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by

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Tournaments and Piece Rates:
An Experimental Study

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0: INTRODUCTION

Over the last four years a literature has developed which studies the theory of tournaments as incentive devices. See Lazear and Rosen(1981), Green and Stokey(1983) and Nalebuff and Stiglitz(1983). Tournaments are distinguished from other incentive devices, for instance piece rates, by the fact that an agent's payment in a tournament depends only on his performance <u>relative</u> to other agents covered by the incentive system. In the simplest possible case, the rank-order tournament, the agent's payoff depends only on the rank of his performance relative to other agents in the system. This literature is empirically relevant given the pervasiveness of tournaments. For example, salespeople are often paid a bonus that depends upon their sales relative to those of the other salespeople in the firm. Most managers are involved in promotion tournaments, for example six VPs competing to be promoted to President, as are assistant professors who compete for a limited number of tenured positions. Finally, election to political office may also be considered a type of tournament.

Given that a theory has been devised for these incentive systems, the next step is, of course, to test the propositions generated by the theory. This, however, is very difficult using natural data because many of the predictions hinge on properties of utility functions and the values of the rewards used. Data on these parameters are seldom available. Indeed, the only readily testable proposition to be generated by the theory concerns the distribution of prizes in sport match-play tournaments. See Rosen(1984). See also Antle and Smith(1984) for an attempt to test the theory of tournaments outside this setting. In view of the enormous problems with testing the theory of tournaments on natural data one is compelled to test the theory in an experimental setting. This paper provides the first experimental evidence on the major predictions of theory.

Our findings are described in detail later in the paper. In summary, we find that the theory explains behavior in tournaments reasonably well in the sense of predicting average behavior across identical tournaments, though it does rather poorly in predicting behavior in any single specific tournament, i.e. there is a large variance of behavior across identical tournaments. Moreover, this variance of behavior is much larger than that observed in a piece rate system. The piece rate system performed extremely well. We attribute this variance to the fact that a tournament, unlike the piece rate, is a game and so requires strategic, as opposed to simply maximizing, behavior. These conjectures are supported by our data. Furthermore, disadvantaged (high cost) agents in uneven tournaments seem to provide more effort than predicted by the theory. Rather than discouraging them, asymmetries in tournaments seem to ellicit high effort levels from disadvantaged contestants. Finally, giving contestants additional, but not complete, information about the action of their opponents also seems to raise effort levels or slow the rate at which agents converge on their optimal choices. Whether these results are generic can only be discovered by further replication of our experiments.

The paper is organized as follows. The next section describes briefly the major results of the positive theory of tournaments and in some detail the precise propositions examined experimentally. Section 2 gives the experimental design and section 3 describes the results. These are then analysed in section 4. Section 5 contains some conclusions and suggestions for further research.

1: THE THEORY OF TOURNAMENTS

Tournaments, because they involve payments to agents that are a function of relative performance, place agents in a noncooperative game. The theory of tournaments predicts the equilibrium to this game. The type of tournament that demonstrates this most clearly is the rank-order tournament in which an agent's payment depends only upon the rank of his or her performance and not upon either the absolute level of performance or the size of the differances in performance across agents. Because of its simplicity we will restrict ourselves to such rank-order tournaments.

Consider the following two-person, symmetric tournament. Two identical agents i and j have the following utility function that is separable in the payment received and the effort exerted,

$$U_i(p,e) = U_j(p,e) = u(p) - c(e) \qquad (1)$$

where p denotes the nonnegative payment to the agent and e, a scalar, is the agent's nonnegative effort. The postive and increasing functions u(.) and c(.) are, respectively, concave and convex. Agent i provides a level of effort that is not observable and which generates an output y_i according to,

$$y_i = f(e_i) + \varepsilon_i \qquad (2)$$

where the production function f(.) is concave and e_i is a random shock[1]. Agent j has a similar technology and simultaneously makes a similar decision. The payment to agent i is $M>0$ if $y_i>y_j$ and $m>0$, $m<M$ if $y_i<y_j$.[2] Agent j faces the same

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[1] O'Keeffe et al. (1984) point out that the random shock can be interpreted not only as true randomness in the technology but alternatively as random measurement error in the principal's monitoring of output.

[2] Some rule is required to deal with cases in which $y_i=y_j$. For simplicity of exposition we ignore this possibility.

payment scheme. Given any pair of effort choices by the agents, agent i's probability of winning M, $\pi(e_i,e_j)$, is just equal to the probability that $(\varepsilon_i-\varepsilon_j) > f(e_j)-f(e_i)$. Thus i's expected payoff from such a choice is,

$$Ez(e_i;e_j) = \pi(e_i,e_j)u(M) + [1-\pi(e_i,e_j)]u(m) - c(e_i) \qquad (3)$$

The above equations specify a game with payoffs given by (1) and a strategy set E given by the set of all feasible choices of effort. The theory of tournaments restricts itself to pure strategy Nash equilibria to this game. Notice that if the distribution of $(\varepsilon_i-\varepsilon_j)$ is degenerate either because there are no random shocks to output or because such shocks are perfectly correlated across agents, then the game has no pure strategy Nash equilibrium.[3]

With suitable restrictions on the distribution of the random shocks and the utility functions a unique, pure strategy Nash equilibrium will exist for the game. This is the behavioral outcome predicted by the theory of tournaments.

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[3] To see this consider the case in which the random shocks to output are identically zero. At any pair $e_i=e_j$, i can raise his expected utility by raising his effort slightly and so winning the tournament for sure. Thus no symmetric pure strategy Nash equilibrium exists. No asymmetric pure strategy Nash exists either. If such an equilibrium were to exist with $e_i>e_j$ then e_j would have to be zero and e_i infinitessimally higher but then j would have an incentive to raise e_j thereby contradicting the existence of the equilibrium.

Testing the theory requires the specification of the utility function, the production function, the distribution of $(s_i - s_j)$ and the prizes M and m. One simple specification is the following. For $k=i,j$

$$U_k(p_k,e_k) = p_k - e_k^2/c \qquad (1')$$

$$y_k = e_k + s_k \qquad (2')$$

where $c>0$ and s_k is distributed uniformly over the interval $[-a,a]$, $a>0$, and independently across the agents. e_k is restricted to lie in $[0,100]$. In this particular case i's expected payoff in the tournament is given by,

$$Ez_i(e_i;e_j) = m + \pi(e_i,e_j)[M-m] - e_i{}^2/c \ . \qquad (3')$$

J's expected payoff is given by a similar expression. If a pure stategy Nash equilibrium exists in this case it will be symmetric, $e_i=e_j=e^*$. If the equilibrium is in the interior of $[0,100]$, each agent's first order condition must be fulfilled,

$$\frac{\delta Ez_i}{\delta e_i} = \frac{\delta \pi}{\delta e_i}(e^*,e^*)[M-m] - 2e^*/c = 0 \qquad (4)$$

The concavity of the agent's payoff function ensures that (4) is sufficient for a maximum.[4] Given the distributional assumptions on the random shocks this condition can be rewritten as,

$$\frac{[M-m]c}{4a} = e^{*} \qquad (5)$$

(5) implies that 0< [M-m]c < 400a is both necessary and sufficient for an interior Nash equilibrium to exist in this example. Notice that (5) provides testable implications, namely that the efforts chosen by agents in the tournament will increase proportionally with any increase in the spread between the first and second prize, while they will move inversely with both the 'width' of the uniform distribution of e and the cost of effort paramaterized by 1/c.

These results are for a symmetric tournament. O'Keeffe et al. (1984) raised the question of how agents might behave in an uneven tournament[5] where one of the agents is 'disadvantaged', for instance, in the sense of having a disutility of effort function, i.e. c(e), that generates a higher total and marginal disutility of effort at every positive level of effort than the

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[4] Naturally, one must also check for corner solutions.

[5] O'Keeffe et al.(1984, p.30) define an even tournament as one between individuals of identical abilities or cost of effort functions. Uneven tournaments are those in which this feature is lacking.

other agent. In particular, consider the case where j's cost of effort is $\alpha e^2{}_i/c$, $\alpha>1$, while i's remains e_i^2/c. j is disadvantaged in the tournament. If an interior Nash equilibrium exists, then at the equilibrium $e_i=\alpha e_j$. When $\alpha=2$, the first order condition for the advantaged agent i in such a Nash equilibrium becomes,

$$e_i^* = \frac{[M-m]c}{2a} \tag{6}$$

and for the disadvantaged agent j becomes,

$$e_j^* = \frac{[M-m]c}{4a} \tag{7}$$

Equations (6) and (7) give testable relationships between the prize spread, c and a on one hand and the level of effort chosen on the other hand.

Several testable hypotheses about economic tournaments are suggested by the description above. The most fundamental hypothesis generated by the theory is that whenever agents are placed in a tournament described by (1') and (2') they will choose effort levels consistent with the Nash equilibrium of the associated game as described by (5). This is the first hypothesis that we tested.

HYPOTHESIS 1A: Equilibrium Hypothesis; Strong Form

The effort levels chosen in an economic tournament will not differ significantly from the effort levels associated with the pure strategy Nash equilibrium of the game defined by the tournament.

Hypothesis 1A is very strong requiring observance of Nash equilibrium behavior in every tournament, either in the real world or the laboratory. A weaker version of this hypothesis only requires that we observe such behavior on average over identical tournaments. This is summarized in hypothesis 1B,

HYPOTHESIS 1B: Equilibrium Hypothesis; Weak Form

The average effort levels chosen by agents engaged in a set of identical tournaments will not differ significantly from the Nash equilibrium of the game defined by the tournament.

If we denote the predicted effort in a tournament by x^e, the mean observed effort by x^o and the variance of observed efforts across identical tournaments by σ^2, then Hypothesis 1A is that $x^o=x^e$ and $\sigma^2=0$ while Hypothesis 1B allows $\sigma^2\neq0$. In this sense Hypothesis 1A is stronger than 1B.

Another hypothesis that can be drawn from (5) concerns the impact of changing the distribution of the random shocks and the prize spread. Specifically, (5) tells us the exact proportionate changes in the prize spread [M-m] that must be carried out in response to a change in c/a in order to keep e^* constant. This gives us hypothesis 2.

HYPOTHESIS 2: Invariance Hypothesis

Changes in [M-m] and c/a that are consistent with a fixed value of e^* in (5) will leave the average level of effort chosen across a set of identical tournaments unaltered.

Note that Hypothesis 2 is independent of Hypotheses 1A and 1B. While these hypotheses predict the equilibrium level of effort, Hypothesis 2 predicts the combinations of parameters that will leave observed effort levels constant. Hence, Hypothesis 2 tests, in effect, whether agents in a tournament are subject to any illusions concerning changes in either the prize spread or the randomness which they face. For instance, a large increase in randomness may lead them to believe that whether they win or not has become so dependant on the realization of ε that it is not worth their while to put out any effort. If, however, the prize spread has also been raised in a way consistent with (5) the agents' conclusion is incorrect.

A similar illusion problem may arise in the uneven tournament. An agent knowing that he or she is disadvantaged may become discouraged and drop out of the tournament by supplying zero effort. Alternatively the lack of symmetry in the tournament may lead the disadvantaged agent to drop out in disgust. Because economic tournaments are so common and almost always involve dissimilar agents, it is of great social and economic concern whether agents do react in these ways rather than those described by the theory and formalized in (6) and (7). This leads to our third hypothesis.

HYPOTHESIS 3: Disadvantaged Contestant Hypothesis

In uneven tournaments of the type described above, the average effort levels of the disadvantaged agents do not differ from those predicted by equation (7).

Our final hypothesis dealing exclusively with tournaments concerns the impact of changing the amount of information given to agents on tournament outcomes. The literature has assumed that the agents are simply informed of their rank. However, in many real world situations agents not only find out their rank afterwards but also by how much they won or lost, e.g. sales tournaments. In the one-shot tournaments described above such differences in information should have no impact on behavior. This can be tested. Since most economic tournaments are in fact repeated rather than one-shot and because information plays a crucial role in determining the equilibria to repeated games, this test has importance that goes beyond simply testing the available theory.

HYPOTHESIS 4: Information Hypothesis

When agents in a symmetric tournament are told both their rank and their realized outputs, their effort levels will on average not deviate significantly from those occuring in an identical tournament in which output information is withheld.

Real world organizations must decide which type of incentive system to use and so the performance of tournaments relative to other incentive systems is as important as their absolute performance. While it was infeasible to compare experimentally the incentive effects of rank-order tournaments with all other incentive systems we did compare tournaments with what is perhaps the archetypal incentive system, the piece rate. The principal comparison to test is whether a tournament that is designed to implement a certain level of effort and a piece rate designed to implement the same level of effort in fact yield identical effort choices.

HYPOTHESIS 5: Piece Rate Equivalence

The average effort levels chosen across a set of identical, symmetric economic tournaments will not differ significantly from the average effort levels chosen in an equivalent piece rate system.

Note that while a tournament defines a game for workers to play, a piece rate only asks them to solve a maximization problem. Since such problems, however complex, are free of the conjectural problems found even in the simplest games, we expect a smaller variance of behavior under a piece rate system than under a tournament. We will define the variance of behavior under a tournament as the variance across identical tournaments of the average within-tournament effort provided by the agents. In particular, we might expect the variance in behavior across tournaments to be greater than the variance of effort choices across agents for an identical piece rate system. Such a difference would clearly be important for a decision about which type of incentive system to install in an organization and so, despite the fact that both theories predict a zero variance of choices, we formulated and tested the following hypothesis.

HYPOTHESIS 6: Variance Hypothesis

The variance of effort levels chosen across a set of identical, symmetric economic tournaments is greater than the variance of effort levels chosen by agents facing an equivalent piece rate system.

We turn now to a description of the experimental design.

2: EXPERIMENTAL DESIGN

The Experiment

To test the theory outlined in the previous section we ran eight separate and different experiments. Seven of these were run, each with different parameters and subjects, to test the first four hypotheses stated in Section 1. The eighth experiment tested a simple piece rate system.

A typical experiment was conducted as follows. A group of subjects, usually 24 in number, were recruited from economics courses at New York University and brought to a room with chairs placed around its perimeter, each facing a wall. The students were randomly assigned seats and subject numbers and given written instructions. (See Appendix A for a sample of the instructions.)

Subjects were informed that another subject was randomly assigned as their "pair member" and that the amount of money they would earn in the experiment was a function of their decisions, their pair member's decisions and the realizations of a random variable. The physical identity of the pair member was not revealed. The experiment then began. Each subject was first asked to pick an integer between zero and 100 (inclusive), called their "decision number", and to enter their choice on their work sheet. Corresponding to each decision number was a cost listed in a table in the instructions. With one exception, in all the experiments these costs took the form of e^2/c, $c>0$, where e represents the decision number and c was a scaling factor used to make sure payoffs were of a reasonable size. After all the subjects had chosen and recorded their decision numbers, an experimental administrator circulated with a box containing bingo balls labeled with the integers, including zero, from -a to +a. These were called "random numbers". Each subject would pull a random number from the box, replace it, enter it on their work sheet and then add it to the decision number to yield their "total number" for that round. This information was recorded on a slip of paper which was then collected from the subject. These slips were recorded by an administrator who then compared the total numbers for each pair of subjects. It was then announced which pair member had the highest total number in each pair.[6] The pair member with the highest and lowest total numbers were awarded, respectively, "fixed payments" M and m, M>m. Each subject then calculated his or her payoff for the round by subtracting the cost of his or her decision number from the fixed payment. Notice that all of the tournament's parameters, though not the physical identity of each subject's pair member, were common knowledge.

When this round was completed and the payoffs recorded the next round began. All of the rounds were identical. Each group of subjects repeated this procedure for 12 rounds. When the last round was completed the subjects calculated their payoff

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[6] In one experiment the size of the difference between the total numbers of the pair members was also announced while in another experiment the decision numbers were also announced.

If both members of a pair had the same total number then a coin was tossed to decide which pair member was to be designated as having the highest total number. The subjects were informed of this tie-breaking procedure before the experiment began.

for the entire experiment by adding up their payoffs for the twelve rounds and subtracting $2. The experiments lasted approximately seventy five minutes and subjects earned between $5.00 and $13.00.[7] These incentives seemed to be more than adequate.

The experiment replicated the simple example of a tournament given in the previous section. The decision number corresponds to effort, the random number to the random shock to productivity, the total number to output and the decision cost to the disutility of effort.

Several points need to be made about our experimental procedures. First, great efforts were made in the instructions never to use value-laden terms. For instance, instead of calling subjects with the high total numbers "winners" we simply called them "high number people". Similarly, M and m were never called "prizes" but simply "fixed payments". This was done to remove any possible emphasis on the game nature of the experiment and reduce the possibility that winning might effect subjects' decisions independently of their payoffs. Second, subjects performed the experiment once and with only one set of parameters so that no carry-over effects from previous experiments or sets of parameters could occur. Third,

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[7] One experiment was run for 25 rounds and in this one $10 was subtracted from the sum of the payoffs for each round. This experiment lasted for almost two hours.

in recruiting subjects we took steps to minimize subject contamination. Six of the eight experiments were run within three two day sessions so that the experiments were completed quickly thereby reducing the possibility of experienced subjects talking to new subjects. We also recruited from each class only once to try to minimize experienced/new subject communication.

Notice that although the theory of tournaments deals with one-shot rather than repeated tournaments, the experimental tournaments were repeated 12 times. This was done because the decisions that the subjects were asked to make were quite complex and so the first few decisions might well have been error ridden simply because the subjects had not understood fully the problem that they faced. Such repetition is common experimental practice. It does introduce dynamic elements into a test of a static theory. However, the only subgame perfect Nash equilibrium to the twelve round repeated game involves the choice of the Nash equilibrium effort levels to the one-shot game in each round. Thus the theory's predictions for the experimental game are independent of finite repetition.[8] Hence, for many of the comparisons made the data from the twelfth

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[8] In future work we will vary pair-members randomly in each round to avoid any reputational effects. This makes the game one of incomplete information and hence generates its own problems.

round is used.[9]

Choosing Parameters

The choice of parameters for our experiments was restricted first of all by equation (5). (5) shows that if one wishes to keep predicted effort levels constant any change in (M-m), a or c must be compensated for by an appropriate change in at least one of the other parameters.[10] In addition we had to choose parameters that did not allow subjects to lose money and, in going from experiment to experiment, we tried to keep payoffs relatively constant. These multiple constraints meant that in two cases [experiments 2 and 3] we were forced to change two or three parameters from experiment to experiment. Experiments 1,5,6, and 7 involved only *ceteris paribus* changes.

Table 2.1 describes the parameter values used in the experiments. Experiment 1 is the baseline experiment in that it furnishes the first test of the predictions of the theory of tournaments as summarized by hypotheses 1A and 1B. The prizes were $1.45 and $0.86, the range of the random numbers -40 to +40 and the cost of effort (decision number) function was

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[9] In experiments there is always the possibility of terminal effects, i.e. discontinuities in behavior between the penultimate and last rounds. There was no evidence of these in these experiments.

[10] One also has to check that the agents do not find it optimal to go to a corner solution and so violate (5).

Table 2.1

EXPERIMENTAL DESIGN

TOURNAMENT EXPERIMENTS

	Experiment	Decision Number Range	Decision Cost Function	Random Number Range	Prizes	Prize Spread (M-m)	Output Function	Info	No. of Rds.	of Equilibrium	Number of Subjects
1	Narrow Random Number Range	$e_i \epsilon (0,..,100)$ $i=1,2$	$\frac{e_i^2}{10,000}$ $i=1,2$	$\epsilon_i \epsilon$ $(-40,...,+40)$ $1=1,2$	M=\$1.45 m=\$ .86	.59	$y_i=e_i+\epsilon_i$ $i=1,2$	Low	12	$e_i=37$ $i=1,2$	34
2	74 Equilibrium	$e_i \epsilon (0,..,100)$	$\frac{e_i^2}{16,000}$ $i=1,2$	$\epsilon_i \epsilon (-40,..+40)$	M=\$1.59 m=\$ .85	.74	$y_i=e_i+\epsilon_i$ $i=1,2$	Low	12	$e_i=74$ $i=1,2$	24
3	Wide Random Number Range	$e_i \epsilon (0,..,100)$ $i=1,2$	$\frac{e_i^2}{20,000}$ $i=1,2$	$\epsilon_i \epsilon$ $(-80,...,+80)$ $i+1,2$	M=\$1.02 m=\$ .43	.59	$y_i=e_i+\epsilon_i$ $i=1,2$	Low	12	$e_i=37$ $i=1,2$	24
4	Asymmetric Costs	$e_i \epsilon (0,..,100)$ $i=1,2$	$\frac{e_1^2}{25,000}$ $\frac{2e_2^2}{25,000}$	$\epsilon_1 \epsilon$ $(-40,...,+40)$ $i=1,2$	M=\$1.60 m=\$ .80	.80	$y_i=e_i+\epsilon_i$ $i=1,2$	Low	12	$e_1=70$ $e_2=35$	22
5	Medium Information	$e_i \epsilon (0,..,100)$ $i=1,2$	$\frac{e_i^2}{10,000}$ $i=1,2$	$\epsilon_i \epsilon (-40, ...+40)$	M=\$1.45 m=\$ .86	.59	$y_i=e_i+\epsilon_i$ $i=1,2$	Med.	12	$e_i=37$ $i=1,2$	26
6	High Information	$e_i \epsilon (0,..,100)$ $i=1,2$	$\frac{e_i^2}{10,000}$ $i = 1,2$	$\epsilon_i \epsilon (-40,.,+40)$ $i=1,2$	M=1.45 m= .86	.59	$y_i=e_i+\epsilon_i$ $i = 1,2$	High	12	$e_i=37$ $i=1,2$	28
7	25 Round Experiment	$e_i \epsilon (0,..,100$ $i=1,2$	$\frac{e_i^2}{10,000}$ $i = 1,2$	$\epsilon_i \epsilon (-40,.,+40$ $i=1,2$	M=1.45 m= .86	.59	$y_i=e_i+\epsilon_i$ $i = 1,2$	Low	25	$e_i=37$ $i=1,2$	20
8	Experiment Piece Rates	$e, \epsilon (0,..,100)$	$\frac{e_i^2}{10,000}$	$\epsilon_i \epsilon (-20, ..+20)$	N.A.	N.A.	$y_i=e_i+\epsilon_i$	N.A.	12	e=37	13

$\$e^2/10,000$. Because we ran a similar experiment with random numbers ranging between -80 and +80, we called experiment 1 the narrow random number range experiment. Given these parameter values, the pure strategy Nash equilibrium to the tournament can be found from (5) to be 37. The parameters were chosen to yield a Nash equilibrium of 37 because that number did not seem to be any kind of natural focal point in the way that, for instance, 50 might be. Thus if 37 is observed as an outcome it would provide striking support for the theory.

In order to check the robustness of the results of experiment 1 we ran a second experiment in which the parameter values (see Table 2.1) were chosen so as to give a pure strategy Nash equilibrium of 74. Thus this second experiment gives us an independent test of hypotheses 1A and 1B.

Experiment 3, while furnishing still another test of hypotheses 1A and 1B, also tests hypothesis 2, the invariance hypothesis. Relative to experiment 1, the prize spread was left constant at $0.59 but the range of the random numbers was doubled to -80 to +80 (hence this is called the wide random number range experiment) while the cost of effort was halved to $\$e^2/20,000$. This halving of marginal cost and doubling of the range of the random number given the same prize spread will, according to (5), leave the pure strategy Nash equilibrium effort level unaltered at 37. Notice that now the random element in output has become very large so one might, contrary to the theory, expect a reduction in effort because payoffs are now extremely dependent on chance. A comparison between the outcomes of experiments 1 and 3 enable us to test hypothesis 2.

The fourth experiment tested the disadvantaged contestant hypothesis, Hypothesis 3. This experiment had prizes of $1.60 and $0.80 and a range of -40 to +40 for the random numbers. While one member of each pair had a cost of effort of $\$e^2/25{,}000$ the other member was disadvantaged by having a cost of effort exactly double this. This fact was common knowledge. From equations (6) and (7) we can see that the theory predicts that the low cost member of each pair will choose 70 while the disadvantaged member will choose 35.

Experiment 5 tests the information hypothesis, Hypothesis 4. The experiment was identical to experiment 1 except that in each round, after the total numbers had been collected from the subjects, the subjects were told not only which of them had the highest total numbers but also by how much their total number exceeded that of their pair member. Each subject could calculate his or her corresponding pair member's total number in each round. A comparison of the results of experiments 1 and 5 thus gives a test of the information hypothesis.

Experiments 6 and 7 were run after we observed that, while the means behaved roughly as expected, the variance of effort across tournaments was large throughout the full twelve rounds of the experiments. The large variances led us to ask if there was some aspect of the experiments that could be changed to reduce them. One explanation for the large variances is that the random number was preventing subjects from inferring accurately the behavior of their opponent. Another is that the conjectural aspect of the decision problem generated a wide variety of behaviors. Experiment 6 was designed to distinguish between these explanations.

The clearest way of distinguishing between the two hypothesized explanations for the variance of behavior is to provide subjects at the end of each round with the decision number chosen by their opponent. This is what we did in experiment 6 which otherwise replicated experiment 1. In this experiment subjects were completely informed about all past plays by their opponent and so if a large variance in behavior is observed across these experimental tournaments we can conclude that the variance in behavior observed in tournaments is not due simply to the presence of the random variable but is instead due to the game nature of tournaments.[11]

While experiment 6 tests the two proposed explanations for the variance of behavior, as a practical guide to the design of tournaments it is of little use because it requires the revelation of effort choices which are, by assumption, not

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[11] Experiment 6 also sheds further light on the Information Hypothesis.

known by the principal. With this in mind we investigated an alternative way of increasing the accuracy of inferences about the behavior of the opponent namely increasing the number of repetitions. As the period between payoffs in repeated tournaments is in the control of the principal this potentially provides the principal with a way of reducing the variance of behavior _if_ that variance is due to such inference problems. In experiment 7 we repeated experiment 1 but extended it to 25 rounds.

The final experiment, experiment 8, dealt with a piece rate. In order to test hypotheses 5 and 6 it was necessary to construct an equivalent piece rate system. In this experiment 13 subjects entered a room and were given writen instructions. In each of twelve identical rounds they were asked to choose an integer between 0 and 100 having first considered a table giving the costs of choosing each decision number. This cost function was $\$e^2/2000$ and the output function was $0.2 + 0.037e + \varepsilon$. The subjects recorded their decision number and then drew a bingo ball from a box containing 41 balls numbered from, and evenly spaced in, -0.2 to $+0.2$ including zero. They recorded the random number (ε). The subject's payoff for the round was just his or her output times one dollar minus the cost of the decision number. Given the payoff function the maximizing choice for a subject was 37. Thus by comparing the results of this experiment with those of experiments 1 or 3 we can test hypotheses 5 (the piece rate equivalence hypothesis) and 6 (the variance hypothesis). Moreover, because of its simplicity this experiment gave us a check on the ability of subjects to comprehend the procedures we were asking them to follow.

The next section reports the results of these experiments.

3: EXPERIMENTAL RESULTS

The broad outlines of the results of the experiments are given in Table 3.1 and Figures 1 through 9. Table 3.1 shows summary statistics for the the first and last six rounds as well as the twelfth round. For the two six round periods it reports, for each experiment, the six round means and variances of the average (across pairs) choices made[12]. It also displays the average choice and variance for the last round. Figures 1 through 8 plot for each experiment the average choices made in each round while Figure 9 combines six of these plots for experiments in which the Nash equilibrium was 37.

The piece rate experiment serves as a point of comparison for many of the tournament experiments and so merits our attention first. As can be seen from Figure 1 and Table 3.1,

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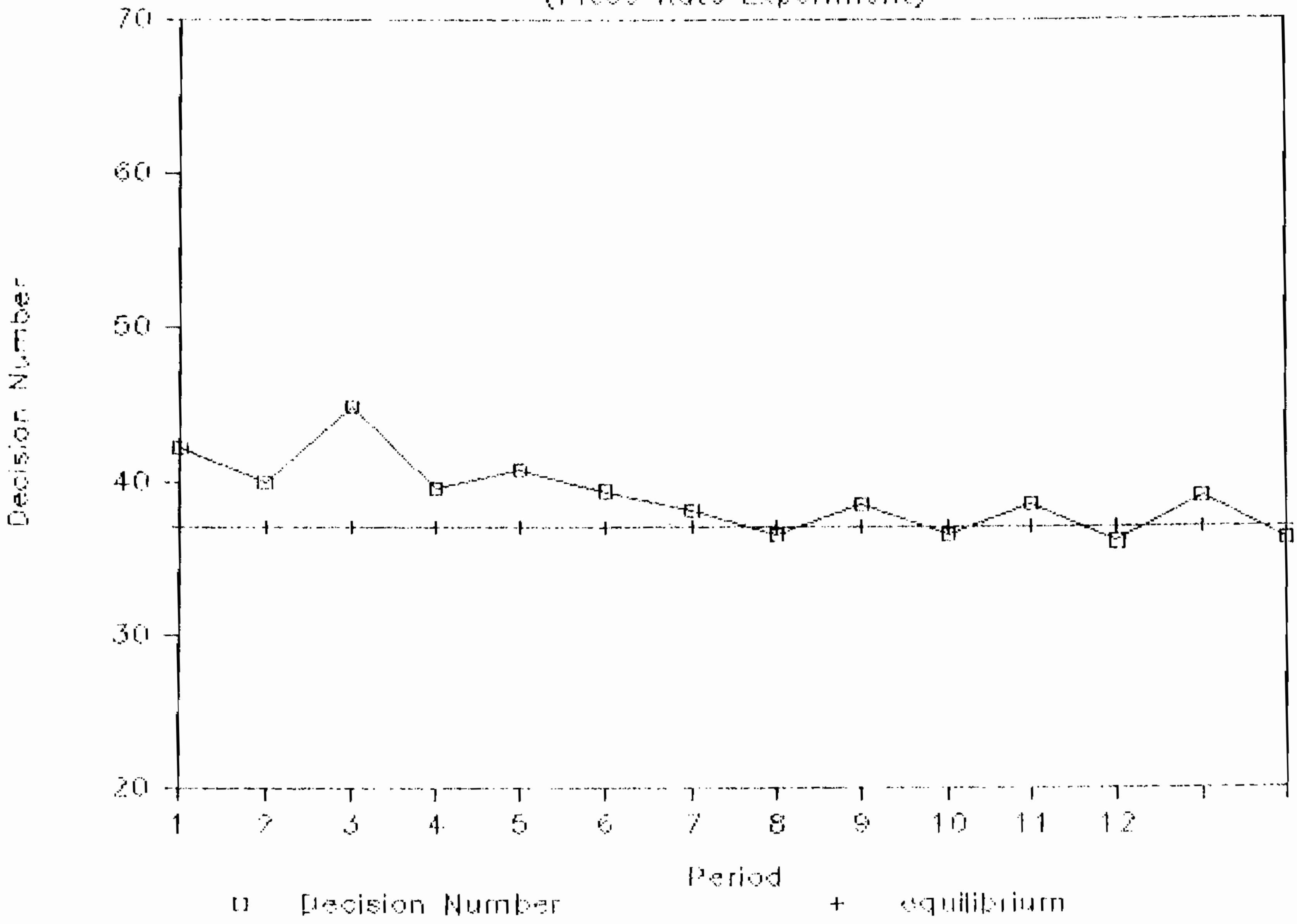
[12] Recall that the variance refers to the variance of the average choice of effort within a tournament, across tournaments.

Table 3.1 - Experimental Results: Means and Variances

Experiment	Mean Decision Number Rounds 1 - 6	Mean Decision Number Rounds 7 - 12	Variance in Decision Number Rounds 1 - 6	Variance in Decision Number Rounds 7 - 12	Mean Decision Number Round 12	Variance in Decision Number Round 12
1	44.00	38.78	242.50	259.33	36.94	311.45
2	65.77	68.85	871.62	892.05	67.61	1005.37
3	42.17	33.55	111.18	87.14	32.50	109.94
4 low cost	69.73	73.33	620.15	708.13	75.55	766.73
high cost	49.68	57.86	529.58	905.03	56.46	805.52
5	54.02	48.86	231.88	222.86	46.11	230.39
6	38.83	33.48	594.67	552.74	36.14	636.77
7	Rd. 1-12 50.62	Rd.13-25 48.00	Rd. 1-12 303.88	Rd. 13-25 362.01	Rd. 25 44.63	Rd. 25 466.44
8	40.44	38.91	103.61	87.38	37.38	33.66

Figure 1

(Piece Rate Experiment)



the piece rate system tested did very well. The theoretical mean effort level was 37 (just as in our baseline tournament) and the mean effort level in the twelfth round was 37.38. This mean is not significantly different from 37 at the 95% confidence level using a median test. The variance across subjects was 33.16. This variance is remarkably small when compared with the tournament variances. In addition, the mean effort level never deviated from 37 by more than 3 during the entire twelve rounds. From the point of view of our experimental design these results are encouraging because they show that the subjects understood our instructions, were motivated by the incentives to carry out calculations and were capable of solving reasonably sophisticated problems. Furthermore, if one accepts that the subjects did understand the instructions and did carry out some kind of maximization, then the fact that the theoretical predictions, which relied on risk neutrality, were empirically confirmed suggests that our subjects were indeed risk neutral over the range of payoffs they were presented with. In summary, our laboratory piece rate system performed exceedingly well and provided strong evidence that our subjects were capable of making the calculations necessary to maximize their expected returns.

The experimental tournaments constitute the closest game-form analogue to the piece rate system just described. Despite this fact, our results offer weaker support for the theory of tournaments than was found for the theory of piece rates. We believe that this is at least partly due to the fact that even simple games are behaviorally far more complex objects than maximization problems. This fact is clearly demonstrated in the data.

Experiments 1, 2 and 3 provide three independent tests of hypotheses 1A and 1B, that is whether the predictions of the theory of tournaments hold in each tournament or on average across tournaments. Both Table 3.1 and Figures 2,3 and 4 show that the average decision by the subjects tended to converge to the neighborhood of the effort level predicted by the theory and converged exactly in experiment 1. In that experiment the theory predicted a choice of 37 and the average choice in the last round was 37.02. Moreover, convergence to this final number was practically monotonic. The theoretical prediction for experiment 2 was 74 and by the last round the average choice was 67.61 (the average over the last four rounds was 70.5). In both cases a median test failed to reject equality between the theoretically predicted and observed medians at a 95% confidence level.[13] Thus there was a tendency for mean

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[13] All statistical tests of hypotheses about means were tested using a non-parametric test on medians. As the theory predicts a zero variance in choices, the theoretically predicted median is equal to the mean. A 95% confidence interval was used throughout. It is important to note that the power of these tests is quite low.

Figure 2

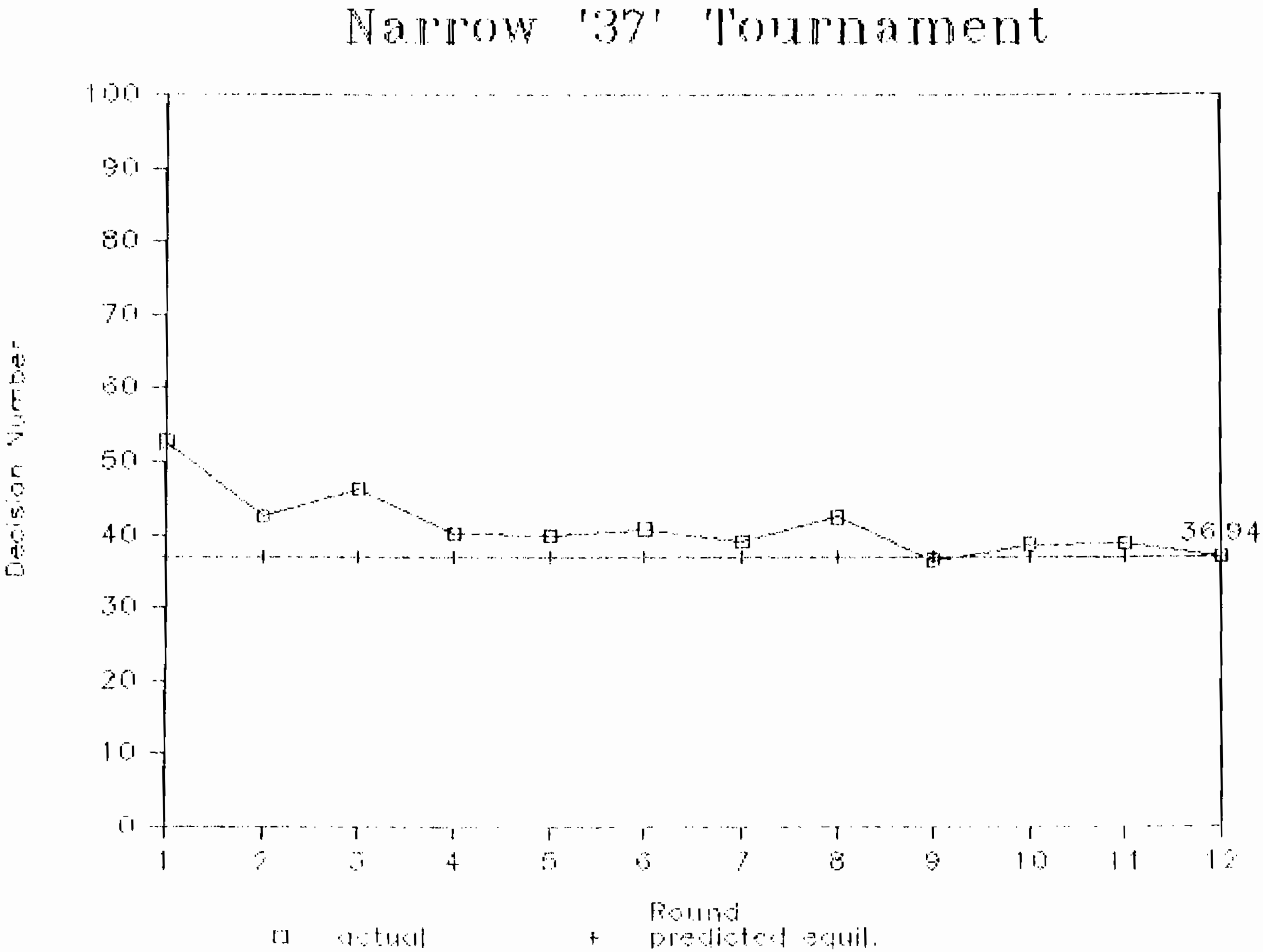

Narrow '37' Tournament
Decision Number
100
90
80
70
60
50
40
30
20
10
0
1
2
3
4
5
6
7
8
9
10
11
12
36.94
Round
□ actual
+ predicted equil.

Figure 3

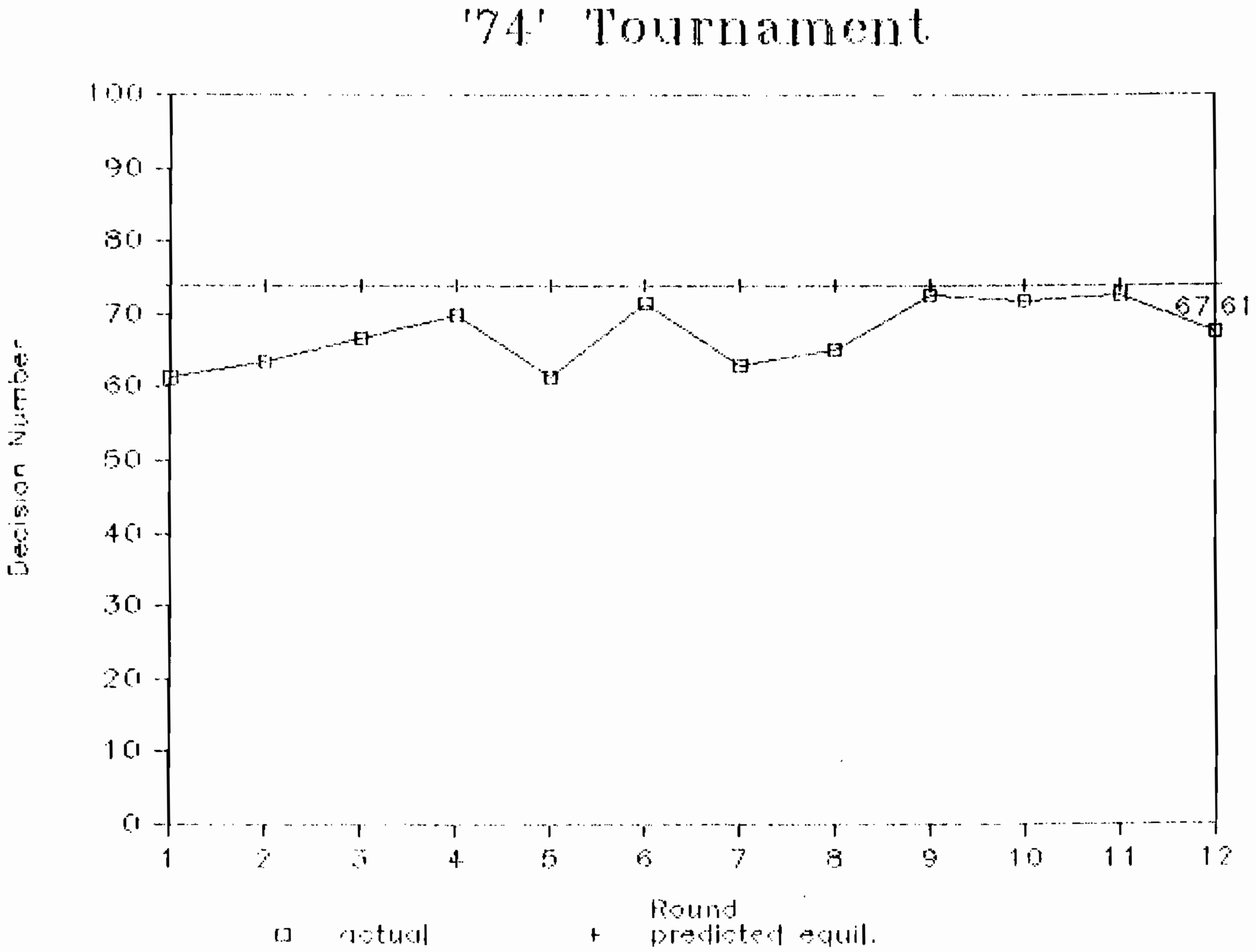
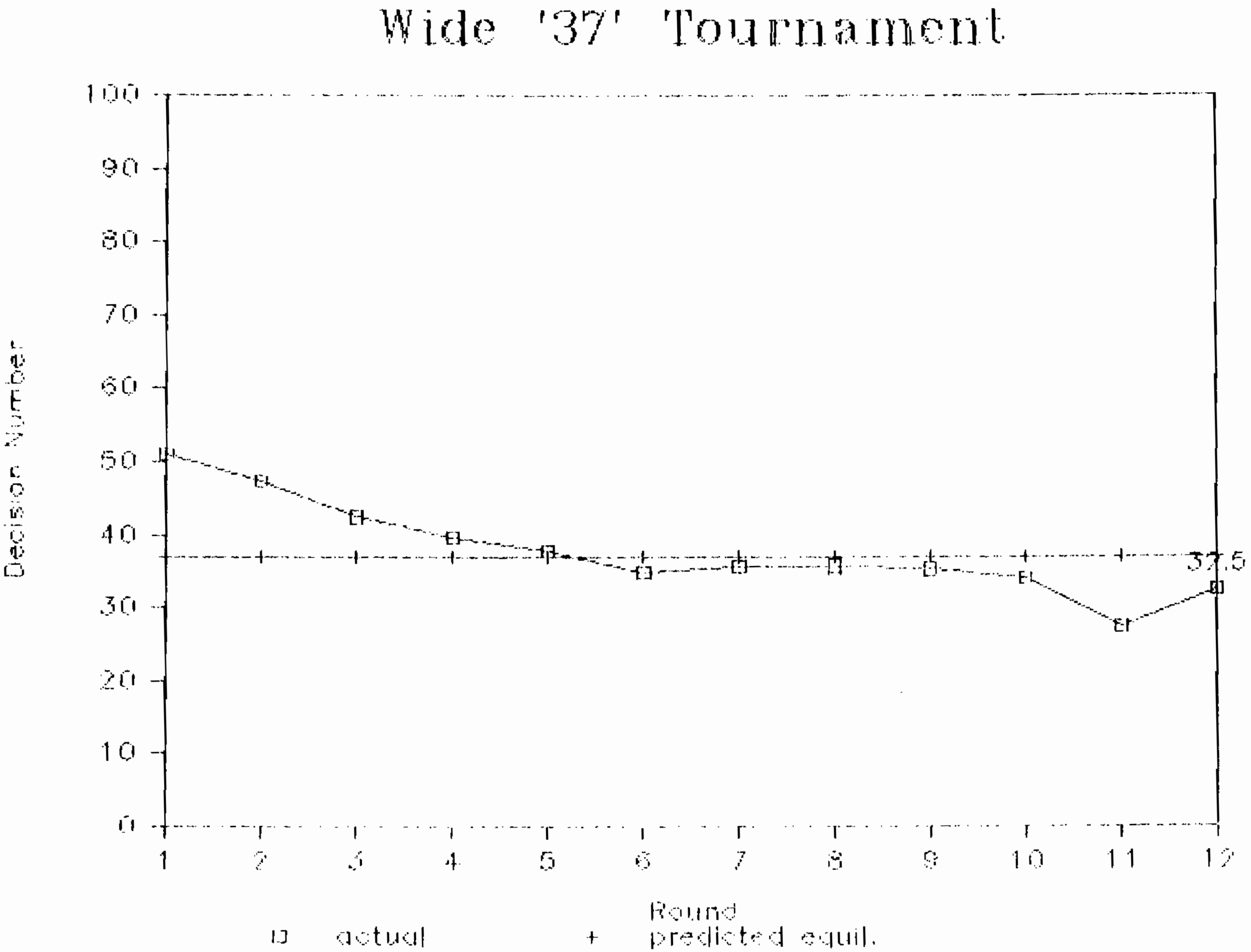

'74' Tournament
Decision Number
100
90
80
70
60
50
40
30
20
10
0
1
2
3
4
5
6
7
8
9
10
11
12
Round
67.61
□ actual
+ predicted equil.

Figure 4


Wide '37' Tournament
Decision Number
100
90
80
70
60
50
40
30
20
10
0
1
2
3
4
5
6
7
8
9
10
11
12
37.5
Round
actual
predicted equil.

effort levels to converge <u>toward</u> their theoretically expected levels in even tournaments. Although the large variance in behavior in the last round prevents us from saying anything very precise about whether choices are converging to the theoretically predicted level or some other level in its neighborhood, there does appear to be systematic behavior at work. In broad terms, mean effort levels in even tournaments usually started around 50 in period 1 and, if the theoretical equilibrium was below 50, then drifted down more or less monotonically and drifted up if the theoretical equilibrium was above 50.

These three experiments do not, then, reject Hypothesis 1B. Hypothesis 1A, however, fares less well. While both hypotheses state that the mean effort level in the twelfth round will equal the theoretical value, Hypothesis 1A requires that the variance across identical tournaments around this value be zero. In fact the variance of the choices in the last round of experiments 1,2 and 3 were, respectively, 311.45, 129.42, and 109.94. These are considerably larger than the zero variance predicted by the theory and are also far higher than the variance of 33.16 found in the piece rate. Moreover, we can see from Table 3.1 that there was no apparent tendency for the variance to decline as the experiments progressed. Thus Hypothesis 1A is not supported by our data, i.e. the predictions of the theory do not hold for each individual tournament.

Experiment 3, the wide random variable experiment, was used to test the invariance hypothesis (Hypothesis 2). This hypothesis predicts that a mean preserving spread of the uniform distribution of the random variable, offset by a reduction in the cost of effort in a manner consistent with equation (5), will leave the average choice across agents in the last period unaltered. This hypothesis can be tested by comparing the twelfth period means of experiments 1 and 3. Using a Wilcoxon-Mann-Whitney test we find that these means were not significantly different at the 95% level of significance and so Hypothesis 2 cannot be rejected.

Comparing the results of experiments 1 and 8 enables us to test Hypotheses 5 and 6, the piece rate equivalence and variance hypotheses. At the 95% level, using a Wilcoxon-Mann-Whitney test we could not reject the hypothesis that the mean twelfth round effort levels were the same in the piece rate and the narrow random variable tournament. The data also does not reject the variance hypothesis; the variance of effort levels in the piece rate is only one tenth of that for the tournament in experiment 1 and is less than one third of the lowest variance observed in any of the experimental tournaments.

Hypothesis 3, the disadvantaged contestant hypothesis, was tested by comparing the results of experiment 4 with the theoretical predictions given by equations (6) and (7). The average choice of the high cost subjects in the last round was 56.46 which is much higher than the 35 predicted by the theory of tournaments and a Wilcoxon-Mann-Whitney test shows that this average choice is, at the 95% level, significantly higher than that of the contestants in experiment 1 in which the theory predicted a choice of 37. [See Figure 5] The low cost subjects, however, are predicted by the theory to choose approximately 70 and their average choice in the last round was 75.55. Thus we are faced with the outcome that on average the theory underpredicts greatly the choices of the disadvantaged subjects and slightly underpredicts that of the advantaged agents. Subjecting contestants to a disadvantage seems to ellicit higher effort from them rather than discourage them or cause them to drop out (provide zero effort) as we had informally conjectured.

The question of relevance is then, why do disadvantaged contestants choose effort levels which are higher than that predicted by the theory? One possible explanation for this is that in a tournament the payoff is not just a function of the monetary reward and the cost of effort but also of winning and losing the game.[14] If, in contrast to the theory, contestants

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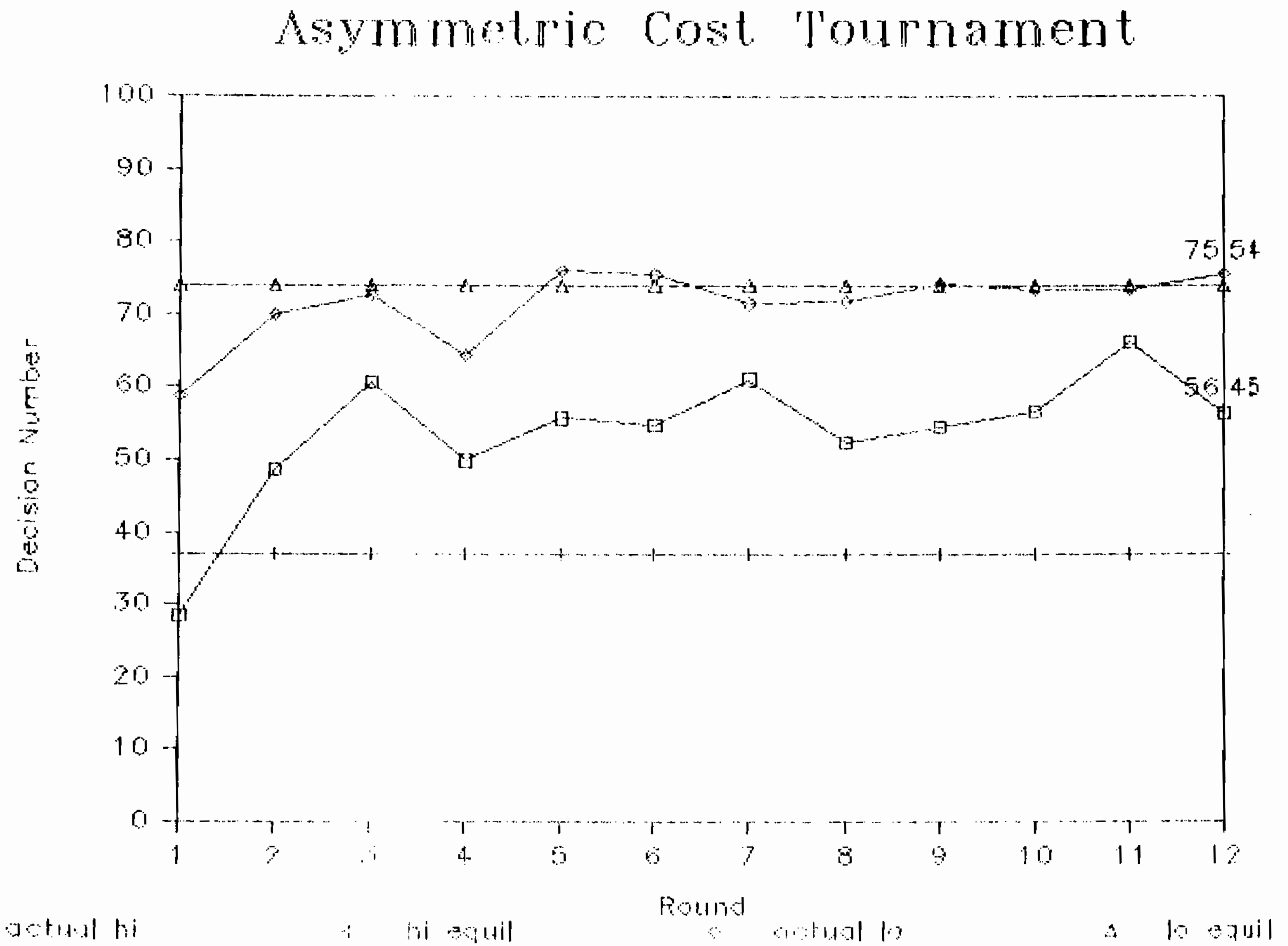
[14] A lack of indifference to winning and losing probably exists in all experiments that involve a game.

Figure 5


Asymmetric Cost Tournament
Decision Number
0
10
20
30
40
50
60
70
80
90
100
1
2
3
4
5
6
7
8
9
10
11
12
Round
75 54
56 45
actual hi
hi equil
actual lo
lo equil

are not, for a given monetary payoff, indifferent to whether they win or lose, then if the low cost contestants choose 70, the high cost contestants will choose a number higher than the 35 predicted by theory.[15] However, this explanation fails to explain the behavior of the low cost contestants. While these contestants' average last period choices were very close to those predicted by the theory they were not best responses to the actual average last period choices of the high cost contestants. This best response was 86. Thus the low cost contestants not only did not reveal a utility of winning but, if anything, revealed a disutility of winning. At this time we have no explanation for the behavior observed in this experiment. Indeed, the result is sufficiently odd to suggest that more experimental work on uneven tournaments is a high priority task.

Experiment 5 investigated the impact of providing more information in the form of reporting the total numbers to the subjects at the end of each round. Hypothesis 4 predicts that this information will have no effect on behavior. A Wilcoxon-Mann-Whitney test for the equality of last period

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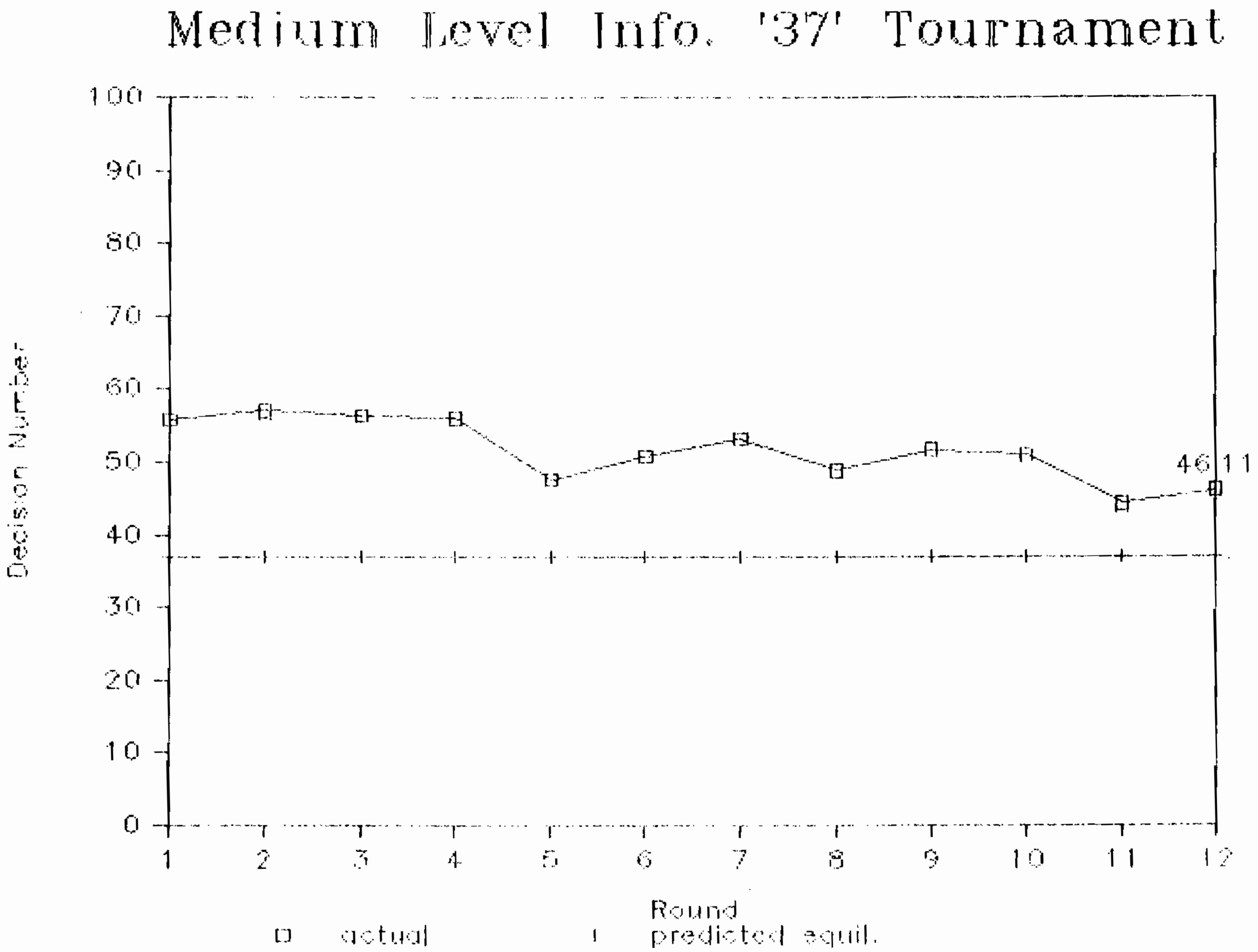
[15] If contestants are getting a positive utility from winning this does not violate Smith's precept of saliency in experimental design (See Smith,1982). That precept only requires in this context that the contestants, when faced with two alternatives that are identical in all respects other than the pecuniary payoff, choose the alternative with the higher pecuniary payoff.

means between experiments 1 and 5 did not reject the null at a 95% confidence level and so hypothesis 4 is not rejected. We had thought that the provision of output information would leave average choices in the twelfth round unaltered but might have reduced the variance of choices in that period compared with that in experiment 1. This hope was not fulfilled. The last period variances in experiments 1 and 5, respectively, were 311.45 and 230.39 which are quite similar. Figure 6 does, however, point out that mean effort levels seem, at best, to converge more slowly in experiment 5 than in experiment 1. This result is curious since when in experiment 6 even more information was given to the subjects their mean effort levels were closer to 37. Because of its practical importance in tournament design, further research on the impact of changing information structures on tournament behavior is clearly called for.

The large variance in behavior, both in absolute terms and relative to that of the piece rate, exhibited in all our experiments demands explanation. Experiment 6, the high information experiment, was designed to discriminate between the two most obvious explanations namely that subjects were making erroneous inferences about the choices of their opponents or that the strategic nature of the tournament, by introducing arbitrary and possibly person-specific conjectural elements into their decision making, generated a variance of

Figure 6



Medium Level Info. '37' Tournament

behavior. By giving the subjects full information about their opponent's prior choices we can rule out errors in inferring these choices as an explanation of variance in behavior. Table 3.1 and Figure 7 show that in terms of mean effort levels in period 12, the subjects behaved much as in experiments 1 and 3. It is interesting to note, however, that convergence in this experiment was from below 37. This differed from the type of convergence seen in other symmetric tournaments with the same parameters (see Figures 2,6 and 7). This is strange because in experiment 5 where there was an intermediate level of information, effort levels remained above their theoretical equilibrium level in every period. In fact, in every period except one, the effort levels were ranked, medium information greater than low information greater than high information. This consistency across periods raises the possibility that this may be a generic characteristic of tournaments.

Finally, the variance of effort levels in the last round was 636.77 which is even higher than in experiments 1,3 and 5 which used the same parameter values and is twenty times that observed in the piece rate. Thus experiment 6 rejects the errors-in-inference explanation of the variance in behavior across tournaments and strongly suggests that it is simply the strategic nature of tournaments that gives rise to this variance.

With a view to providing a practical way of reducing the variance in behavior, experiment 7 iterated the tournament of experiment 1 25 rather than 12 times. Table 3.1 shows that in fact the variance of behavior was not reduced significantly by increasing the number of repetitions. Given the results of experiment 6 this should not be surprising. While playing against an opponent for longer may enable you to make more accurate inferences about his strategy, we have already seen that this informational problem is not the cause of the high variance. Notice that Figure 8 shows that the mean effort level in experiment 7 behaved somewhat differently compared with other experiments using the same parameters. In particular, it did not fall very rapidly at all.

Finally, notice that experiments 1,3,5,6 and 7 all used parameter values that give a Nash equilibrium of 37. Indeed, all of them, except 3, use identical parameter values. The paths of the mean effort levels are collected in Figure 9. This Figure confirms the observations made on the basis of experiments 1,2 and 3 alone that there is some systemmatic behavior at work. Also plotted on Figure 9 is the result of the 74 tournament. If we contrast this plot with those of the 37 Nash experiments the systematic behavior appears to be loosely predicted by the theory of tournaments.

4: CONCLUSIONS

Figure 7

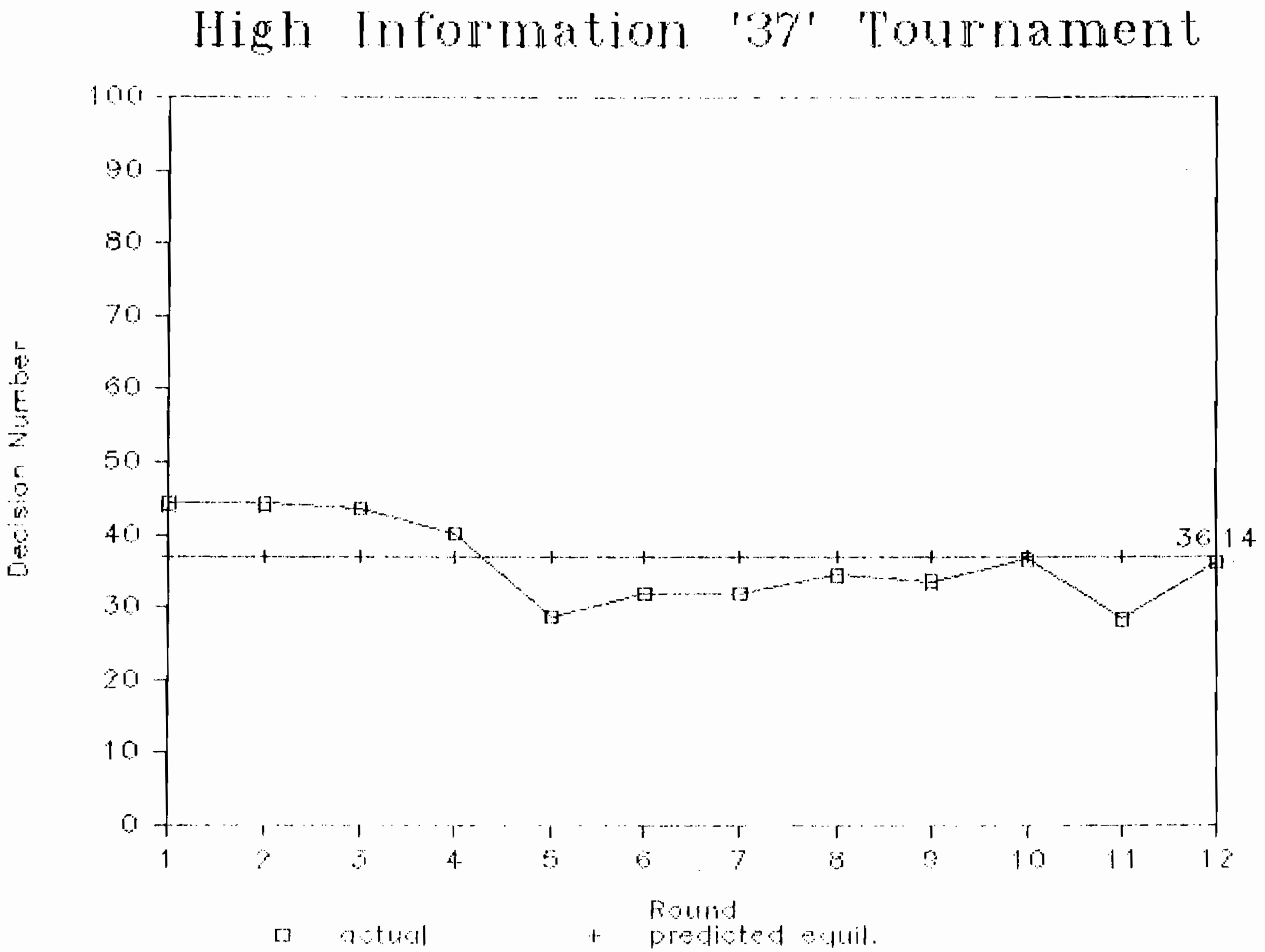
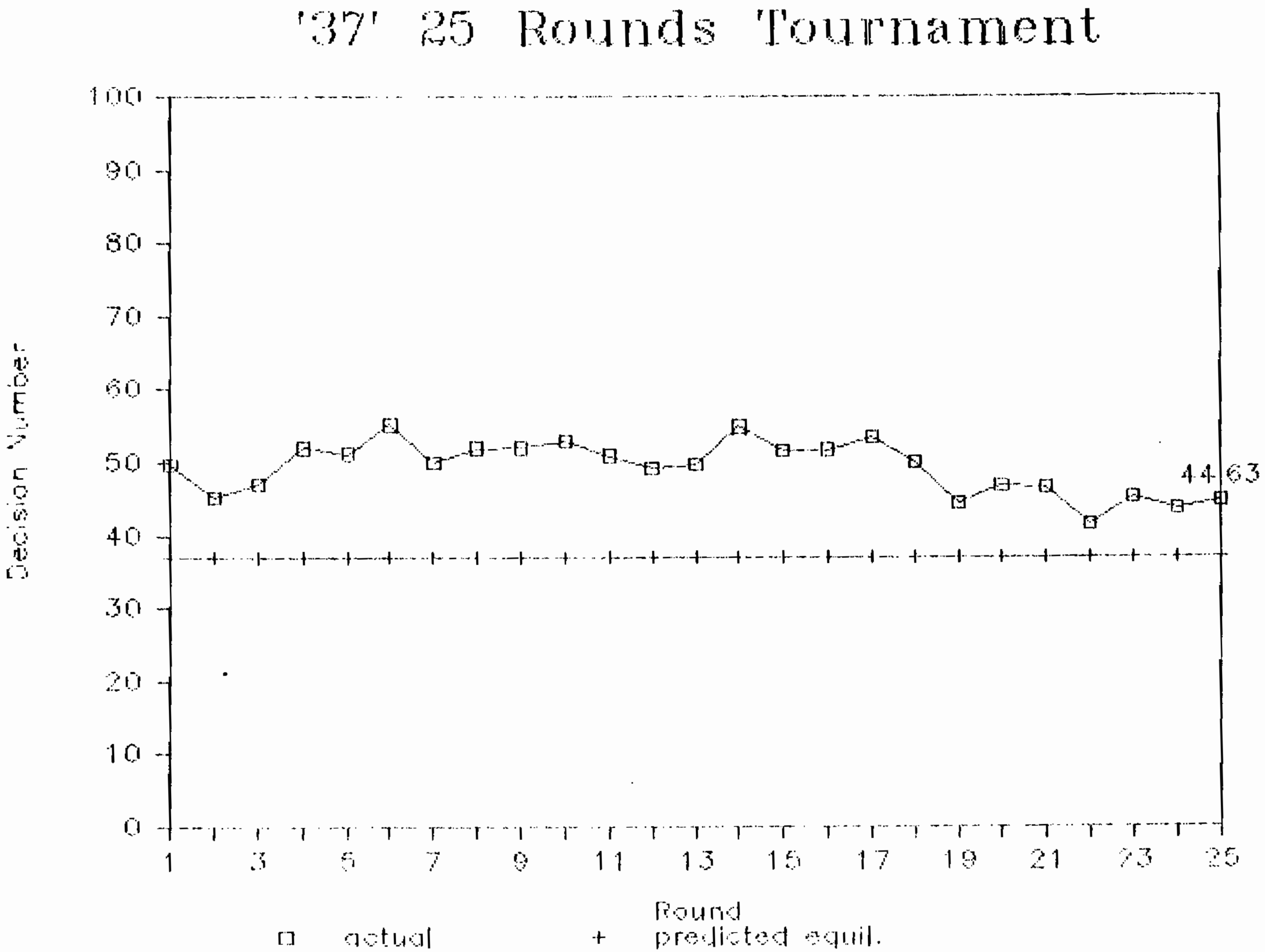

High Information '37' Tournament
Decision Number
100
90
80
70
60
50
40
30
20
10
0
1
2
3
4
5
6
7
8
9
10
11
12
Round
36.14
□ actual
+ predicted equil.

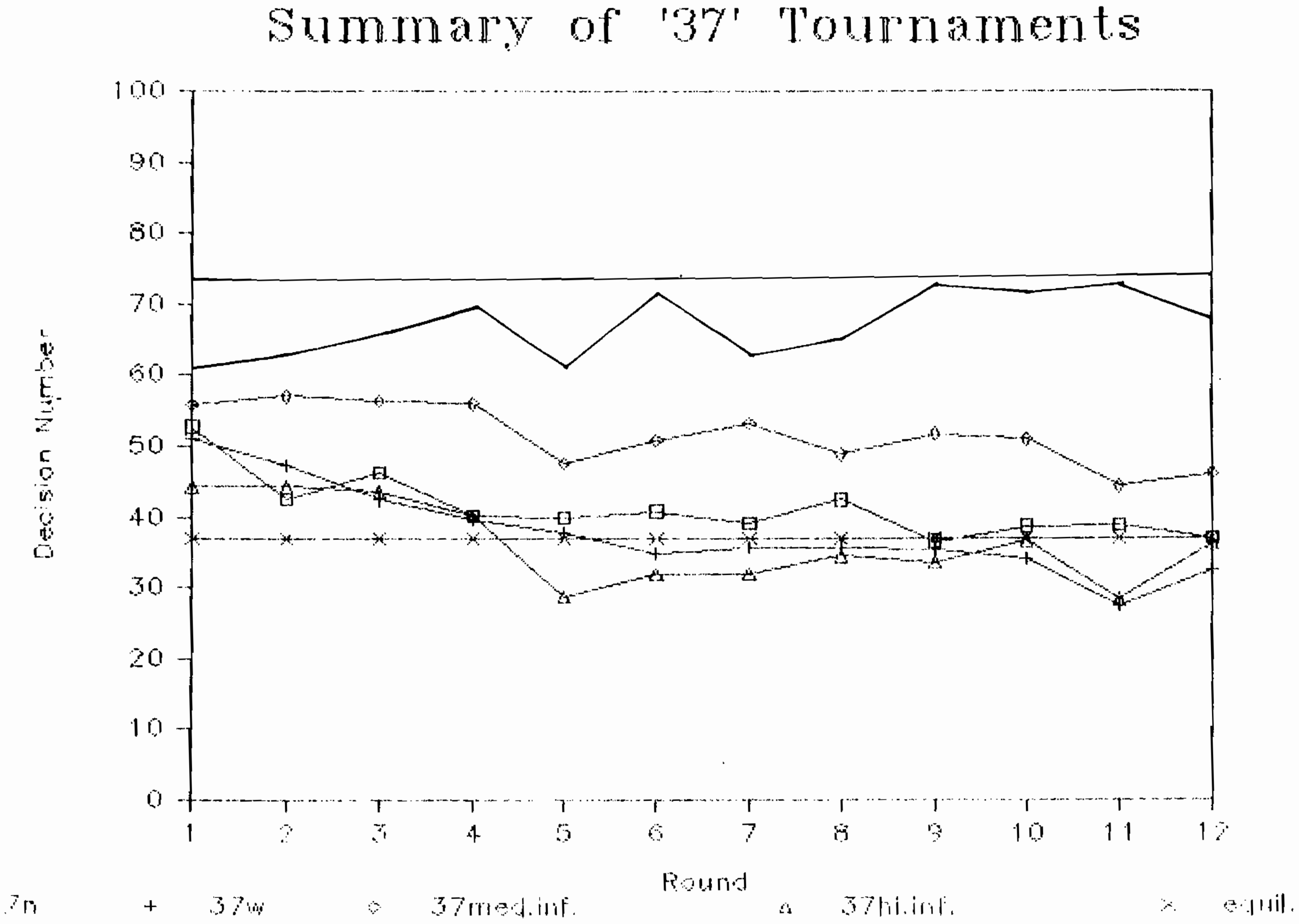
Figure 8


'37' 25 Rounds Tournament
Decision Number
100
90
80
70
60
50
40
30
20
10
0
1
3
5
7
9
11
13
15
17
19
21
23
25
44.63
Round
□ actual
+ predicted equil.

The experimental results show that there is systematic behavior by agents when faced by a tournament. This is demonstrated most clearly in Figure 9 and is also shown by the very different behavior of the high and low cost subjects in the uneven tournament. However, while on average there is systematic behavior there is at the individual level a very large variance of behavior. As we have seen, this latter fact contrasts sharply with what is observed in a piece rate setting and, we conjecture, in other incentive systems that rely solely on simple maximization by the individual agent. Such variance of behavior in tournaments seems to be a result of their game nature rather than the information structure usually assumed.

At a gross level the theory of tournaments seems to be able to predict at least some of the qualitative properties of systematic average behavior. Thus if we consider two even tournaments whose Nash equilibria are in some sense far apart our experiments suggest that average behavior in those two tournaments will diverge in the way predicted by the theory. Similarly, in an uneven tournament in which the different types of subjects differ substantially, the theory predicts qualitatively the right differences in average behavior between the types though here the theory's quantitative predictions seem to be in error. In this sense the theory did quite well in these experiments. Where it did considerably less well was in

Figure 9



Summary of '37' Tournaments
Decision Number
100
90
80
70
60
50
40
30
20
10
0
1
2
3
4
5
6
7
8
9
10
11
12
Round
7n
+ 37w
◇ 37med.inf.
△ 37hi.inf.
× equil.

the accuracy of its quantitative predictions. This is very clear at the level of the individual tournament. Here behavior was extremely diverse across subjects, a result that is foreign to the theory.

This inability of the theory to explain the diversity of individual behavior has significant practical and theoretical implications notably for the choice of incentive systems. In particular, incentive systems based purely on individual maximization seem to result in much less diversity of behavior than occurs in tournaments. Because this difference between the two types of incentive systems does not show up in the theory of the two systems, it has not entered into the discussion of the environments in which tournaments are more efficient than piece rates. However, as a practical matter it appears that a cost to choosing a tournament system over a piece rate system is that the principal must bear uncertainty as to how the agents will react to the tournament.

If the principal is risk neutral, as is usually assumed in the literature, he or she can bear the uncertainty concerning the outcome of the tournament at no cost. The agents, however, are usually treated as risk averse. Our results suggest that in deciding whether or not to join a tournament the agents will have to take into account two sources of uncertainty. One of these is a part of the existing theoretical literature namely the distribution of the prizes induced by the randomness in production. Note that this distribution is conditional upon the use of Nash equilibrium strategies by all the agents in the tournament. The second source of uncertainty which is not in the literature is precisely the uncertainty concerning how the specific tournament that the agent enters will be played. The fact that an agent must bear this uncertainty will be reflected in a higher expected payment necessary to induce him or her to join the tournament. Thus even a risk neutral principal will have to take into account this additional source of uncertainty.

In our experiments the theory of tournaments did much better in symmetric tournaments than in uneven ones. In the relevant experiment the theory predicted well the average behavior of the low cost agents but underpredicted the effort of the high cost or disadvantaged agents. Because most actual tournaments are uneven this failure of the theory is particularly important and so extensions of the theory in this direction are to be encouraged.

All of these conclusions are necessarily highly tentative because they are based on a very small amount of experimental evidence. While there is certainly plenty in the evidence presented here to keep theorists busy, we feel that the most pressing need is for considerably more extensive experimental evidence on tournaments to see whether the deviations from the theory noted here are empirical regularities or not.

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Appendix A

Instructions for experiment 1.

Subject #____

Instructions

Introduction

This is an experiment about decision making. The instructions are simple, and if you follow them carefully and make good decisions, you could earn a considerable amount of money which will be paid to you in cash.

Specific Instructions

As you read these instructions you will be in a room with a number of other subjects. One of these subjects has been chosen to be paired with you by a random drawing of subject numbers. This subject will be called your pair member. The identity of your pair member will not be revealed to you.

In the experiment you will perform a simple task. Attached to these instructions are two sheets, labelled sheet 1 and sheet 2. Sheet 1 shows a 100 numbers, from 0 to 100 in column A. These are your decision numbers. Associated with each number is a decision cost, which is listed in column B. Note that the higher the decision number chosen, the greater is the associated cost.

Your pair member has an identical sheet. In each round of the experiment, you and your pair member will each select a decision number separately. Record your number in column 1 of sheet 2 and record its associated cost in column 5 of sheet 2.

When all subjects have selected their decision numbers, an experimenter will bring around a cage containing 81 balls, numbered from -40 to +40. Each of you will draw one of these balls. The number of this ball will be called your random draw number. Record the random draw number in column 2 of sheet 2, and then replace the ball in the cage.

Calculation of Payoffs

Your payment in each round of the experiment will be computed as follows. You will add your decision number, and random draw number, and record this sum in column 3 of sheet 2. Your pair member will do the same.

Since all subjects have worked in privacy, the experimenter will then compare the totals of you and your pair member, and you will be told by how much your total is greater or less than that of your pair member. If your total in column 3 is greater than your pair member's total in column 3, you receive the fixed payment X ($1.45), if not you receive Y ($0.86). Whether you receive X or Y as your fixed payment only depends on whether your total is greater than your pair member's. It does not depend on how much bigger it is. Circle the appropriate fixed payment in column 4, and subtract, from column 4, the cost associated with your decision number listed in column 5. Record this difference in column 6. This amount in column 6 is your earnings for the round. The earnings of your pair member is calculated in exactly the same way. If both you, and your pair member have the same total in column 3, a coin will be flipped to determine which fixed payment you receive.

After round 1 is completed, you will perform the same procedure That is, you will choose a decision number again (though of course you may pick the same one), you will draw another random number from the cage, and you will calculate a new payoff. When round 12 is completed, add your earnings from each of the rounds, and record the total earnings at the bottom of sheet 2. Subtract from this the fixed cost of $2.00. This amount will be paid to you in cash at the end of the experiment.

<u>Example of Payoff Calculations</u>

For example, say that pair member a_1 chooses a decision number of 60, and draws a random number of 10, while pair member a_2 selects a decision number of 50, and a random draw of 5.

a_1's payoff calculation will look like this:

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4			Col. 5 Minus Cost		Col. 6 Total Earnings
					X Amt.	Y Amt.				
60	+	10	=	70	[✓ $1.45]	$0.86	-	$0.36	=	$1.09

a_2's payoff calculations will look like this:

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4			Col. 5 Minus Cost		Col. 6 Total Earnings
					X Amt.	Y Amt.				
50	+	05	=	55	$1.45	[✓ $0.86]	-	$0.25	=	$0.61

Note, the amount subtracted in column 5 (decision cost), is only a function of your decision number - i.e. your random number draw does not affect the amount subtracted. Additionally, your total earnings depend on your random draw, your selected decision number (both in its contribution to your total, and the subtraction of its associated cost from your fixed payment, either X or Y), and your pair member's selected decision number, and random draw.

Subject #_____

Sheet 1 - Decision Costs Table

Column A Decision Number	Column B Cost of Decision	Column A Decision Number	Column B Cost of Decision	Column A Decision Number	Column B Cost of Decision
0	$0.0000	36	$0.130	72	$0.518
1	$0.0001	37	$0.137	73	$0.533
2	$0.0004	38	$0.144	74	$0.548
3	$0.0009	39	$0.152	75	$0.563
4	$0.002	40	$0.160	76	$0.578
5	$0.003	41	$0.168	77	$0.593
6	$0.004	42	$0.176	78	$0.608
7	$0.005	43	$0.185	79	$0.624
8	$0.006	44	$0.194	80	$0.640
9	$0.008	45	$0.203	81	$0.656
10	$0.010	46	$0.212	82	$0.672
11	$0.012	47	$0.221	83	$0.689
12	$0.014	48	$0.230	84	$0.706
13	$0.017	49	$0.240	85	$0.723
14	$0.020	50	$0.250	86	$0.740
15	$0.023	51	$0.260	87	$0.757
16	$0.026	52	$0.270	88	$0.774
17	$0.029	53	$0.281	89	$0.792
18	$0.032	54	$0.292	90	$0.810
19	$0.036	55	$0.303	91	$0.828
20	$0.040	56	$0.314	92	$0.846
21	$0.044	57	$0.325	93	$0.865
22	$0.048	58	$0.336	94	$0.884
23	$0.053	59	$0.348	95	$0.903
24	$0.058	60	$0.360	96	$0.922
25	$0.063	61	$0.372	97	$0.941
26	$0.068	62	$0.384	98	$0.960
27	$0.073	63	$0.397	99	$0.980
28	$0.078	64	$0.410	100	$1.000
29	$0.084	65	$0.423		
30	$0.090	66	$0.436		
31	$0.096	67	$0.449		
32	$0.102	68	$0.462		
33	$0.109	69	$0.476		
34	$0.116	70	$0.490		
35	$0.123	71	$0.504		

Subject #______

Sheet 2 - Payoff Record Sheet

Round 1

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4 X Amt.	Y Amt.		Col. 5 Minus Cost		Col. 6 Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 2

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4 X Amt.	Y Amt.		Col. 5 Minus Cost		Col. 6 Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 3

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4 X Amt.	Y Amt.		Col. 5 Minus Cost		Col. 6 Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 4

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4 X Amt.	Y Amt.		Col. 5 Minus Cost		Col. 6 Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 5

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4 X Amt.	Y Amt.		Col. 5 Minus Cost		Col. 6 Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 6

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4 X Amt.	Y Amt.		Col. 5 Minus Cost		Col. 6 Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 7

Col. 1 Decision Number		Col. 2 Random Number		Col. 3 Total 1 + 2	Col. 4 X Amt.	Y Amt.		Col. 5 Minus Cost		Col. 6 Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 8

Col. 1		Col. 2		Col. 3	Col. 4			Col. 5		Col. 6
Decision Number		Random Number		Total 1 + 2	X Amt.	Y Amt.		Minus Cost		Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 9

Col. 1		Col. 2		Col. 3	Col. 4			Col. 5		Col. 6
Decision Number		Random Number		Total 1 + 2	X Amt.	Y Amt.		Minus Cost		Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 10

Col. 1		Col. 2		Col. 3	Col. 4			Col. 5		Col. 6
Decision Number		Random Number		Total 1 + 2	X Amt.	Y Amt.		Minus Cost		Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 11

Col. 1		Col. 2		Col. 3	Col. 4			Col. 5		Col. 6
Decision Number		Random Number		Total 1 + 2	X Amt.	Y Amt.		Minus Cost		Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Round 12

Col. 1		Col. 2		Col. 3	Col. 4			Col. 5		Col. 6
Decision Number		Random Number		Total 1 + 2	X Amt.	Y Amt.		Minus Cost		Total Earnings
______	+	_____	=	_____	$1.45	$0.86	-	$_____	=	$______

Sum of Total Earnings Rounds 1-12 $______

Minus Fixed Cost $ 2.00

Net Earnings $______

Name____

Social Security #____

Telephone Number____

Address____

____