting that tournaments implement greater welfare inequality so that high ability growers might benefit at the expense of low ability growers.

With regard to the magnitudes of our estimates, the coefficient for the low cost dummy in the post auction data is 0.07 which is the difference in pay between high and low cost agents under F. This is remarkably close to the predicted gap of 0.074. The estimated gap under T contracts is simply the sum of the low cost coefficient and the lowcost  $\times$  T interaction coefficient, which is estimated to be 0.26 and significantly different from zero. This is also fairly close to the predicted gap of 0.233. Consequently, our numeric model does a remarkable job of predicting actual earnings outcomes.

### **Impact of Risk Aversion**

Our experimental results suggest that, (1) the type of contract does not affect pooled effort level; (2) the type of contract does not affect the effort gap between low and high cost subjects; (3) the type of contract does affect pooled net earnings; (4) the incremental impact of T contracts on net pay appears to positively increase with  $s_c$ ; and (5) contract type does affect the net pay gap between low and high cost subjects. While these results are interesting, observations (2), (3), and (4) do not seem to be consistent with the predictions of our risk neutral simulation model outlined earlier. We now discuss whether risk aversion can explain the divergence between these empirical regularities and the theoretical predictions.

We originally chose to calibrate our model parameters under the assumption of risk neutrality because any risk preferences we would have imposed on our subjects would have been arbitrary. Thus, the assumption of risk neutrality provides a reasonable benchmark that imposes a common assumption concerning subjects' utility functions. Alternatively, we could have induced risk preferences using techniques developed by Berg et al., but such procedures have questionable reliability. Thus, there is no easy way of dealing with risk aversion.

Given the unreliability of methods used to induce risk preferences, and the arbitrary nature of imposing risk preferences, we want to emphasize that the following risk simulations are primarily used to determine whether qualitative predictions changed from the case of risk neutrality. We were less concerned with specific numeric results because any method we used would have led to ad hoc quantitative results but qualitative results can remain relatively robust.

We used WR's approach to simulate whether the addition of risk aversion might generate predictions that are consistent with our data regularities. That is, we modify (3) and (10) to get:

$$(12) \quad E[U(\mathbf{p}_i)] = [1 - F(e_j - e_i)]u\left(R - \frac{\mathbf{a}_i e_i^2}{k}\right) + F(e_j - e_i)u\left(r - \frac{\mathbf{a}_i e_i^2}{k}\right)$$

$$(13) \quad E[U(\mathbf{p}_i)] = [1 - G(y^* - e_i)]u\left(R - \frac{\mathbf{a}_i e_i^2}{k}\right) + G(y^* - e_i)u\left(r - \frac{\mathbf{a}_i e_i^2}{k}\right)$$

where  $u'(\square) > 0$  and  $u''(\square) \leq 0$ . The T first order condition for players  $i$  is:

$$(14) \quad f(e_j - e_i) \left[ u\left(R - \frac{\mathbf{a}_i e_i^2}{k}\right) - u\left(r - \frac{\mathbf{a}_i e_i^2}{k}\right) \right] =$$

$$\left[ 1 - F(e_j - e_i) \right] 2u'\left(R - \frac{\mathbf{a}_i e_i^2}{k}\right) \frac{\mathbf{a}_i e_i}{k} + F(e_j - e_i) 2u'\left(r - \frac{\mathbf{a}_i e_i^2}{k}\right) \frac{\mathbf{a}_i e_i}{k}$$

$$\Leftrightarrow f(e_j - e_i) \left[ u\left(R - \frac{\mathbf{a}_i e_i^2}{k}\right) - u\left(r - \frac{\mathbf{a}_i e_i^2}{k}\right) \right] = \frac{2\mathbf{a}_i e_i}{k} E \left[ u'\left(m - \frac{\mathbf{a}_i e_i^2}{k}\right) \right]$$

where  $m = \begin{cases} R & \text{with Probability } 1 - F(e_i - e_j) \\ r & \text{with Probability } F(e_i - e_j) \end{cases}$

Similarly, for subject  $j$ , we have,

$$(15) \quad f(e_j - e_i) \left[ u\left(R - \frac{\mathbf{a}_j e_j^2}{k}\right) - u\left(r - \frac{\mathbf{a}_j e_j^2}{k}\right) \right] = \frac{2\mathbf{a}_j e_j}{k} E \left[ u'\left(m - \frac{\mathbf{a}_j e_j^2}{k}\right) \right].$$

Note that the left-hand side represents the marginal benefits from increasing effort and the right hand side represents the expected cost which is now a function of the expected marginal utility. Conducting comparative statics using the general forms (14) and (15) is difficult. For example, suppose that we want to know whether an increase in the common shock standard deviation will reduce effort in our experiments. First, note that the only variance that matters under T is the idiosyncratic risk,  $\sigma$ , because T filters out the common shock. Secondly, because we held total variance fixed at 500 in our experiments, an increase in  $\sigma_C$  implies a reduction in  $\sigma$  so that we can recast the statement “effort decreases with  $\sigma_C$ ” as “effort increases with  $\sigma$ .” Consequently, we can attempt to answer our question by understanding how effort might vary with  $\sigma$ .

Examining either (14) or (15), we can see that whenever there is a reduction in  $\sigma$  (or an increase in  $\mathbf{s}_C$ ), holding all else constant,  $f(e_j - e_i)$  increases because it is a normal distribution.

Moreover, examining the right hand side term, we see that a reduction in  $\sigma$  implies a decrease in expected cost so long as the agents exhibits DARA. This is because DARA implies that

$$u'''(\square) > 0 \text{ (Gollier, p. 25) so that } E \left[ u' \left( m - \frac{e_i^2}{k} \right) | \mathbf{s} \right] < E \left[ u' \left( m - \frac{e_i^2}{k} \right) | \mathbf{s}' \right] \text{ for } \mathbf{s} < \mathbf{s}'.$$

With an increase in expected benefit and a decrease in expected cost, we anticipate an increase in effort, all else being equal. However, before concluding that effort should necessarily increase with a reduction in  $\sigma$  across our experiments, we should remind the reader that we adjusted the spread  $R - r$  across our experiments in order to implement an effort level of 37 under risk neutrality. Note

$$\text{that with risk aversion, we know that } R - r \geq u \left( R - \frac{\mathbf{a}_i e_i^2}{k} \right) - u \left( r - \frac{\mathbf{a}_i e_i^2}{k} \right) \text{ for non-negative effort}$$

levels and weakly concave utility functions. Thus, it is unclear whether equilibrium effort will vary across our tournament experiments as we adjust  $\mathbf{s}$  and  $\mathbf{s}_C$ .

In order to get unambiguous predictions, we conducted simulations by postulating the same utility function and risk parameters as in the Holt and Laury experiments. That is, Holt and Laury chose the “flexible” expo-power utility function (Saha),

$$(16) \quad u(p_i) = q - \exp\{-bp^a\}$$

and determined parameters to be  $\beta = 0.029$  and  $\alpha = 0.731$ . The parameter  $\theta$  does not affect optimal effort levels so we arbitrarily set the value at  $\theta = 5$ . Our payments  $R$  and  $r$  are as before. We conducted simulations by replacing  $u(\square)$  with (16) and then numerically solved conditions (14) and (15). The results are reported in Table 12. We also report simulation results for F, which involved replacing  $u(\square)$  in (13) with (16) and solving for the optimal effort level.<sup>13</sup>

We also conducted a sensitivity analysis where we repeated the simulations by changing the parameters to  $\beta = 0.1$  and  $\alpha = 0.1$ . This increased the degree of risk aversion by more than doubling the coefficient of relative risk aversion. This had only a minor impact on the numerical results and it did not affect the qualitative results. Hence, we did not report these results, but they are available from the authors upon request.

Using the risk averse simulation results reported in Table 12, we note the following:

- The predicted effort gap between high and low cost subjects under risk neutrality was 14 under T contracts and 20 under F contracts. Thus, the *difference* in effort gap across the two contracts is 6. Under risk aversion, the predicted effort gap ranges between 15 and 16 under T, and is 22 under F. Thus, the *difference* in effort gap across the two contracts is between 6 and 7 (depending on  $s_c$ ). Since the difference in effort gap did not decline

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<sup>13</sup> Payments used in the simulations were scaled up to avoid payments  $< 1$  since we have an exponential utility function. However, relative payment sizes were not changed in order to maintain the same incentives, and expected earnings were converted back into per-round measures

under risk aversion, we are doubtful that observation (2) – the type of contract does not affect the difference in effort gap – can be explained by introducing risk aversion.

- Under risk neutrality, the expected pooled earnings per round was \$0.473 under both T and F. Under risk aversion, predicted pooled earnings differ across contracts so the introduction of risk aversion is consistent with observation (3) – the type of contract affects pooled net earnings.
- Under risk neutrality, the incremental impact of T (relative to F) on earnings is predicted to remain constant for both low and high cost subjects across  $s_c$ . Under risk aversion, the incremental impact is predicted to increase with  $s_c$  for low cost subjects but not for high cost subjects (although it does not remain constant). Observation (4) – the incremental impact of T on net pay positively increased with  $s_c$  - appears to be consistent with risk averse predictions for low cost subjects, but not consistent with risk averse predictions for high cost subjects.

The above points suggest that there is mixed evidence about whether risk aversion can explain the data patterns from the experiments. WR also found disparities between their data patterns and predictions made under risk aversion and raised the possibility that *ambiguity aversion*, as discussed by Viscusi and Chesson, might offer a plausible alternative to either risk neutrality or risk aversion. While ambiguity aversion is a potentially important topic, a full investigation would be beyond the scope of this paper and is left for future research.

### **Conclusion**

We discuss the results of an experimental study that investigated the behavioral and earnings effects of tournaments and fixed performance standard contracts. The experimental design is based on the study conducted by Wu and Roe (in press), but extends their study by allowing for

heterogeneous experimental subjects, which is captured by differences in effort-cost functions. This led to a number of additional insights that can be used to deepen our understanding of how a ban on tournaments can potentially impact performance, aggregate profits, and the distribution of profits across agents with different ability levels.

With regard to performance, we found that neither the pooled mean effort level, or the *difference* in effort levels between high and low ability subjects, would be affected by contract type. This suggests that a ban on tournaments would have minimal impact on a processor's ability to implement particular effort levels even in an environment with heterogeneous agents.

We also found that the use of tournaments and fixed performance standard contracts had significantly different impacts on agent profits. Specifically, we found that tournaments had a positive impact (relative to fixed performance standard contracts) on the earnings of high ability agents, but a negative effect on the earnings of low ability agents. This suggests that a ban on tournaments can have distributional consequences. We also found that there was greater inequality in earnings under tournaments so that a ban on tournaments may result in more equal earnings across heterogeneous grower pools. With respect to aggregate profits, our results suggest that a ban may only improve aggregate profits when common shocks are small relative to idiosyncratic shocks. When idiosyncratic shocks dominate, low ability agents are most hurt by tournaments whereas high ability agents are relatively unaffected by contract type. In this circumstance, a ban may have little impact on high ability growers but would increase the earnings of low ability growers. However, when common shocks are dominant, a ban might reduce aggregate profits as low ability growers might be relatively unaffected by tournaments whereas high ability growers could benefit greatly from tournaments.

Our results can shed light on why there appears to be political conflict between processors and growers over the use of tournaments. While tournaments may not necessarily reduce aggregate surplus, especially when common shocks are important, it does increase the earnings gap between agents with heterogeneous abilities suggesting greater inequality in the distribution of pay. This may provide insights into why policy makers and farm advocates have called for the banning of tournaments even if tournaments are, in principle, a legitimate economic device for managing aggregate risk.

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**Table 1: Experimental Parameters for the Tournament Sessions.**

	$e_i^* (e_j^*)$	$R$ $r$	<i>Expected earnings per round</i>	$s_c^2$	$s^2$	<i>Effort Cost</i>
Experiment 1	44(30)	\$0.95 \$0.33	High Cost: \$0.355 Low Cost: \$0.588	250	250	Asymmetric $a_i = 1$ $a_j = 1.5$
Experiment 2	44(30)	\$0.90 \$0.35	High Cost \$0.355 Low Cost: \$0.588	350	150	Asymmetric $a_i = 1$ $a_j = 1.5$
Experiment 3	44(30)	\$0.99 \$0.29	High Cost \$0.355 Low Cost \$0.588	150	350	Asymmetric $a_i = 1$ $a_j = 1.5$

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

	$e_i^*(e_j^*)$	$R$ $r$	<i>Expected earnings per round</i>	$s_c^2$	$s^2$	<i>Effort Cost</i>
Experiment 1	47(27)	\$0.95 \$0.40	High Cost: \$0.437 Low Cost \$0.51	250	250	Asymmetric $\mathbf{a}_i = 1$ $\mathbf{a}_j = 1.5$
Experiment 2	47(27)	\$0.95 \$0.40	High Cost: \$0.437 Low Cost: \$0.51	350	150	Asymmetric $\mathbf{a}_i = 1$ $\mathbf{a}_j = 1.5$
Experiment 3	47(27)	\$0.95 \$0.40	High Cost: \$0.437 Low Cost: \$0.51	150	350	Asymmetric $\mathbf{a}_i = 1$ $\mathbf{a}_j = 1.5$

**Table 3: Summary Statistics for Pooled Effort Levels – Overall Means (Standard Deviations)**

	<i>Tournament (T)</i>		<i>Fixed Performance Standard (F)</i>	
	Pre-Auction Data	Post Auction Data	Pre-Auction Data	Post Auction Data
1. From all experiments	46.7 (21.8)	37.5 (21.4)	43.2 (17.6)	41.8 (16.4)
2. From $\sigma_c=12.2$ experiment	39.9 (18.4)	37.2 (18.3)	39.3 (18.2)	41.5 (13.8)
3. From $\sigma_c = 15.8$ experiment	56.8 (16.8)	46.2 (20.1)	48.6 (18.5)	44 (18.4)
4. From $\sigma_c=18.7$ experiment	43.3 (25.5)	29.09 (21.6)	41.9 (14.5)	39.8 (16.4)

Note 1: The means were calculated across players and the specified rounds.

**Table 4: Summary Statistics for Effort Levels of Low Cost Subjects – Overall Means (Standard Deviations)**

	<i>Tournament (T)</i>		<i>Fixed Performance Standard (F)</i>	
	Pre-Auction Data	Post Auction Data	Pre-Auction Data	Post Auction Data
1. From all experiments	54.1 (19.3)	45.9 (19.3)	51.3 (13.5)	49.7 (12.6)
2. From $\sigma_c=12.2$ experiment	48.3 (16.3)	45.7 (13.2)	50.4 (11.3)	50.3 (7.1)
3. From $\sigma_c = 15.8$ experiment	63.7 (14.1)	57.8 (13.0)	58.7 (15.0)	56.6 (12.2)
4. From $\sigma_c=18.7$ experiment	50.4 (22.8)	34.1 (22.5)	44.9 (10.2)	43.5 (13.4)

Note 1: The means were calculated across players and the specified rounds.

**Table 5: Summary Statistics for Effort Levels of High Cost Subjects – Overall Means (Standard Deviations)**

	<i>Tournament (T)</i>		<i>Fixed Performance Standard (F)</i>	
	Pre-Auction Data	Post Auction Data	Pre-Auction Data	Post Auction Data
1. From all experiments	39.3 (21.7)	29.1 (20.1)	35.2 (17.5)	32.7 (15.5)
3. From $\sigma_c=12.2$ experiment	31.5 (16.7)	28.7 (18.8)	28.2 (17.0)	32.8 (13.2)
4. From $\sigma_c = 15.8$ experiment	50 (16.6)	34.6 (20.8)	38.5 (16.1)	31.4 (14.7)
5. From $\sigma_c=18.7$ experiment	36.2 (26.2)	24.0 (19.5)	38.8 (17.4)	34.3 (19.1)

Note 1: The means were calculated across players and the specified rounds.

**Table 6: Effort Regression Results for Subjects (Effort is Dependent Variable)**

<b>Variables</b>	<b>Random Effects Estimates</b>	
	Pre-Auction Data	Post-Auction Data
Constant	24.46* (12.78)	34.18** (10.62)
Tournament dummy (1 if obs. from a tournament session, 0 otherwise)	0.70 (11.49)	-0.80 (13.68)
Common Shock Std Deviation	0.47 (0.73)	-0.34 (0.66)
Lowcost dummy (1 if obs. from a low cost subject)	16.16** (4.21)	16.13** (4.14)
Tournament × Common Shock	0.22 (0.80)	-0.19 (0.91)
Tournament × Lowcost	-1.32 (4.76)	0.60 (5.98)
Round Effects – One dummy for each round (9 total)	Control Variables	Control Variables
Chi-Sq(12) statistic of joint significance of all right hand variables.	90.69**	48.08**
Chi-Sq(9) statistic of joint significance of round effects	31.52**	31.40**
Chi-Sq(3) statistic of the joint significance of Tournament, Tournament x Common Shock, and Tournament x Lowcost	2.27	1.52
Estimated sum of the coefficients for Lowcost and Lowcost × Tournament	14.84** (4.29)	16.73** (5.95)
Chi-Sq(2) statistic for the joint significance of the Lowcost and Lowcost × Tournament variables	19.45**	17.68**
No. of Observations	720	600

Note 1. \*\*indicates significance at the 5% level. \*indicates significance at the 10% level.

Note 2. Standard errors are contained in the parentheses below the coefficients and were calculated using the White heteroskedasticity-consistent covariance estimator (White).

Note 3. Chi-square statistics were reported for the joint tests instead of F-statistics because all that is known about the random-effects estimator is its asymptotic properties. Our regressions were estimated using STATA<sup>®</sup> which reports Chi-square statistics for random effects regressions.

**Table 7: Summary Statistics for Per-Round Earnings by Subjects – Means (Standard Deviations)**

	<i>Tournament (T)</i>		<i>Fixed Performance Standard (F)</i>	
	Pre-Auction Data	Post Auction Data	Pre-Auction Data	Post Auction Data
1. From all experiments	0.32 (0.32)	0.42 (0.29)	0.44 (0.24)	0.44 (0.25)
3. From $\sigma_c=12.2$ experiment	0.42 (0.33)	0.44 (0.32)	0.48 (0.23)	0.47 (0.24)
4. From $\sigma_c = 15.8$ experiment	0.22 (0.30)	0.34 (0.28)	0.38 (0.24)	0.49 (0.24)
5. From $\sigma_c=18.7$ experiment	0.32 (0.30)	0.47 (0.26)	0.45 (0.26)	0.35 (0.24)

Note: All numbers are given in dollar amounts. For example, 0.32 equals \$0.32 or 32 cents.

**Table 8: Summary Statistics for Per-Round Earnings by Low Cost Subjects – Means (Standard Deviations)**

	<i>Tournament (T)</i>		<i>Fixed Performance Standard (F)</i>	
	Pre-Auction Data	Post Auction Data	Pre-Auction Data	Post Auction Data
1. From all experiments	0.40 (0.32)	0.54 (0.25)	0.48 (0.25)	0.46 (0.24)
3. From $\sigma_c=12.2$ experiment	0.51 (0.32)	0.61 (0.28)	0.52 (0.24)	0.52 (0.24)
4. From $\sigma_c = 15.8$ experiment	0.29 (0.32)	0.45 (0.24)	0.41 (0.23)	0.52 (0.21)
5. From $\sigma_c=18.7$ experiment	0.41 (0.28)	0.57 (0.21)	0.51 (0.26)	0.37 (0.25)

Note: All numbers are given in dollar amounts. For example, 0.32 equals \$0.32 or 32 cents.

**Table 9: Summary Statistics for Per-Round Earnings by High Cost Subjects – Means (Standard Deviations)**

	<i>Tournament (T)</i>		<i>Fixed Performance Standard (F)</i>	
	Pre-Auction Data	Post Auction Data	Pre-Auction Data	Post Auction Data
1. From all experiments	0.24 (0.30)	0.29 (0.28)	0.40 (0.24)	0.41 (0.25)
3. From $\sigma_c=12.2$ experiment	0.32 (0.31)	0.27 (0.27)	0.44 (0.22)	0.42 (0.23)
4. From $\sigma_c = 15.8$ experiment	0.15 (0.26)	0.24 (0.29)	0.35 (0.24)	0.47 (0.25)
5. From $\sigma_c=18.7$ experiment	0.24 (0.30)	0.37 (0.26)	0.40 (0.25)	0.31 (0.23)

Note: All numbers are given in dollar amounts. For example, 0.32 equals \$0.32 or 32 cents.

**Table 10: Net Pay Regression Results for Subjects (Netpay is Dependent Variable)**

Variables	Random Effects Estimates	
	Pre-Auction Data	Post-Auction Data
Constant	0.53** (0.06)	0.69** (0.07)
Common Shock Std Deviation	-0.004 (0.003)	-0.02** (0.004)
Lowcost dummy (1 if obs. from a low cost subject)	0.08** (0.02)	0.07** (0.03)
Tournament × Common Shock	-0.01 (0.008)	0.02** (0.01)
Tournament × Lowcost	0.08* (0.04)	0.19** (0.06)
Round Effects – One dummy for each round (9 total)	Control Variables	Control Variables
Chi-Sq(12) statistic of joint significance of all right hand variables.	182.87**	132.28**
Chi-Sq(9) statistic of joint significance of round effects	29.12**	45.79**
Chi-Sq(3) statistic of the joint significance of Tournament, Tournament x Common Shock, and Tournament x Lowcost	44.22**	23.05**
Estimated sum of the coefficients for Common Shock and Tournament x Common Shock	-0.02** (0.007)	0.003 (0.009)
Chi-Sq(2) statistic for the joint significance of the Lowcost and Lowcost × Tournament variables	19.32**	32.85**
Estimated sum of the coefficients for Lowcost and Lowcost × Tournament	0.16** (0.05)	0.26** (0.05)
No. of Observations	720	600

Note 1. \*\*indicates significance at the 5% level. \*indicates significance at the 10% level.

Note 2. Standard errors are contained in the parentheses below the coefficients and were calculated using the White heteroskedasticity-consistent covariance estimator (White)

Note 3. The dependent variable is measured in dollars.

Note 4. Chi-square statistics were reported for the joint tests instead of F-statistics because all that is known about the random-effects estimator is its asymptotic properties. Our regressions were estimated using STATA<sup>®</sup> which reports Chi-square statistics for random effects regressions.

**Table 11: Incremental Impact of Tournaments on Net pay at Various Levels of the Standard Deviation of the Common Shock (Post-Auction Data)**

<i>Common Shock Standard Deviation</i>	<i>Low Cost (Low cost dummy equals "1")</i>	<i>High Cost (Low cost dummy equals "0")</i>
$s_c = 12.2$	0.014 (0.06)	-0.177** (0.04)
$s_c = 15.8$	0.088** (0.04)	-0.104** (0.04)
$s_c = 18.7$	0.147** (0.05)	-0.04 (0.06)

Note 1. \*\*indicates significance at the 5% level.

Note 2. Standard errors for the incremental effects are contained in the parentheses below the incremental effects.

**Table 12. Simulated Equilibrium Effort Level and Expected Profits Under Risk Aversion.**

	<i>Tournament</i>		<i>Fixed Performance Standard (FPSC)</i>	
	<i>Equilibrium Effort</i>	<i>Expected Earnings Per-Round</i>	<i>Optimal Effort</i>	<i>Expected Earnings Per-Round</i>
$s_C = 12.2$	High cost: 29 Low cost: 45	High cost: \$0.357 Low Cost:\$0.598 Pooled avg: \$0.4775	High cost: 25 Low cost: 47	High cost: \$0.437 Low cost: \$0.512 Pooled avg: \$0.4745
$s_C = 15.8$	High cost: 29 Low cost: 44	High cost: \$0.359 Low cost: \$0.605 Pooled avg: \$0.482	High cost: 25 Low cost: 47	High cost: \$437 Low Cost: \$0.512 Pooled avg: \$0.4745
$s_C = 18.7$	High cost: 28 Low cost: 43	High cost: \$0.335 Low cost: \$0.608 Pooled avg: 0.4715	High cost: 25 Low cost: 47	High Cost: \$437 Low Cost: \$0.512 Pooled avg: \$0.4745

Note. Payments used in the simulations were scaled up to avoid payments  $< 1$  since we have an exponential utility function. However, relative payment sizes were not changed in order to maintain the same incentives, and expected earnings were converted back into per-round measures.