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An Examination Of Effort: An Experimental Approach

Alexander P. Brumlik

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AN EXAMINATION OF EFFORT: AN EXPERIMENTAL APPROACH

BY

ALEXANDER PARIS BRUMLIK

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
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in the
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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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Any errors that remain in this dissertation are, of course, my own.

This dissertation is dedicated to my parents, Patricia and Timothy Brumlik.

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ABSTRACT

AN EXAMINATION OF EFFORT: AN EXPERIMENTAL APPROACH

BY

ALEXANDER PARIS BRUMLIK

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Committee Chair: Dr. James C. Cox

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This dissertation studies issues associated with various topics related to a worker's effort. For example, I explore how different wage incentives affect a worker's productivity. I explore how exogenous shocks, what we often refer to as "luck," can affect a worker's motivation. In addition, I explore how different wage contracts destroy cooperation and can lead to destructive activities such as cheating and sabotage, as well as how these activates, in turn, affect productivity. Finally, in the last chapter, I analyze behavioral issues related to fairness and altruism in tournaments, and how these behaviors affect worker's effort.

CHAPTER I

INTRODUCTION

At the heart of all principal agent models lies the dilemma of how to properly incentivize workers. I use an experimental approach to explore how effort varies across various labor institutions. For example in the second chapter, entitled "How is Effort Affected When Luck Impacts Outcomes," I set out to explore the impact of a stochastic variable, "luck," on effort with an emphasis on competition. I find that luck severely reduces effort by approximately 17 percent which is highly statistically significant. The significance is robust using both non – parametric tests as well as regressions.

I begin the third chapter, entitled, "How Deviant Behavior Affects Effort" by recognizing that although many organizations value cooperation, many times the incentives of an organization have the opposite effect. For example, bonuses are a commonly used mechanism to motivate agents to put forth greater effort. However, this effort might not always be manifested as productive work and, at times, such effort might result in destructive activity. Given relative performance incentives, agents have a reason to cheat and/or sabotage their opponents. These activities can have a negative effect on the amount of effort put forth by workers, which results in decreased labor productivity. The paper's principal conclusion shows that despite cheating and sabotage, bonuses do increase effort over the fixed wage contract.

Having demonstrated in Chapters II and III that bonus contracts motivate workers to put in high effort, Chapter IV, entitled “Competitors with ORP,” explores the behavioral consequences of bonuses. The paper begins by taking a multi-disciplinary approach by examining different theories of motivations from economics, psychology, and sociology. Many of the traditional theories in economics rely on agents’ having selfish preferences and disregarding the actions of others. Newer economic theories referred to as other-regarding preferences incorporate the monetary payoffs of their colleagues. If agents have other-regarding preferences and there is perceived inequity, then this will create tensions or distress within the individual. And, in the long run, when workers are mobile and have employment flexibility these tensions reduces the willingness to participate in these competitive industries. The preliminary results find that, out of 108 subjects recruited in the Experimental Economics Center, 29% avoided tournaments; 17% did so to avert feelings of guilt.

CHAPTER II

HOW IS EFFORT AFFECTED WHEN LUCK IMPACTS OUTCOMES

II.1 Introduction

Incentives based on relative performance are commonly found in the workplace and sports contests. Boyle (2001) pointed out that 25% of Fortune 500 companies use forced-ranking systems. In addition, competition for promotions and bonuses are important motivating factors in many firms. Lazear and Rosen (1981) pioneered the research on relative performance with their seminal work on tournament theory, which has been adapted to explain such incentives in politics, patent races, and even courtships. In tournament models, agents compete for a fixed and known set of prizes. The agents exert effort, which increases their likelihood of winning the contest and receiving the better prize. However, despite its wide application, there is still much that economists do not know about the incentive effects of tournaments (Carpenter, Matthews, & Schirm, 2010). Ironically, the role of "luck" is not fully understood outside of the theoretical realm of research, although the pioneering work of Lazear and Rosen (1981) and others, such as Amegashie (2009) and Nalebuff and Stiglitz (1983), established that "luck"[1] is a necessary component of a tournament.

In this article, the role of "luck" and its effect on workers' effort takes center stage and is analyzed through the use of an economics experiment. Economic experiments offer a unique opportunity to examine key features of tournaments that are often impossible to analyze because they are confounded with other behaviors. The experiment collects data on how people's effort changes in a real-effort task in two different environments: (a) an environment in which their effort returns a certain outcome or score and (b) an environment in which there is a stochastic element that introduces noise between subjects' effort and score. A second purpose is to collect data on how effort changes as the wage spread varies between low and high producers.
More specifically, subjects competed in a series of six tournaments by answering arithmetic questions. For the duration of the experiment, they were randomly assigned to one of three contracts that differed with respect to the inequality of the wage spread. Theory predicts, *ceteris paribus*, that, as the wage spreads become more unequal, workers' incentive to provide more effort increases. To preview the results, the data were largely consistent with the theoretical predictions. The average treatment effect of the prize spreads was approximately a 7% increase in effort, which was statistically significant.

Every real-life competition involves some degree of luck. Good luck might be a favorable call by the referee, a strong gust of wind that keeps a tennis ball just inside the base line when it would otherwise have gone out of bounds, or simply knocking on the right door at the right time to make a sale. Of course, its evil twin, "bad luck", is never far off. These momentary shocks can immediately change the momentum of a contest and affect a contestant's

[1] Lazear and Rosen (1981) defined the term "luck" as any random factor that is beyond the agents' control, yet affects their production function and, consequently, their relative position in the contest. This article keeps with the convention.

behavior. Therefore, as in real-life tournaments, this article incorporates luck in real-time throughout the task. While subjects answered questions, the computer randomly selected certain questions to be luck questions, which added points to or subtracted points from the subjects' scores. The subjects knew the distribution of luck, which varied across treatments, but could not distinguish between luck and normal questions until they submitted their answer to the question.

Tournament theory posits that luck introduces noise between a worker's effort and the outcome, thereby diluting the incentives to perform. The effect of luck was substantial, relative to the effect of pecuniary incentives. The average effect of luck reduced effort by approximately 17%. In both the low and high luck treatment groups, the effects were highly significant.

In this article, section 2 includes a brief review of the relevant literature and the theoretical model with predictions. In Section 3, the experimental design is discussed. Finally, in section 4, the results are presented.

II.2 Relevant Literature and Theoretical Predictions

The empirical literature generally supports the idea that tournaments increase effort. Because of the availability of sufficient high-quality data, much of the early work was in the sporting arenas, such as tennis, NASCAR races, and golf. For example, Ehrenberg and Bognanno (1990) analyzed golfers from the 1984 men's PGA tour. Consistent with tournament theory, they found that the level and structure of prize money did influence player performance. *Certis paribis*, higher prizes led to lower scores, which they associated with more effort. Outside of the sports arena, Knoeber and Thurman (1994) found similar results in the broiler chicken industry. They reported that higher prizes led to better performance on the part of farmers, as evidenced by the provision of higher-quality chicken. There are two main types of empirical studies of tournament theory with luck as a variable of interest: field experiments and economic experiments.

Field Experiments

Audas et al. (2004) reviewed the personnel records at a large British financial firm and found that workers reduced their absenteeism, the proxy for worker effort, when responding to larger remuneration spreads and increased certainty (i.e., less luck) in the promotion process. In a more recent study, Delfgaauw et al. (2011) conducted a field experiment in a Dutch retail chain to test basic predictions regarding prize spreads and noise (or luck). They designed elimination tournaments with two rounds. In each round, four randomly assigned stores competed. The two stores with the highest average number of products per customer (APC) won a bonus and qualified for the second round of the tournament, whereas the bottom two stores were eliminated. In the second round, qualified stores were once again grouped based on store performance during the previous year. Findings were generally supportive of tournament theory. The average effect of the tournament increased APC by approximately 1.5%. In addition, more convex prize spreads increased performance, but the magnitude of these effects was small. Further stores that historically had relatively stable APCs had a significantly larger treatment effect compared to stores with more noisy performance.

I consider this article to be complementary to the work of Delfgaauw et al. (2011), as the general research agendas of the two studies are nearly identical; first, to investigate the relationship between prize structure and the incentive effects of tournaments and second, to test the effect of a stochastic element (i.e., luck or noise) on measured performance within a tournament setting. The methodologies, however, differed. Delfgaauw et al. (2011) employed field experiments, whereas this study utilized economics experiments. As List (2008) argued, field experiments can bridge the empirical approaches between economic experiments and naturally occurring data. Delfgaauw et al. (2011) might also agree that these methodologies complement each other, as they stated that “the effects of noise on performance in tournaments are rarely studied in experiments” (p.5). The present experiment allows for a strong degree of control in order to accurately measure the variables of interest (i.e., effort and luck) while holding other variables, such as ability and risk attitudes, constant. In field experiments, researchers give up some of this control to gain more realism. This study supports the basic conclusions by Delfgaauw et al. (2011), specifically that tournaments and increased remuneration spreads enhance effort, whereas increased luck reduces effort.

Economic Experiments

Bull et al. (1987) brought the study of tournaments to the laboratory. One of their tests was to examine how luck affected subjects’ effort choice. They induced a supply curve of effort by setting the prize structure so that the subjects increased their chances of winning by choosing a higher “decision number”. Because the decision number represented a worker’s effort, higher numbers were associated with higher costs, which were deducted from the subject’s payoff. The subject’s decision number plus a random number ranging from -40 to +40 in the low luck treatment group and from -80 to +80 in the high luck treatment group determined the subject’s production level. The subjects were grouped in pairs of two. The subject with the higher (lower) production level received the higher (lower) prize minus their associated cost of effort. In this chosen-effort experiment, the decision numbers, or effort, were similar across the low and high luck treatments.

Due largely in part to the work of Bull et al. (1987), chosen-effort elicitation methods became a standard method of analyzing tournaments within the laboratory. Another method that is more recent is the use of a real-effort task. Although there is some debate surrounding the differences between real-effort tasks versus induced valuation designs (Bruggen & Strobel, 2007), I share the views of Dijk et al. (2001) and Carpenter (2010, p. 1), who argued that real work "involves effort, fatigue, boredom, excitement, and other affections not present in chosen effort" (p. 14). Also, most researchers assume that the agents are homogeneous, with identical costs of effort and productivity. In this paper, there is no assumption about ability, as it uses a real-effort task and subjects differ with regard to ability or other non-observables.

In a more recent study, Rustichini and Vostroknutov (2007) had subjects play two games. One game involved skill,[2] whereas the other was a game entirely of luck. Their primary finding was that subjects were more willing to reduce the winnings of others' scores in the skill game over the luck game. Rustichini and Vostroknutov (2007) stated that subjects attached more importance to the relative outcome in the skill game in which outcomes were reflections of personal ability or skill than in the luck game in which the signal was uninformative. Gill and Prowse (2010) addressed the question of whether women were less competitive than men by exploring differences in how they responded to good or bad luck in a competitive environment. Although it was not a test of tournament theory, their paper had some similarities to the present one. For example, they measured effort with a real-effort task. Further, the outcome of the

[2] The game of skill was the classic two-player board game "Hare and Hounds" in which subjects had to use a combination of logic and analytical skills to beat their opponent, the computer. In addition, the luck game was simply a guessing game in which subjects had to guess a number between 0 and 100. The subject would win if he or she was less than 10 units away.

contest depended partly upon both effort and luck. However, the research agenda was different, as their primary concern was explaining gender differences in a repeated competition. They implemented luck after the subjects performed their task and then compared these effort levels to previous rounds to examine if there were gender differences in winning or losing. In this framework in which all 10 rounds[3] were paid, women responded by reducing effort more than men after losing.

Finally, Hammond and Zheng (2013) used an economics experiment to discriminate between two alternative models of optimal effort in tournaments. First, the additive model posited that a worker's output was equal to the sum of the worker's effort, ability, and luck. Second, the multiplicative model posited that output was equal to the product of the three. They focused on a key difference, namely that ability and effort are complements in the multiplicative model, but are neither complements nor substitutes in the additive model. This difference has separate implications regarding the relationship between optimal effort and tournament heterogeneity. Multiplicative models predict that effort increases as heterogeneity increases in ordinal and cardinal tournaments, whereas additive models predict that effort is unchanged as heterogeneity increases. Their results supported the conclusion that subjects do not respond to heterogeneity, thereby supporting the additive model.

Theoretical Predictions

The main theoretical model is the tournament model introduced by Lazear and Rosen (1981). Tournaments possess four important features:

1. The prizes within the tournament are fixed in advance and are based upon a player's relative performance, rather than the agents' or workers' absolute performance.

[3] There were also two practice rounds (12 total rounds).

2. It is assumed that the spread between the winner's and loser's prize affects effort.

3. There is an optimal spread.

4. Luck can determine the outcome of the tournament

Consider a simple tournament with two contestants, i and j. Each contestant produces a given output or score, s_i, according to the following equation:

$$s_i = e_i + \epsilon_i,$$

where e_i is the level of effort of player i, which is unobservable to the principal,[4] and ϵ_i is a random variable or luck component, which is i.i.d. normal. The agents have no control over luck. Further, the luck they experience can be negative, representing bad luck, or positive, representing good luck.

Assuming that the utility of each player is additively separable in the prize they receive from the tournament and the disutility of exerting effort, then the player's problem is:

$$\max_{e_i}\left[W_h Pr\left(e_i; e_j\right) + W_l\left[1 - Pr\left(e_i; e_j\right)\right] - C(e_i)\right], \qquad (1)$$

where W_h represents the fixed prize, which is contingent upon winning the contest, $s_i > s_j$, and W_l is the fixed prize for the loser. The prize spread is simply $W_h - W_l$. The cost of effort $C(\mu_i)$ is increasing and strinctly convex in effort. $Pr\left(e_i; e_j\right)$ is the probability that player i will win the contest given j's effort decision, and we can write this as:

$$\begin{aligned} Pr\left(e_i; e_j\right) &= \Pr\left(s_i \geq s_j\right) \\ &= \Pr\left(e_i - e_j \geq \epsilon_j - \epsilon_i\right) \\ &= \Pr(e_i + \epsilon_i \geq m) \end{aligned}$$

[4] This study is concerned with the agent's effort decisions. It is not concerned with the principal's profit function. Refer to Lazear and Rosen (1981) for the complete solution to the principal agent problem via tournament theory.

where $m = \epsilon_j - \epsilon_i$, given that ϵ is randomly distributed with a mean of zero and a variance of σ^2. Then, it follows that *m* also has the same properties and has a cumulative distribution function (CDF), G, with an associated probability density function (PDF), g. Then, *i*'s payoff can be rewritten as:

$$\mathrm{G}\left(e_i - e_j\right)(W_h - W_l) - C(e_i)$$

Also, the F.O.C. can be written as:

$$\mathrm{g}\left(e_i - e_j\right)(W_h - W_l) = C'(e_i)$$

$$\text{or } \frac{1}{\sigma\sqrt{2\pi}} \exp\left(\frac{e_i - e_j}{\sigma\sqrt{2}}\right)^2 (W_h - W_l) = C'(e_i)$$

$$\text{or } \frac{1}{\sigma\sqrt{2\pi}} \exp\left(\frac{e_i - e_j}{\sigma\sqrt{2}}\right)^2 (W_h - W_l) = C'(e_i) \tag{2}$$

A similar equation can be written for j. In equilibrium, the marginal cost of both contestants must be equal, and as result $e_i = e_j$. And therefore (2) can be simplified so that:

$$\left(\frac{1}{\sigma\sqrt{2\pi}}\right)(W_h - W_l) = C'(e_i). \tag{3}$$

Theoretical Prediction 1: As remuneration spreads $(W_h - W_l)$ increases, subjects respond by increasing their effort.

(Proof provided in Appendix A.3)

Theoretical Prediction 2: As luck (represented by σ) increases, subjects respond by decreasing effort.

(Proof provided in Appendix A.3)

II.3 Procedures and Design of the Experiment

Protocol

The experiment consisted of two parts plus a questionnaire at the end. Two sessions were conducted at the Experimental Economics Center (ExCEN) at Georgia State University. Each session had 36 subjects who were invited via email using the center's recruitment software. The subjects were all students with varying majors and racial/ethnic backgrounds. A summary of demographic information can be found in Table II-1. Overall, there were slightly more females in the sample; most subjects were African-American. There were relatively few math or engineering majors. Further, the subjects were fairly uniformly distributed across class standing. Most subjects reported that they either did not work or worked part-time.

After signing the consent forms, the subjects were allowed to enter the laboratory and could sit at any available workstation. Instructions[5] were handed out, and subjects were given time to read them at their own pace. Then, the experimenter summarized the instructions using a PowerPoint presentation. During this presentation, assistants passed by each subject and allowed them to randomly select a mailbox key. After being shown the mailboxes, the subjects were told that only they would know their number and that all of their choices and payoffs would remain private. They were also told that the mailboxes would be wheeled outside of the laboratory and that partitions would be placed alongside the mailboxes. In addition, they were informed that they would take their money from their mailbox one at a time, while the other participants waited in the lab.

The double-blind protocol was used to ensure privacy and, more importantly, because a subject's choice of effort might differ if he or she can be directly associated with an effort level. The change might be an attempt by the subject to please the experimenter if the subject believes

[5] Included in Appendix A.1

Table II-1 Participant Characteristics

	N	Proportion (%)
Gender		
Female	38	53
Male	34	47
Age (Mean Years)	20	
Race		
African American	40	55.5
Asian	15	29
Hispanic	4	5.5
White	8	11
Other	5	7
Major		
Biological Sciences	15	21
Business (Non-Economics)	12	16.5
Economics	3	4
Education	3	4
Math or Engineering	4	5.5
Psychology	4	5.5
Social Science (Non-Economics or Psychology)	4	5.5
Other	24	33.5
Does Not Apply	3	4
Class Standing		
Freshman	19	26.5
Sophomore	18	25
Junior	18	25
Senior	14	19.5
Other	3	4
Work		
Part-Time	24	33.5
Full-Time	1	1.5
Neither	47	65

that the experimenter hoped for high effort. Suppose, for example, the subject both grows tired and wishes to curtail effort but also wants to impress the experimenter with a high score. The subject might then elect to maintain a higher effort level. This is a potential source of bias. Further, subjects might choose a different level of effort to support status-seeking behavior. Increasing one's effort improves one's chances of winning the tournament, and winning could be associated with behavioral rewards that result from some form of external recognition. The double-blind protocol is used to eliminate any external motivations and/or reputation effects stemming from the experimenter. As a result, differences across treatments should be related to changing treatment conditions, rather than originating from the experimenter.

While the subjects randomly chose their mailbox key, the experimenter told the subjects that they would each be assigned to one of three contracts. The contracts differed with regard to the wage spread between "low" and "high" prizes (see Table II-2). Also, the subjects were told that their assignment would be determined randomly. They were shown 36 balls (12 red, 12 white, and 12 orange). The balls were then put into an urn and shaken. Next, the assistants went around the room and allowed the subjects to take a single ball. Since no ball was replaced, each subject was equally likely to choose either a red, white, or orange ball. The subjects were told to keep the ball hidden from view. No one, including the experimenter, knew which color ball corresponded to which of the three contract types; it was determined randomly later in the experiment. After each participant selected a mailbox key and a colored ball, the computer terminals became active. The subjects commenced by entering their mailbox number and the color of the ball that they selected. The experiment was programmed using the Z-tree platform[6]

[6] Checks were put in place to ensure that mistakes were minimized. For example, subjects were asked to confirm their mailbox numbers and the color of their balls. Moreover, the program then made sure that no mailbox number was repeated and that they fell in the range of 1-36. Similarly, the computer counted to make sure that there were 12 balls of each color. In the event that an error occurred, such as a repeated number, those subjects were

Table II-2 Possible Contracts That Differ with the Wage Spread

Contracts	Wage for Subject with Higher Score, W_h	Wage for Subject with Lower Score, W_l	Wage Spread $\Delta W = W_h - W_l$
Low Incentive	\$10	\$10	\$0
Medium Incentive	\$14	\$6	\$8
High Incentive	\$18	\$2	\$16

(Fischbacher, 2007). The subjects did not have an incentive to mislead the experimenter, as they did not know the matching of the ball color to their corresponding contract type. However, they did have an incentive to provide accurate reports, as they were told that their payment would otherwise not be accurate. After they entered this information, the experimenter then showed three balls, one of each color, and placed them into the urn. The order in which the experimenter pulled a ball from the urn determined which colored ball corresponded to which contract. The first color corresponded to contract 1, the second corresponded to contract 2, and the third corresponded to contract 3.

Experimental Design

Part 1. Subjects participated in six individual two-player tournaments. Each tournament lasted 6 minutes for a total of 36 minutes. Subjects competed by answering arithmetic questions. This real-effort task is traditionally used in economic experiments (Niederle & Vesterlund, 2007;

taken to another screen that indicated the problem and prompted the subjects to make a change. In the event that this occurred, the experimenter would have approached the subjects in question, but this action was not necessary in either session.

Eriksson et al., 2008; Cason et al., 2010; and Hammond & Zheng, 2013). It has been shown to be gender neutral (Bouville, 2008). Because arithmetic is learned at childhood, subjects' ability to answer arithmetic questions should not significantly change over the experiment. Further, arithmetic is presumed to require effort and concentration, rather than luck.

During each tournament, the subjects had a total of six minutes to answer the questions involving addition at their own pace. The subjects were given pencils and scratch paper, although they were forbidden from using a calculator or any other electronic device. Once the contests began, the subjects saw their first question on their respective computer terminal. After entering their answer using the keyboard and clicking submit, a new question appeared. The subjects earned one point for correct answers and lost a point for incorrect answers.

Most theoretical models presume that effort costs grow at an increasing rate; therefore, to incorporate this feature during the first two minutes of the tournament, the computer generated questions that involved the addition of 2 numbers ranging from 00 to 100. In the following two minutes, the questions became more difficult as the computer added a third two-digit number. Finally, in the last two minutes, the computer included a fourth two-digit number. This may be the first experiment that incorporates increasing marginal costs in this fashion into a real-effort task.

The subjects were randomly grouped into one of three possible contracts (see Table II-2). The contracts differed with regard to the disparity between the "high" and "low" wage; however, the total amount of the prizes always equaled $20. The subjects remained in their assigned contract throughout the experiment (all six tournaments). The subject with the highest score at the end of the round won that tournament. The subjects earned one point for each correct answer and lost a point for each incorrect answer. The wage spread, therefore, became the driving force for this incentive mechanism. For example, if a subject were assigned to the "low incentive" contract, then he or she would earn $10, regardless of the score. However, in the "high incentive" contract, the wage spread was $16. Consequently, subjects had a monetary incentive to win and, thus, theoretically a greater incentive to provide more effort.

To mitigate framing effects, neutral language was used in the instructions and during the experiment. The experimenter refrained from using words, such as "winner", "loser", "contests", and "tournaments". For example, the instructions stated, "You will receive the high prize if this round is selected." The word "round" was used to refer to each of the six tournaments. Further, only one round, which was randomly selected at the end of Part 1, was used for payment. Otherwise, as Cox et al. (2012) pointed out, it would open the door for wealth effects. In an experimental context, wealth effects arise when payments (or anticipated payments) from earlier rounds impact decisions in later rounds. Had all tournaments been paid, as contestants proceeded through the experiment, they would have known how much money they had earned from each previous round, thereby placing the subjects on different utility curves and possibly altering their effort choices. Due to the independence axiom, paying only one round theoretically kept each round independent of all other rounds in terms of wealth positions. Therefore, to control for wealth effects, one tournament was randomly selected for payment.

Treatments

Luck was introduced across tournaments, and there were three variations (see Table II-3). The first tournament was used as the baseline treatment; therefore, there was no luck. In the second and third tournaments, luck was present. The subjects were clearly told during the instruction

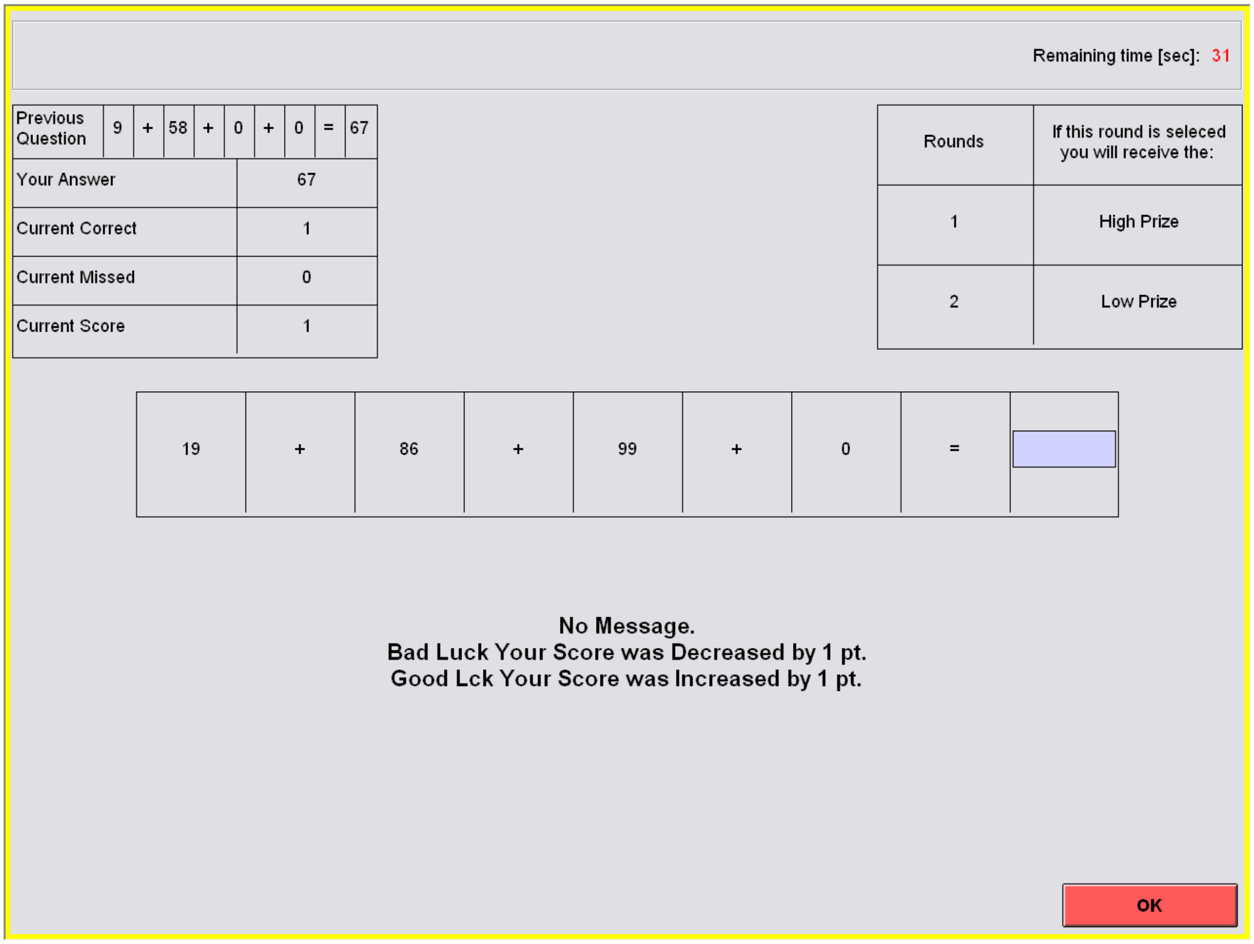
Table II-3 Three Luck Environments

Contracts	Told Probability	Actual Probability
No Luck	p = 0.00	p = 0.00
Low Luck	p = 0.20	p = 0.21
High Luck	p = 0.33	p = 0.32

period that luck would be a factor in some rounds. Luck would affect the subjects' score, depending upon the degree of luck in that tournament, as the computer would select $\propto$ questions to be luck questions; consequently, $(1-\propto)$ questions would be the normal questions. Normal questions would be scored based solely on whether the subject answered the questions correctly (+1) or incorrectly (-1). For example, in the second tournament, in a "high luck" treatment, 33% of the questions would be influenced by luck. Luck-influenced questions had an equal chance of being "good luck" or "bad luck". Good luck always improved the subjects' scores. That is, they received one point if the question was answered incorrectly and two points if the question was answered correctly. Equally likely, the question could be a bad luck question, and the subject would lose a point if the question was answered correctly and two points if the question was answered incorrectly.

The subjects were aware of the distribution of luck before the round. In addition, they were informed that everyone would be in the same luck environment. That is, the allocation of luck in a given tournament would be the same for each subject. However, the subjects could not infer if a question was selected to be a luck question or a normal question until after answering the question. If the question they just answered on the previous screen was a luck question, then

Figure II-1 Display of Real-Effort Task



a message would appear on the subsequent screen notifying them that the question was a good- or bad luck question and that they had just earned or lost points, respectively. If the previous question was a normal question, then no message would appear.

As shown in Figure II-1, the screen also contained information regarding the subject's progress within the round. In the top left corner, a box displayed the previous question with the correct answer, as well as the submitted answer, the number of questions the subject answered correctly and incorrectly, and the subject's current score. In the top right corner, the remaining time counted down in seconds. Below the time, a history box summarized the outcomes of the previous tournaments.

After the six minutes elapsed and the tournament ended, a summary screen would display the subject's progress, as well as that of his or her opponent. The information displayed the number of correct and incorrect answers together with the subject's current score and the score of his or her opponent. Also displayed was the prize that each subject would earn if this round was selected for payment. In the event of a tie, the computer would randomly select one of the subjects to obtain the high prize, leaving the other contestant with the low prize.

At the commencement of the experiment, all of the subjects entered their mailbox numbers and ball color. They also answered two arithmetic questions before a practice round began. This practice round replicated an actual round without luck, although it only lasted for two minutes, rather than the usual six. This round gave subjects hands-on experience using the software, typing in answers, and clicking "submit". At the end of the practice round, time was provided for questions. However, for the most part, the subjects were eager to start. All of the subjects in both sessions were given the following sequence of six tournaments: no luck (NL1), high luck (HL1), low luck (LL1), high luck (HL2), low luck (LL2), and no luck (NL2).

Cox and Oaxaca (1989) highlighted the importance of controlling for order effects in a within-subject design. The first objective they discussed was keeping the experiment short enough to avoid exceeding the attention span (or patience) of the subjects. As in their experiment, this objective was extremely important in the present study to prevent bias in the effort results. If the subjects had begun to feel bored, tired, or impatient, they might have reduced the number of questions they answered or began making more errors. Either way, this would be reflected negatively in their scores. A second[7] objective described by Cox and Oaxaca (1989) was repeating the baseline and treatments to see if the subjects yielded similar scores. Similar scores would indicate that there were no learning and sequence effects. With practice, the subjects could potentially "learn" by improving either their arithmetic ability or the speed at which they submitted answers. This learning could increase their scores without necessarily increasing their effort, thereby biasing the results. Alternatively, sequence effects could stem from an intrinsic motivation to improve one's score. Because the subjects knew[8] their score, competitive individuals might have increased their level of effort in later rounds to outperform themselves.

Measuring effort.

The production model used in this study is additive and has support from Hammond and Zheng (2013):

$$s_i = \mu_i + \epsilon_i \tag{1}$$

In the no luck treatments, ϵ was assumed to be close to zero. Therefore, subjects' scores in this treatment equaled their effort, $s_i = \mu_i$. In the luck treatments, effort was found by rearranging (1) and obtaining $\mu_i = s_i - \epsilon_i$. The computer tracked all subjects' scores and the net luck,

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[7] Cox and Oaxaca's (1989) second and third objectives were to repeat the baseline trials and to run the baseline and treatments in pairs to enhance internal validity. These objectives increased the total number of trials to eight. Although this was appropriate in their job search experiment, this would have tested the patience of most of the subjects in my experiment, especially given that there was still another task to complete in Part 2 plus a questionnaire.

[8] A great deal of consideration was given to whether to inform subjects of their individual scores. However, it was decided that it was in the best interest of the experiment to make this information available, as some students would be keeping track of their own scores. Moreover, given that many of the subjects' arithmetic mistakes (for which they lost a point) most likely would otherwise go unnoticed, their reported scores would be higher than their actual scores graded by the computer. The computer indicated their answer and the actual answer, as well as whether the subjects' answer was correct or incorrect. By showing their scores in real time, it minimized the chance that a subject would believe that the experimenter was utilizing deception.

which was the amount of good luck minus bad luck. Therefore, effort was measured by simply subtracting net luck from a player's score at the end of each tournament.

Part 2. Following the completion of the tournaments and learning which of the six rounds in which they would be paid,[9] the subjects participated in a lottery choice experiment (LCE), which is often referred to as the Holt and Laury procedure (2002). The experiment was designed to elicit a measure of risk aversion. This mechanism is regarded by many as the "gold standard" (Anderson, Freeborn, & Hulbert, 2012). It is a commonly used tool in experimental economics because it is simple to implement and explain to subjects. Further, results of several studies have supported the external validity of the Holt and Laury (2002) task by linking the measures to real-world behaviors (Anderson & Mellor, 2008). Harrison et al. (2005a) showed that the task elicited risk preferences that were stable over time.

Theoretically, tournaments incentivize workers to provide costly effort upfront, which increases the likelihood of their outperforming their opponent and receiving the better prize. However, given that increasing effort only enhances the workers' expectation of winning a fixed prize, the workers incur risk. Their effort might be for naught, as they do not know with certainty how much their opponent will produce and, consequently, whether they will win the contest. Further, because some workers have different attitudes toward risk, it is possible that risk attitudes might affect the subjects' effort choice in the experiment. To control for this possibility, the experiment included an LCE.

In the LCE, subjects were presented with 10 choices between Lottery A and Lottery B (see Figure II-2). Lottery A was the relatively safe lottery because it had less variability between the payoffs than Lottery B. For example, in the first row labeled, Decision Q, Lottery A offered

[9] Cox (2010) and Cox et al. (2012) investigated portfolio effects and found evidence for them in their data when all choices were paid sequentially at the end of the experiment. The purpose of revealing subjects' payoffs was to eliminate the possibility that they could form a portfolio across both parts of the experiment.

Figure II-2 Screenshot from the Lottery Choice Experiment

Decision From Bingo Ball	Option A From Die	Option B From Die	Please make your Decisions
Q Ball = 5	$6.00 if the die is 1 or $4.80 if the die is 2-10	$11.55 if the die is 1 or $0.30 if the die is 2-10	Option A Option B
R Ball = 3	$6.00 if the die is 1-2 or $4.80 if the die is 3-10	$11.55 if the die is 1-2 or $0.30 if the die is 3-10	Option A Option B
S Ball = 8	$6.00 if the die is 1-3 or $4.80 if the die is 4-10	$11.55 if the die is 1-3 or $0.30 if the die is 4-10	Option A Option B
T Ball = 9	$6.00 if the die is 1-4 or $4.80 if the die is 5-10	$11.55 if the die is 1-4 or $0.30 if the die is 5-10	Option A Option B
U Ball = 1	$6.00 if the die is 1-5 or $4.80 if the die is 6-10	$11.55 if the die is 1-5 or $0.30 if the die is 6-10	Option A Option B
V Ball = 10	$6.00 if the die is 1-6 or $4.80 if the die is 7-10	$11.55 if the die is 1-6 or $0.30 if the die is 7-10	Option A Option B
W Ball = 2	$6.00 if the die is 1-7 or $4.80 if the die is 8-10	$11.55 if the die is 1-7 or $0.30 if the die is 8-10	Option A Option B
X Ball = 4	$6.00 if the die is 1-8 or $4.80 if the die is 9-10	$11.55 if the die is 1-8 or $0.30 if the die is 9-10	Option A Option B
Y Ball = 7	$6.00 if the die is 1-9 or $4.80 if the die is 10	$11.55 if the die is 1-9 or $0.30 if the die is 10	Option A Option B
Z Ball = 6	$6.00 For Sure	$11.55 For Sure	Option A Option B
			Submit

a 10% chance of receiving $6 and a 90% chance of receiving $4.80. Lottery B had a 10% chance of receiving $11.55 and a 90% chance of receiving $0.30. The presumption was that only risk-loving subjects would chose Lottery B in the first decision because the expected value of Lottery A was $4.92 compared to an expected value in Lottery B of $1.82. When the subjects advanced to the next decision, the probabilities of the lotteries changed in a predictable and set manner (or pattern). The likelihood of the higher payoff increased by 10%. This resulted in the expected value of both lotteries to increase, but the expected value of Lottery B increased more.

The structure was designed so that the expected value of Lottery A was greater than that of Lottery B for the first four choices, Decisions Q - T (see Figure II-2). The expected value of Lottery B was greater than that of Lottery A in the remaining choices, Decisions U - Z. Therefore, subjects were classified as risk-neutral if they selected Lottery A for Decisions Q - T and then permanently switched to Lottery B on Decision U. Similarly, subjects were classified as risk-averse if the switch occurred after Decision U. The last choice, Decision Z, was a test to make sure that the subjects understood the instructions because they could choose Lottery B and receive a sure payoff of $11.55.

There were two deviations in this LCE from the original Holt and Laury (2002) design. First, the payoffs were three times the baseline amounts of Holt and Laury (2002); however, these higher payoffs were used in Anderson & Mellor (2008) and Anderson et al. (2012). The higher payoffs were chosen to keep the cost-to-effort ratio relatively constant between Parts 1 and 2 of the experiment and to increase the average amount of payoffs for subjects, which was an average of $19.19 per subject.

The second difference involved the protocol. In Holt and Laury (2002), two dice were used. The first die randomly determined which of the 10 lottery choices would be selected for payment. The second die was used to determine the payoff from the lottery. In this LCE, each of the 10 lottery choices was associated with a letter (Q - Z) and a corresponding bingo ball numbered 1 - 10. As shown in Figure II-2, each subject's table displayed the 10 decisions (Q - Z), thus preserving the order of the task and the required pattern to determine the switching point between Lottery A and Lottery B. However, the bingo ball number associated with each letter differed across subjects. Therefore, when the experimenter revealed the outcome of the bingo ball number, *ex post*, the matching letter and lottery choice were not the same for each subject.

This technique minimized the variance between payment possibilities and helped the experimenter with budgetary considerations. A bingo ball was used in this technique to minimize subject confusion during the instruction period. All of the other features of this LCE were identical to those in Holt and Laury (2002).

The experiment concluded with a questionnaire that contained both demographic questions and measures of competitiveness. The Revised Competitiveness Index (Houston, Harris, McIntire, & Francis, 2002) was used to assess competitiveness. This index contained 14 items using a 5-point rating scale. Responses ranged from strongly disagree to strongly agree. A copy of the questionnaire is attached in appendix, A.2. Nine of the items are directly related to the enjoyment of competition, whereas the five remaining items measure contentiousness. Using the responses to all 14 questions, one can obtain an overall measure of competitiveness. Houston et al. (2002) reported that the enjoyment of competition measure (9 items, $\alpha = 0.90$) and the overall measure of competitiveness (14 items, $\alpha = 0.87$) had acceptable internal reliability. Harris and Houston (2010) conducted a test-retest of the index. They found that the index replicated the results and were positively correlated with other competitive measures.

Although this might be the first study to use the Revised Competitiveness Index in economics, researchers in the field do rely on self-report questionnaires that employ subjective well-being questions. Veenhoven (1993) had a detailed classification concerning well-being questions, wording, and responses. The reason for incorporating this measure in this study was that it was anticipated that highly competitive subjects might improve their performance in later tournaments stemming from an internal motivation to surpass their previous scores.

II.4 Results

Contracts

Figure II-3 supports prediction 1 that subjects' effort will increase as the wage spread becomes more unequal. Each bar represents 144 observations (the average effort of 24 subjects across 6 tournaments). The figure reveals two important findings. First, subjects exerted a large amount of effort in the low incentive contract in which each subject earned $10, regardless of the score. This condition was analogous to a fixed wage. Most economic theories that assume a model of "economic man[10]" posit that effort will be zero because the payoff is guaranteed and the costs of answering questions are positive. However, as can be seen in effort is quite high in the low incentive contract. Numerous theories attempt to explain why subjects might provide effort when they are not financially rewarded. Nevertheless, discriminating among these theories is not the goal of this research. Instead, the experiment was designed to minimize intrinsic motivations, although they are usually present in real-world contests, to obtain cleaner observation of the variables in questions (i.e., the role of luck). For these reasons, the design of the experiment was double-blind. In addition, specific items were included in the questionnaire regarding enjoyment of competition.

The second noteworthy observation is that effort increased with the wage spread slightly. The average effect of the tournament versus the fixed wage was a 7.3% increase in effort. To test the null hypothesis that the mean effort levels would be the same, pairwise comparisons were conducted using two sample Kolmogorov-Smirnov (K-S) tests.[11] In the first comparison between low incentive and medium incentive,

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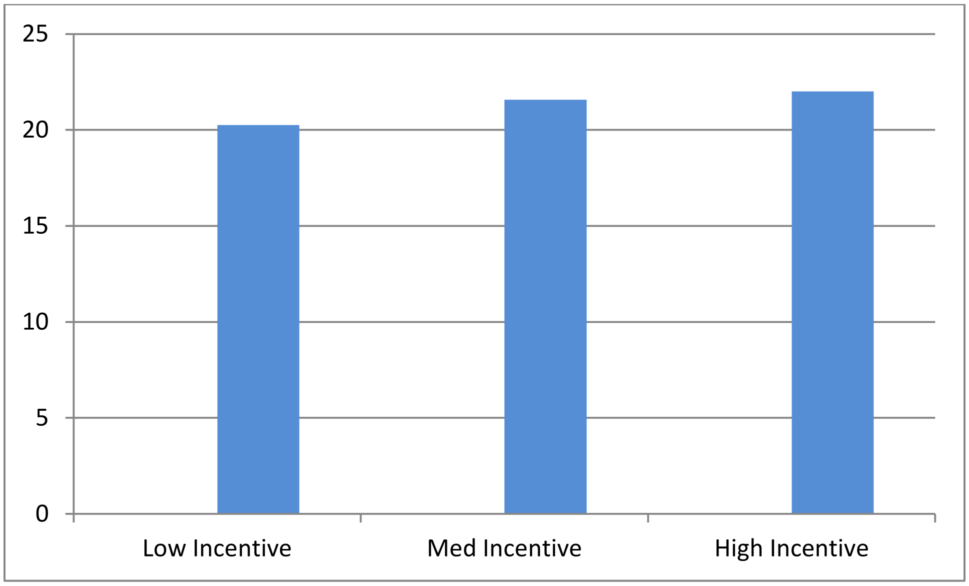
[10] Homo economicus" is a model that refers to an individual who acts entirely out of self-interest to maximize his or her own utility (i.e., payoff).

[11] Two-sample Kolmogorov-Smirnov (K-S) tests is a non-parametric test to determine whether two underlying probability distributions differ. T-tests were also run. T-tests assume that the means are normally

Figure II-3 Average Effort Induced by Incentive Contract



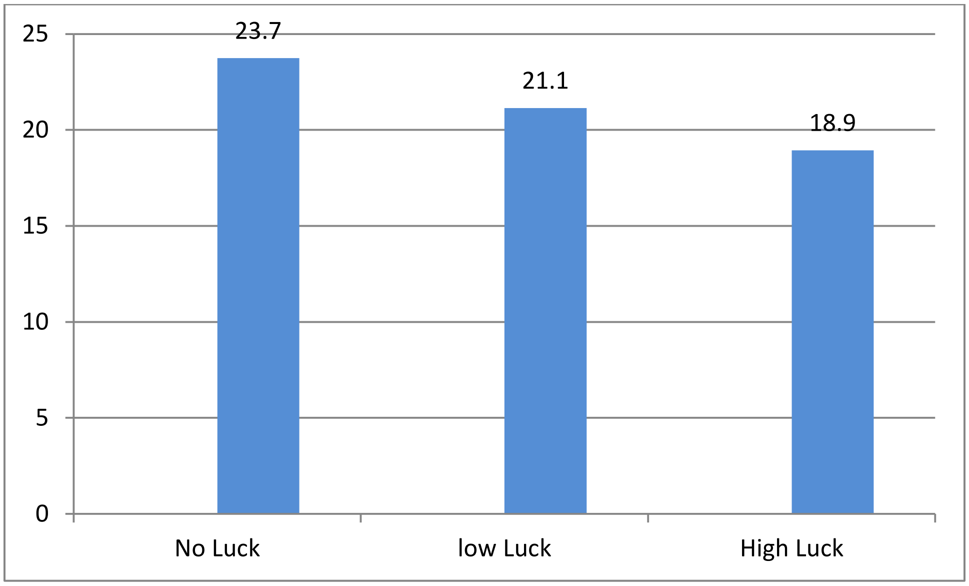
the null hypothesis was rejected (α = 0.05). The second comparison between medium incentive and high incentive was not rejected at conventional levels. Finally, for the comparison between low incentive and high incentive, the null was rejected (α = 0.01).

Luck

In the vast majority of studies on tournament incentives, luck is a component of the theoretical model or part of the design; however, only a few researchers have attempted to directly identify its role and effect on effort. Figure II-4 shows that some researchers may have underestimated luck's function; it appears to have had a significant role in determining effort in tournaments. When scores depend entirely upon whether subjects answer questions correctly or incorrectly without any exogenous shocks, effort levels are at their highest. In tournaments with

distributed. Kernel densities were performed, which appeared to be normal, and the T-test results were consistent with the KS results reported above.

Figure II-4 Average Effort across each Luck Environment



a stochastic element, effort levels fall and continue to do so as the variance in the noise increases. KS tests[12] were used to compare the means between the three luck treatments (no luck vs. low luck, low luck vs. high luck, and no luck vs. high luck). All three combinations were strongly rejected ($\alpha < 0.01$). According to these experimental data, the effect of luck is stronger than the pecuniary incentives. The difference in effort levels between tournaments with luck versus those without luck were larger than the differences in effort between the fixed wages versus tournaments. For example, the average effect of luck (23.7 versus 20) is a 16.9% decrease in effort.

[12] T-Tests were performed as well, and Kernel density show normality.

Linear Regression

These results were also confirmed by the regressions reported in Table II-4. Model (1) applies a linear regression with robust standard errors to control for heteroskedasticity. The dependent variable was subjects' effort levels. The baseline was the no luck treatment with the low incentive, and the other incentives were coded as dummy variables. What became apparent from constructing this test was that the coefficients increased with the wage spread. However, the difference between the medium incentive and high incentive was extremely small. These monetary incentives had a relatively minor impact compared to how luck and effort interacted. The coefficients on both luck variables were negative and highly significant.

Given that this experiment used a within-subject design, and the data came from subjects participating in six separate tournaments, the subjects most likely formed a cluster sample. That is, the efforts from each subject were likely to be correlated. Therefore, model (2) used a linear regression with robust standard errors, but also applied cluster samples. In this model, all of the coefficients remain unchanged;[13] however, the standard errors of the monetary incentives increased and the errors on luck decreased. In model (3), a fixed-effects model was used. Because the subjects remained in the same contract throughout the experiment, the effect of the contracts was omitted. The coefficients on luck remained the same, and the standard errors were virtually identical to model (2). These three tests supported prediction 2, luck became more important as effort was crowded out.

[13] This is consistent with autocorrelation. Autocorrelation does not bias the coefficients in a linear regression, although it does cause the error terms to be underestimated.

Table II-4 Linear Regression with Effort as the Dependent Variable

Dependent Variable	(1) Effort	(2) Effort	(3) Effort	(4) Effort	(5) Effort
Medium Incentive, Δ = $8	1.326	1.326		1.562	1.562
	(0.862)	(1.843)		(0.962)	(2.109)
High Incentive, Δ = $16	1.757**	1.757		1.581*	1.581
	(0.888)	(1.961)		(0.940)	(2.097)
Low Luck	-2.597***	-2.597***	-2.597***	-2.60***	-2.60***
	(0.904)	(0.472)	(0.471)	(0.899)	(0.474)
High Luck	-4.799***	-4.799***	-4.799***	-4.799***	-4.799***
	(0.855)	(0.464)	(0.463)	(0.851)	(0.466)
Enjoyment of Competition				0.069	0.069
				(0.518)	(0.114)
Risk-Averse				-0.622	-0.622
				(1.003)	(2.258)
Risk-Loving				-1.270	-1.270
				(1.334)	(2.960)
=1 if Female				-1.263	-1.263
				(0.774)	(1.686)
Constant	22.715	22.715	23.743	21.508	21.508
	(0.870)	(1.521)	(0.279)	(2.352)	(5.095)
R^2	0.078	0.078	0.241	0.093	0.093
Observations	432	432	432	432	432
Autocorrelation	No	Yes	Yes	No	Yes
Heteroskedasticity	Yes	No	Yes	Yes	No

Notes: ***, **, * denote statistically significant effects at the 1%, 5%, and 10% level, respectively.

The regressions that controlled for heteroskedasticity and autocorrelation were repeated in models (4) and (5), respectively. However, they also included control variables for enjoyment for competition, risk preferences, and gender. As explained above, the measure of enjoyment for competition came from the Revised Competitive Index. Women had a mean score of 32.95, whereas men had a mean score of 36.62, implying that men had a greater taste for competition. Although these averages were similar to those found in Harris and Houston (2010), who reported mean scores of 31.38 for women and 36.04 for men, this variable had little effect on subjects' effort. Moreover, enjoyment of competition was not statistically significant.

The results from the LCE are discussed next. As is the custom when using this technique, the subjects were categorized based on the number of safe choices.[14] When a subject chose fewer than four safe choices, he or she demonstrated a positive disposition toward risk and was categorized as risk-loving. A subject was categorized as risk-neutral when he or she picked exactly four safe choices. Finally, a subject was classified as risk-averse when he or she chose more than four safe choices. For comparison, the proportion of subjects in each category is shown in Table II-5 alongside the results reported by Holt and Laury (2002) and Anderson et al. (2012). Table II-5 indicates that the overall distribution was similar across the three experiments and that the majority of subjects were in the risk-averse category. However, in the estimations from models (4) and (5), as shown in Table II-4, risk preferences were not found to be statistically significant.

Table II-5 Proportion of Subjects' Risk Classification by Study

Classification	LCE	Holt and Laury (2002) (Low Payoff Treatment)	Anderson et al. **(2012)**
Risk-Loving	12.5%	8%	7%
Risk-Neutral	25.0%	26%	19%
Risk-Averse	62.5%	66%	73%

[14] Other techniques are based on when the subject switched from safe choices to risky choices. However, this requires rules on how to treat multi-switchers and subjects who begin with the riskier choices and then switch to safer ones.

The last control variable was gender. Addition questions were chosen for this design specifically because gender is not supposed to influence performance. This result was confirmed in the first tournament, which had no luck. Without luck, effort was assumed to be equal to the subjects' scores. In this tournament, men had a mean score of 22.2, whereas women had a mean score of 22.1. The difference was not statistically significant. However, as the experiment continued, women's effort levels decreased compared to men's. This reduction can be shown in models (4) and (5), as the coefficient of women was negative. The p-value in model (4) was 0.103, which was close to being significant, but it might have been biased due to autocorrelation. In model (5), gender was not found to be significant.

Scores

According to the theoretical model, subjects' scores are a combination of their effort plus the net effect of luck. Therefore, the scores should be highly correlated with effort and simply be a noisier measure of effort. As a robustness check, the regressions were repeated in Table II-6, but this time the dependent variable was the subjects' final scores at the end of each tournament. What became immediately apparent from the estimations in columns (1) and (2) was that the magnitudes of all coefficients, with the exception of the effect of the medium incentive, decreased. Also, the standard errors on every item were somewhat larger. As a result, the coefficient of high incentive lost significance. However, the standard errors of luck also increased and remained highly significant. This finding is consistent with those of field experiments in which researchers found stronger effects of luck than prize spreads. For

Table II-6 Regressions with Final Score as the Dependent Variable

Dependent Variable	(1) Scores	(2) Scores	(3) Scores	(4) Scores	(5) Scores
Medium Incentive, Δ = $8	1.340	1.340		1.549	1.549
	(0.944)	(1.913)		(1.045)	(2.194)
High Incentive, Δ = $16	1.472	1.472		1.270	1.270
	(0.950)	(2.000)		(1.00)	(2.13)
Low Luck	-2.486***	-2.486***	-2.486***	-2.486***	-2.486***
	(0.938)	(0.504)	(0.503)	(0.935)	(0.506)
High Luck	-4.535***	-4.535***	-4.535***	-4.535***	-4.535***
	(0.940)	(0.612)	(0.610)	(0.936)	(0.615)
Enjoyment of Competition				0.075	0.075
				(0.057)	(0.119)
Risk-Averse				-0.427	-0.427
				(1.084)	(2.344)
Risk-Loving				-1.104	-1.104
				(1.459)	(3.13)
=1 if Female				-1.092	-1.092
				(0.845)	(1.739)
Constant	22.806	22.806		21.182	21.182
	(0.893)	(1.54)		(2.560)	(5.346)
R^2	0.059	0.059		0.070	0.070
Observations	432	432		432	432
Autocorrelation	No	Yes	Yes	No	Yes
Heteroskedasticity	Yes	No	Yes	Yes	No

Notes: ***, **, * denote statistically significant effects at the 1%, 5%, and 10% level, respectively.

consistency, the fixed-effects model was included in column (3). Further, controls were added in estimations (4) and (5). Nevertheless, there were few changes across the different estimations.

Luck by Tournament

One of the major benefits of this experiment was that it was a within-subject comparison, which associated effort within the same subject across multiple treatments. Within-subject comparisons control for many unobservable confounding factors, such as innate abilities.

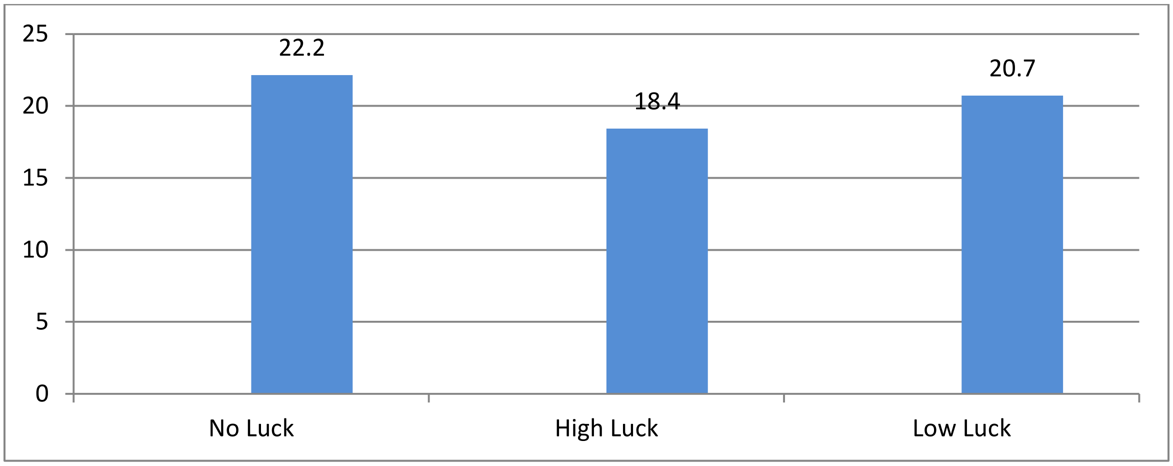
However, as noted by Schweigert (1994, p. 239), “If within-subjects designs were flawless, no one would use a between-subject design” (p. 239). Because subjects repeated the same task, they might have grown fatigued; consequently, their effort might have decreased. Then again, scores might have increased if learning occurred. To control for learning, subjects participated in two types of practice rounds. First, they had to answer a two-question addition quiz during the instruction and presentation portion of the experiment. Second, subjects participated in a two-minute practice round that replicated the task, except they could not earn money. This mitigated the learning bias by the time the tournaments commenced.

Order effects are always a potential concern when using a within-subject design. To examine if order effects biased effort levels, in Figure II-5, subjects’ effort levels were divided by treatment. Panels A and B showed the mean effort from tournaments 1 - 3 and 4 - 6, respectively. This was the order in which all subjects completed the experiment. Panel A began with the baseline treatment and had no luck. Moreover, it showed the highest level of effort. Next came the high luck tournament, which resulted in subjects’ providing the lowest effort. The low luck tournament followed in which subjects responded by raising their effort, but not to the levels in the no luck tournament.

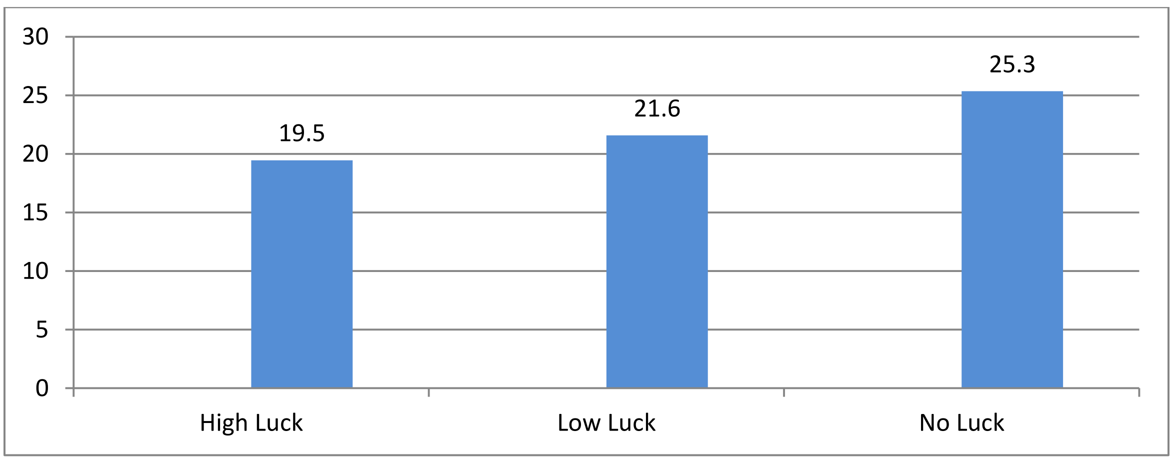
In panel B, the environments started off with the high luck tournament before certainty was introduced across the tournaments. In each case, as theory would predict, effort increased. In Panel A and B of Table II-7, shows the corresponding t-tests[15] for the pairwise comparisons. In all six comparisons, the null hypothesis was rejected. Finally, in panel C of Figure II-5, each tournament was compared with its pair. For example, when matching the first no luck tournament with the second no luck tournament, the second showed a greater level of effort. Both no luck tournaments (columns 1 and 2) were higher than any other tournament. However,

[15] Kernel density showed normality. K-S tests were performed as well.

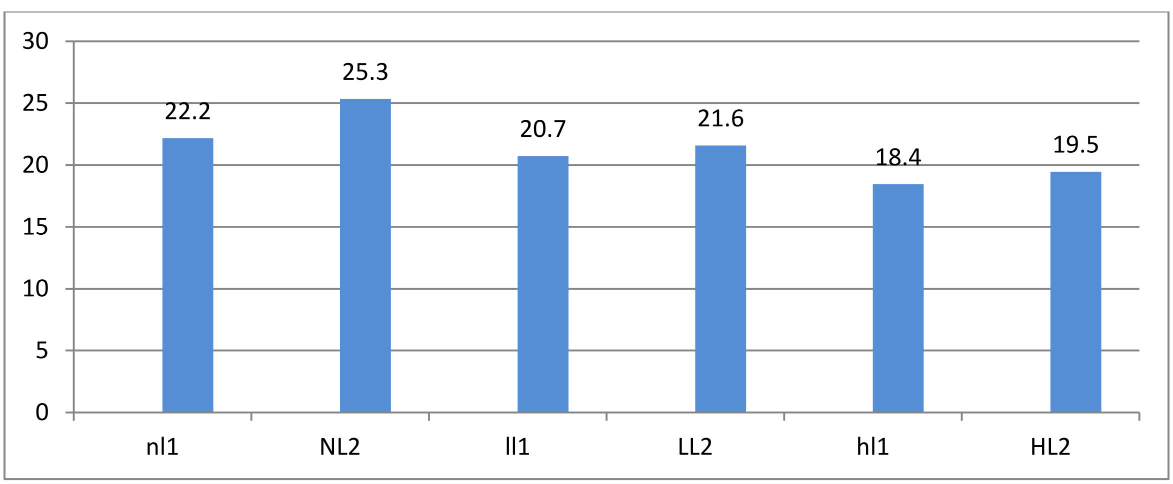
Figure II-5 Panel A, Average Effort as influenced by luck in Tournaments 1 – 3.



Panel B, Average Effort as influenced by luck in Tournaments 4 – 6.



Panel C, Luck Environments Paired



the effort levels were significantly higher in the second no luck tournament than in the first. In each pair, the second tournament was always somewhat higher than the first, indicating that some learning occurred. Yet, the pattern of the tournaments and its effect on effort was always the same. This is easiest to observe in panel C of Figure II-5. The high luck tournaments had the lowest effort levels, regardless if one compared the first or the second tournament. Therefore, the low luck tournaments were in the middle. This pattern demonstrated that, although learning occurred and effort levels increased, the effect of luck on effort was still clearly observable and as predicted by tournament theory. In panel C of Table II-7, the t-tests of each pair of tournaments are shown. The t-tests show that the effort levels in both of the no luck tournaments and both of the low luck tournaments are statistically different. While in the high luck treatments the subjects' effort levels were not statistically different. Again this indicates that learning may have occurred; but this does not negate the pattern that is shown in Figure II-5.

Table II-7 Pairwise Comparisons of Mean Effort by Tournament Rounds

	No Luck	Low Luck	High Luck
A. Rounds 1 -3			
No Luck	--	α = 0.016	α = 0.000
Low Luck		--	α = 0.000
High Luck			--
B. Rounds 2 - 6			
No Luck	--	α = 0.000	α = 0.000
Low Luck		--	α = 0.000
High Luck			--
C. Across Same Treatments			
No Luck	0.000	--	--
Low Luck		0.062	--
High Luck			0.160

Heterogeneous Subjects

Next, there might have been differences in how overall performance interacted with the variable of interest, luck. For example, high performers who achieved relatively high scores were most likely to be the subjects who had the highest effort costs. In addition, they were likely to be the ones who were influenced the most by the monetary incentives and luck. To test the hypothesis that monetary incentives and luck may affect subjects differently based on overall

Table II-8 Grouping Subjects by Level of Performance

Three Quintiles	Frequency Number of Subjects	Percent	Mean Score
Low Performers	157	36.34	13.5
Middle Performers	143	33.10	21.4
Strong Performers	132	30.56	30.8
Total	432	100.00	

performance, the sample was first divided into three subgroups based on the scores of all subjects (see Table II-8).

The deviance in the scores due to the different contracts was low. For example, in Table II-9, the average difference between low incentive and medium incentive was -0.42[16]. The negative implies that the average scores of low performers were higher with a fixed wage than with a small wage spread. This result is a bit puzzling because it occurred across all three subgroups; however, the magnitudes were small and not statistically significant. Then, for larger wage spreads, the scores increased as expected. The last row in panel A shows the difference between the low incentive and high incentive. Again, the differences were quite small because of the sign change and because the differences between each incentive were quite small. The conclusions from this test are that monetary incentives affect low performers and high performers in a similar manner, while middle performers react negatively towards tournaments.

[16] - 0.42 is the mean average of -0.50, -0.49, and -0.28

Table II-9 Average Score by Performer Type

	Low Performers	Δ	Middle Performers	Δ	High Performers	Δ
A. Contract						
Low Incentive	13.56		21.62		30.82	
		-0.50		- 0.49		-0.28
Medium Incentive	13.06		21.13		30.54	
		0.74		0.40		0.53
High Incentive	13.8		21.53		31.07	
Total Δ		0.24		-0.09		0.25
B. Luck						
No Luck	14.64		21.43		31.93	
		- 0.68		-0.09		- 1.89
Low Luck	13.96		21.34		30.04	
		-1.49		-0.93		-0.10
High Luck	12.47		20.41		29.94	
Total Δ		-2.17		-1.02		-1.99

Panel B shows that all performers' scores decreased as luck increased. The magnitudes of these changes are greater than the changes from the monetary incentives found in Panel A. This is consistent with the general finding that subjects reacted more to luck rather than pecuniary effect. Yet, middle performers reacted less compared to low performers and high performers. This pattern is similar to the one found in Panel A, and suggests that low and high performers react stronger to changes in tournament conditions than middle performers. Since performance reacts negatively across all performance levels, the author's recommendation is that managers should increase certainty within their performance structure (i.e. promotion process and bonuses).

II.5 Conclusion

The purpose of this study was to gain a greater understanding of tournaments as they relate to luck. Luck is a necessary component of tournaments. As Lazear and Rosen (1981, p. 845) noted, “Contests are feasible only when chance is a significant factor”. All theoretical investigations take this into account, yet few[17] empirical studies have devoted their attention to how luck affects effort. Two notable exceptions are the field experiments conducted by Audas et al. (2004) and more recently by Delfgaauw et al. (2011). This investigation is complementary to these field experiments. The primary contribution of this study is providing a cleaner test of the interaction between luck and effort, as unobservable confounding effects were mitigated. Also, the effects of luck were compared to remuneration spreads to gain insight into the relative importance of the effect of luck. Most of the findings are in line with theoretical predictions and the findings of previous studies, which adds credibility to the main results found in the paper.

[17] Relevant studies are discussed in the review of literature above.

CHAPTER III

HOW DEVIANT BEHAVIOR AFFECTS EFFORT

"Cheating is often more efficient."

Jeri Ryan

III.1 Statement of the Problem

Tournament-like rewards are frequently used in practice, with contestants incentivized through relative performance. For example, workers compete within corporate hierarchies for the limited-supply of job promotions, and salespersons and managers often compete for bonuses. Pfeffer and Sutton (1999) estimated that a quarter of the Fortune 500 companies link aspects of their employees' individual merit to relative performance evaluations—that is, forced rankings.

In sports contests, relative performance is the norm. Athletes compete to win greater prizes as they improve within the tournament. Ranking one individual against another is natural selection at its finest: the strongest, most able person obtaining the prize. However, is this always the case or can a less able individual "steal" the prize?

"Ability" is employed to designate the fruitful contribution of a worker within a production function; for example, workers use a combination of their own efforts and the available capital and technology provided by the firm to produce an output that has value to the company. However, tournaments provide an incentive for each contestant to engage in some sort of deviant behavior, such as cheating or sabotage. In fact, two ways exist for each contestant to win. Contestants can expend effort (a) to increase their own scores within the rules of the contest or (b) to improve their chances of winning, albeit illegally. I call the latter "deviant behavior," which is divided into two categories. The first category is cheating, or essentially improving one's own chances of winning using a method that is either illegal or violates the implicit principal–agent contract. Examples of cheating include an athlete so focused on winning that he or she takes performance-enhancing drugs in an attempt to gain an edge over competitors. The second category involves sabotage, or expending effort to reduce a rival's performance, thereby increasing one's own chances of winning. Examples of sabotage are subtle, for instance, conveniently forgetting to give a fellow sales representative a message from a potential client or failing to respect a particular sales territory. Politicians are often accused of sabotage by resorting to slander campaigns based on lies or half-truths. Egregious instances of sabotage have occurred as well, such as during the 1994 Lillehammer Olympic Games when Tanya Harding[18] hired a man to injure fellow figure skater Nancy Kerrigan.

The current paper utilized experiments to analyze the effect of deviant behavior on effort. Economic experiments are particularly helpful tools for this type of research because they enable us to observe and measure the variables of interest, i.e. effort and deviant behavior. A number of studies used experiments to measure effort in various tournament settings, but only a relative few have studied tournaments under the lens of sabotage and cheating; see Harbring and Irlenbusch (2005b) for a thorough overview and comparison. Harbring and Irlenbusch (2005a, 2005b) investigated whether the tournament size (the number of contestants) influenced subject behavior and found that this factor had virtually no effect on behavior. Further, they varied the fraction of

[18] Ms. Harding's ex-husband, Jeff Gillooly, and her bodyguard, Shawn Eckhardt, were also implicated and found guilty of charges associated with the beating of Ms. Kerrigan.

winner prizes (the proportion of winners to losers) and found that an even number of winners and losers enhances productive activities. Finally, they varied the magnitude of the winners' prizes without keeping the wage sum constant, and found that higher prizes increased both effort and sabotage. Because the purpose of their paper was to investigate the effect of different prize structures, they could not determine whether the results of increased effort and sabotage were from the higher wage sum or the higher wage spread.

In Falk et al. (2008) and Harbring and Irlenbusch (2009), a two-stage game is played during which the principal begins by choosing the wage spread. Harbring and Irlenbusch (2009) allowed the principal to also choose between two wage sums; then, the agents simultaneously chose their level of effort and decided whether they wanted to engage in sabotage. In Harbring and Irlenbusch (2009), subjects chose greater effort and more sabotage when the wage spreads increased, as well as when the wage sum was increased. The strategy of Falk et al. (2008) was to compare tournaments with and without sabotage. In tournaments without sabotage, subjects chose greater effort as the wage spread increased. In tournaments where sabotage was permitted, the subjects faced the binary choice of whether or not to sabotage. If they engaged in sabotage, then they destroyed the entire output of their competitor. Given this very strong effect of sabotage, effort increased from a fixed wage tournament to a tournament with a mild wage spread. The mild wage spread incentivized the subject to put forth their maximum level of effort. Every other contract, which had larger wage spreads, resulted in subjects providing less effort.

These papers used a method called "chosen-effort," whereby subjects chose their level of effort (and sabotage) from a cost table. Greater effort increased a subject's probability of winning, but the greater effort was also associated with a higher cost that reduced their overall payoff. A more recent method is the use of a real-effort task whereby subjects are assigned to a task, which better reflects an actual tournament. Carpenter et al. (2010) used a real-effort task to specifically examine the role of sabotage in tournaments. Their task required student subjects to complete form letters and stuff them into envelopes. The authors compared four treatments: two piece-rate contracts, one with and the other without sabotage, relative to two tournament contracts with and without sabotage. In the piece-rate contracts, subjects earned $1 per envelope produced (times a quality adjustment). In the tournament contracts, the subjects were divided into groups of eight and their pay was identical to the piece-rate but with an additional $25 bonus to subjects in each group that performed best. In the sabotage treatments, the other seven workers could blatantly engage in sabotage by underreporting the number of envelopes produced by opponents or, as a more subtle form, by underrating the quality of their competitors during peer-to-peer evaluations. The principal finding of Carpenter et al. (2010) was that tournaments increase effort only in the absence of sabotage; competitors are more likely to sabotage each other in tournaments and, as a result, actually provide less effort simply because they expect to be victims of sabotage.

The experiment, in the current study, involved two-player tournaments and three primary treatments. All experiments began and ended with the baseline treatment in which subjects competed by answering arithmetic questions. The baseline treatment can be thought of as having "perfect monitoring" or perfect managerial oversight because cheating and sabotage were not available options to the subjects. The other two treatments were a form of "imperfect monitoring," which allowed contestants to either "cheat" or "sabotage" to increase their own scores or, alternatively, decrease their opponents' scores by a fixed amount. The experiment randomly placed subjects into one of three contract types (see Table III-1). The first was a fixed

Table III-1 Possible Contracts That Differ with the Wage Spread

Contracts	Wage for Subject with Higher Score, W_h	Wage for Subject with Lower Score, W_l	Wage Spread $\Delta W = W_h - W_l$
Low Incentive	$10	$10	$0
Medium Incentive	$14	$6	$8
High Incentive	$18	$2	$16

performance contracts, in which the subject with the higher score earned more money. The two tournament contracts differed with respect to the wage spread. One had a moderately large wage dispersion of $8 (henceforth the moderate incentive contract) and the other contract (henceforth the high incentive contract) fostered a very competitive environment because the wage spread was twice as large, at $16. These three contracts allowed for an analysis of wage dispersion in a between-subjects design. Subjects in the fixed wage contract had the lowest effort levels across all three treatments: the treatment with perfect monitoring and both of the imperfect monitoring treatments (cheating and sabotage treatments). Therefore, tournaments increase effort in all three treatments including the treatments with imperfect monitoring. However, effort did not increase monotonically: maximum effort was found in the contract with only a moderate wage spread, and effort levels with a large wage differential showed an improvement over those for fixed wage contracts, but the increase was less than the effort related to the moderate contract. The difference between the moderate and high incentive contracts is not statistically significant; however, as will be discussed later in the results section, this pattern was repeated in five of the six sessions and using different measures of effort.

A possible explanation for why subjects in the high incentive contract exerted less effort was because they believed that they would be victims of deviant behavior. This hypothesis is supported by two findings. First, in the treatment with perfect monitoring, the likelihood of being a victim of deviant behavior is zero because no subject had the option to engage in deviant behavior. Only with perfect monitoring did effort increase linearly with the wage spread. For instance, subjects in the fixed wage contract put forth the lowest effort and the high incentive contract instilled the greatest motivation. Secondly, subjects in the high incentive contract reported a greater expectation of being victims of deviant behavior compared with the other contract types. Ironically, although expectation of victimization increased with the wage spread, in actuality, instances of deviant behavior were greatest in the moderate incentive contract. I find that those who provided the greatest effort were often the most vested in the task and, as such, were the most willing to engage in deviant behavior. For example, subjects who repeatedly engaged in deviant behavior had statistically greater effort than the majority of the subjects.

For treatments with imperfect monitoring, the benefits of cheating and sabotage (the point advantage) and the costs (the probability of being discovered) were designed to be equal across the two treatments. Because all of the tournaments involved exactly two players, a unique situation arose in which the strategies to cheat and to sabotage, and the payoffs for each treatment, were the same.[19] With this design (a) the effect of cheating and the effect of sabotage on effort can be analyzed independently, and (b) it can be determined whether the subjects viewed cheating and sabotage as identical strategies to improve their chances of winning.

Despite the imposed equivalence of the strategies, cheating occurred 49% of the time (105 observations) when it was available to the subjects. The rate of sabotage was much lower

[19] If the subjects believe that their opponent will play like economic man, and, therefore, have self-regarding preferences, then the treatments are also strategically equivalent.

and occurred only 35% of the time (76 observations). The strong and highly significant (P – values for Wilcoxan sign rank $\alpha = 0.00$) difference is more likely the result of psychological differences between perceptions of cheating and sabotage. Through the questionnaire, many subjects stated that they were not willing to engage in sabotage because this action would harm their opponent; however, they often chose to gain an advantage by cheating. These subjects did not interpret sabotaging and cheating as equivalent despite the fact that the end result of both activities was to reduce their opponents' likelihood of winning by the same degree. On average, subjects who cheated and sabotaged put in 14% ($\alpha = 0.04$) and 8% ($\alpha = 0.08$) more effort, respectively, than did counterparts who chose not to engage in deviant behavior. Despite differences in the individual behavior of cheaters, saboteurs, and honest players, little statistical variation existed across treatments with perfect monitoring compared with treatments that allowed for deviant behavior.

Before the setup of the theoretical model an overview of the experimental protocol and design is presented in Section 2. Section 3 explores the theoretical model and its predictions. Finally, Section 4 presents the results.

III. 2 Experimental Protocol and Design

The experiment was conducted in the Experimental Economics Center (ExCEN) at Georgia State University. The sessions were computerized using the Z-Tree platform (Fischbacher, 2007). In total, 108 subjects participated, each in only one session. Table III-2 provides an overview of the subjects' characteristics. In terms of standard demographics, the subjects were approximately evenly split between female and male; the average subject was

Table III-2 Participants' Characteristics

	Session 1[1] N	Session 1[1] Proportion (%)	Session 3–5[2] N	Session 3–5[2] Proportion (%)
Gender				
Female	17	47.2	31	57.4
Male	19	53.8	23	42.6
Age (Mean Years)	19.5	—	20.6	-
Race				
African American	21	58.3	36	66.6
Asian	3	8.3	6	11.1
Hispanic	2	5.5	3	5.5
Mixed	2	5.5	2	3.7
White	7	30.5	7	13
Other	1	2.8	—	—
Major				
Biological Sciences	4	11.1	7	13
Business (Non-Economics)	4	11.1	14	25.9
Economics	3	8.3	2	3.7
Education	1	2.8	—	—
Health Services	5	13.9	5	9.3
Humanities	2	5.5	—	—
Math or Engineering	2	5.5	5	9.3
Psychology	2	5.5	8	14.8
Social Science (Non-Economics or Psychology)	1	2.8	1	1.9
Other	10	27.8	10	18.5
Does Not Apply	2	5.5	2	3.7
Class Standing				
Freshman	18	50	20	37
Sophomore	7	19.4	19	35.2
Junior	6	16.7	13	24.1
Senior	5	13.9	1	1.9
Other	—		1	1.9

[1] Session 1 had 36 subjects.

[2] Session 3–5 had 54 subjects.

[3] Session 2's demographic information is missing because the questionnaire for this session was corrupted

about 20 years old, African American, and in his or her first two years at Georgia State University. Each session lasted approximately 1.5 hours, and on average a subject earned $21.15.

Before the start of the experiment, the subjects exchanged their signed consent forms for a set of instructions.[20] The instructions were written in non-emotive, neutral language and avoided "loaded" words[21] that could affect behavior (Klaus & Heike, 2006). Subjects were allowed to read the instructions at their own pace, after which the experimenter summarized the instructions in a PowerPoint presentation. The presentation began with an outline of the experiment, reminding the subjects that the experiment consisted of two independent parts. The subjects were told that Part 1 entailed six rounds, but that only one round, decided randomly at the end of Part 1[22], would be selected for payment. Each round required the subjects to perform a real-effort task (explained in more detail below) and there was no information revelation at the end of each round. After completion of the sixth round, a six-sided die determined which round was to be paid, and the subjects earnings then appeared on their respective screen. The subjects were told that the instructions for Part 2 of the experiment would be handed out immediately after Part 1 had concluded, and once they knew how much money they earned. Part 2 was a Holt and Laury (2002) lottery-choice experiment, in which subjects made ten choices, but only one would be randomly paid at the end[23]. Below I give more detail concerning each part of the experiment and procedures during the experiment.

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[20] An example of the instructions is found in Appendix B.1.

[21] For example, the words "opponent," "cheating," "sabotage," and "winning" were replaced with "group member," "manipulating your own score," "manipulating your group member's score," and "the subject with the higher score will get the larger prize."

[22] The maintained theory in this paper is by Lazear (1989)and is based on expected utility; therefore paying one round at random is theoretically incentive compatible (Cox et al., 2012).

[23] Because between Parts 1 and 2 there is a realization of payoffs, there are possible wealth effects in Part2. Studies that have looked carefully for wealth effects in experiments involving decisions under risk have found that they are insignificant (Cox & Epstein, 1989; Cox & Grether, 1996)

Part 1

Part 1 consisted of six rounds in which subjects competed by adding numbers. Arithmetic questions have been used in experiments frequently (e.g. Cason et al. (2010), Eriksson et al. (2008), and Niederle and Vesterlund (2005)). Each round required the subjects to add sets of randomly generated numbers for six minutes without using a calculator (scratch paper and pencils were provided); see Figure III-1 for a screenshot. This task is commonly used in experiments because performance is not associated with gender, socioeconomic background, or physical conditioning. The task does not involve learning, does require effort (mental concentration), is easily measured, and considerable variability exists across individuals.

Each round lasted for a total of six minutes. During the first two minutes of the task, the subjects were required to add two sets of randomly generated numbers between 0 and 100. To ensure increasing marginal cost of effort, once two minutes passed, the computer increased the level of difficulty by requiring the subject to add a third two-digit number. In the remaining two minutes, the computer increased the difficulty once again by adding an additional two-digit number, thus requiring the subjects to compute the sum of four numbers from 0 to 100.

The subjects were told that "luck" affected their score, and luck[24] was considered any random factor beyond anyone's control. Every question had a 10% chance of having a good luck or a bad luck element added or deducted to their score. Good luck always added an additional point to the subject's score; for example, if the computer selected that this question would receive good luck, and the subject answered the question incorrectly, the subject earned zero points. Normally, a subject lost one point for the incorrect answer; however, because the

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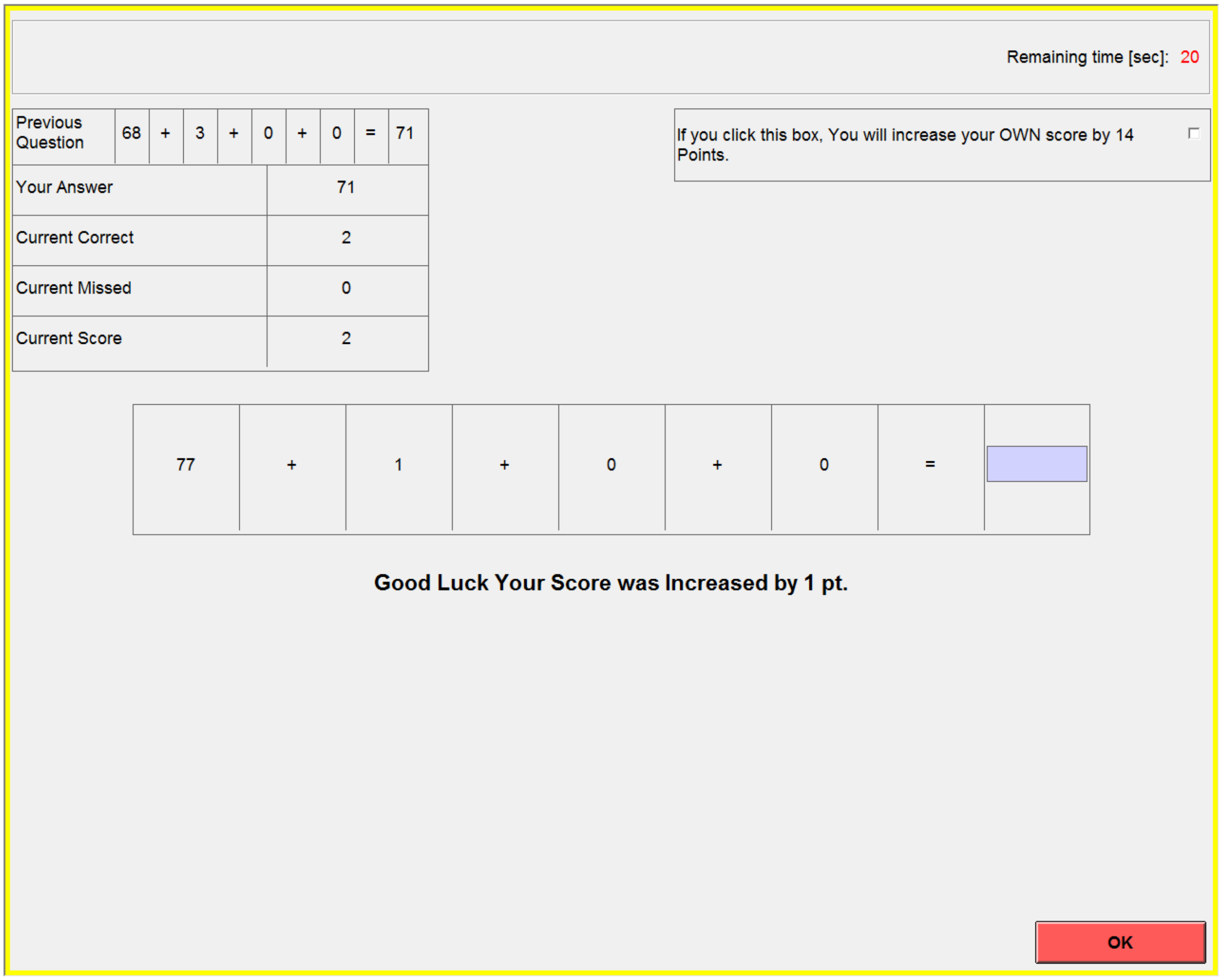
[24] As in the Lazear-Rosen model, output also depends on an additive shock term often referred to as "luck." Lazear and Rosen (1981) noted that "contests are feasible only when chance is a significant factor" (p. 845), which is detailed in Nalebuff and Stiglitz (1983). Commonly used examples of "luck" are random weather patterns and measurement error in the principal's monitoring of output.

Figure III-1 Display of Real-Effort Task.



question had this element of good luck, a point was subtracted but a good luck point was added, resulting in no change in score. If a subject answered this question correctly, *two* points were added to the subject's score. Bad luck always worked in reverse, subtracting an additional point to each question affected by bad luck. If a subject answered the question correctly then the net change in the score was zero, but if the subject answered it incorrectly then two points were subtracted. Subjects did not know if a particular question was affected by luck until after they answered the question and moved on to the next question (see Figure III-1).

All subjects participated in six rounds; however, the six rounds were not identical. In fact, each of the first three rounds could be viewed as different treatments (one with perfect monitoring and two with imperfect monitoring), whereas the last three rounds repeated the treatments to check for possible confounding of treatment effects with behavioral often time – order effects. The perfect monitoring treatment was the baseline treatment in which a subject earned one point for each correct answer and lost one point for each incorrect answer. If time remained within the round, a new question immediately appeared.

At the start of the experiment, each subject randomly selected a ball (which varied in color) and a mailbox key (with number known only to the subject). The subjects were shown that there were an equal number of red, white, and orange balls and that the total number of balls was the same as the number of subjects in the room; therefore, every subject saw that each color had an equal chance of being selected. Lab assistants then placed the balls in an urn and allowed subjects to select their individual balls without looking inside the urn. The three contracts were displayed on a screen at the front of the room and on TV monitors around the lab. The contracts differed with regard to the disparity between "high" and "low" wages (see Table III-1). After the subjects selected their ball and entered their color into their computer terminal, the experimenter placed three balls (one of each color) into the urn and selected one ball at a time. The experimenter revealed the color of the first ball, and all of the subjects who selected this ball color were in Contract 1, the fixed wage. Then the experimenter selected the second and third balls, placing the remaining subjects in Contracts 2 and 3, the moderate and highly incentivized tournaments. Using this method, all subjects were randomly assigned to one of the three contracts.

Although the subjects participated in six rounds, only one[25] round chosen randomly at the end of Part 1 was used for payment. Moreover, the subject who had the higher score in the chosen round earned the high wage. However, unlike Harbring and Irlenbusch (2005a, 2005b), who varied the wage sum, in this paper the prizes always summed to $20. Holding the wage sum constant made it possible to test for the effect of wage spread on effort without the confounding effect of differing wage sums.

The lab assistants also walked around with a box containing envelopes. Each subject was asked to select one envelope, inside of which was a uniquely numbered mailbox key. The subjects selected their envelopes and the experimenter showed the subjects the mailboxes. The experimenter informed the subjects that none of their decisions—nor their individual payoffs—were identifiable by the experimenter (or anyone else) because all of their decisions were linked to a mailbox whose number only they knew. Furthermore, they were told they did not have to see the experimenter or interact with anyone at the end of the experiment to receive their cash; their payments would be placed in their respective mailboxes. To ensure ultimate privacy, the subjects collected their payoffs out of sight from others since partitions were placed around the mailboxes.

The double-blind protocol was especially important in this task to eliminate all social consequences associated with engaging in deviant behavior. Because cheaters and saboteurs in real-life normally pursue these activities in private and hope to remain unobserved, this experiment allowed the subjects actions to remain anonymous. One of the integral parts of the experiment is that everyone who engaged in deviant behavior had a 20% chance of being

[25] Justification for the random selection of one round is found on page 19-21 of this paper.

audited. This audit most likely reduced their payoff[26] because if they were discovered to have engaged in deviant behavior they would automatically receive the low prize; however, no social stigmas were associated with engaging in deviant behaviors because no one—not even the experimenters—could personally identify cheaters (saboteurs) from non-cheaters (non-saboteurs). Unlike in a single-blind protocol, in which subjects approach the experimenter at the end of the experiment to receive their payments, the double-blind protocol removed this confrontation[27] and, consequently, removed any bias that may exist if subjects have heterogeneous sensitivity to being caught cheating or sabotaging. If subjects had differing sensitivities toward being "found out," then this would create differences in subjective costs associated with cheating and sabotage. *Ceteris paribus*, subjects with a lower sensitivity threshold are more likely to engage in cheating and sabotage.

Additional treatments. In addition to the perfect monitoring treatment previously described, two additional treatments allowed contestants to "cheat" or "sabotage," which increased their own score or decreased their opponents' score by a fixed amount. Subjects engaged in cheating or sabotage by clicking a box that appeared in the top right-hand portion of the screen (see Figure III-1). The subjects were told that at the commencement of each round, the experimenter would inform them of which treatment[28] they would be in, and every subject would have the same options (to cheat/sabotage/none of the above) available to them. The box giving subjects the option to engage in cheating (sabotage) remained on the screen throughout the round

[26] Subjects discovered during the audit automatically received the low wage; however, because one-third of the subjects were in the fixed wage, these subjects would not receive a monetary penalty for being caught. Further, in situations in which both subjects engaged in score manipulation, the computer randomly selected one of two subjects to receive the lower prize.

[27] A single – blind protocol would have be interesting and have validity. Most often than not, when one is caught cheating or sabotaging one is reprimanded by their superior or referee. For example in soccer a player may receive a yellow card, and for a serious infraction an expulsion. In any event, the agent does not have the luxury of remaining anonymous.

[28] The word *environment* was used in the instructions and presentation.

to enable them to make their decision at any time within the round. If they chose to cheat (sabotage) their opponent, a message appeared only on their screen, and their opponent was not notified. With asymmetric information, which simplified the experiment, the timing of when a subject chose to engage in cheating (sabotage) did not matter. Some subjects may have relatively certain attitudes toward cheating or sabotage, and knew at the commencement of the round whether they were going to cheat or sabotage. For other subjects, the decision may be conditional on their perceived performance within the round, and they may choose to cheat or sabotage only if they feel that their performance is low.

In contrast if the subject knew that his opponent was aware in real-time of his cheating or sabotage, then the optimal strategy of a subject would be to wait to click the box to engage in cheating or sabotage until the very last moment in an attempt to surprise their opponent. The theoretical implications changed from an analysis of unconditional preferences to conditional or reciprocal behavior, such as an “eye for an eye” behavior. In practice, real-time updates may distract subjects’ attention from the math task because subjects would need to remain vigilant of their opponents’ choices. This would most likely result in poorer performance in the math task because the subjects’ concentration would now be split among doing the task, deciding on their choices to cheat or sabotage or not do so, as well as monitoring their opponents’ strategy.

Again, subjects who clicked the box—engaging in cheating or sabotage—had a 20% chance of being detected. Subjects were told that a six-sided die would determine which of the six rounds would be selected for payment. After the selection of the round, a 10-sided die would then be rolled. If 1–8 (80%) came up on the die, then no one was caught cheating or sabotaging; however, if either 9 or 0[29] (20%) came up, then anyone who clicked the box (in the round that

[29] The die ranged from 0–9; therefore, 0 represented the number 10.

was selected by the six-sided die) automatically got the "low" prize.[30] In the first session, which had 36 subjects,[31] these subjects who cheated or sabotaged had either their score increased by 7 points or their opponent's score decreased by 7 points. Since the average score was 23.7, and cheating increased their score by 7 points, the result was one standard deviation[32] away from the mean score. The experiment was designed so that the average risk-neutral subject with self-regarding preferences should cheat or sabotage since the benefits outweigh the costs. A known exception is a subject who perceives their ability to excel in answering addition questions. This person would see little advantage in cheating or sabotaging because they recognize a corresponding decrease in their payoff if they are caught.

Little variations in effort were seen between the treatment with perfect monitoring and the cheating and sabotaging treatments. Therefore, the following sessions (Sessions 2–5) increased the incentive to engage in deviant behavior by increasing the effect of cheating and sabotaging to +14 and −14, respectively, which was approximately two standard deviations away from the mean. However, the cost—the probability of being detected engaging in deviant behavior—did not change and remained at 20%.

Overview of the Experiment. As shown in Figure III-2, Part 1 of the experiment always began and ended with the perfect monitoring treatments, Rounds 1 and 6. This technique allowed for a return to baseline verification to detect learning effects. The remaining rounds (2–5) always alternated the cheating and sabotage treatments. Half of the sessions were run with the cheating treatments preceding the sabotaging treatments (Rounds 2 and 4) and the cheating treatments

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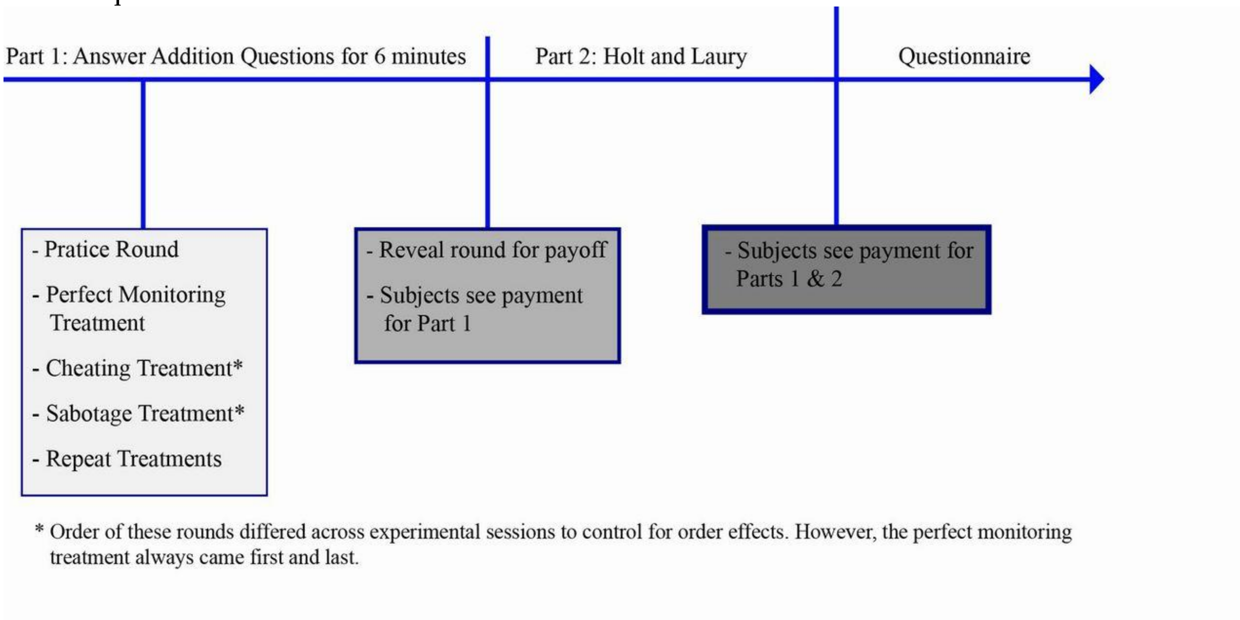
[30] Both subjects in a group clicking the box was treated as a tie, and the computer broke the tie by randomly selecting the subjects who received the low and high prizes.

[31] Given network constraints within the laboratory, later sessions were conducted with 18 subjects.

[32] The decision of 7 points was determined by estimating what one standard deviation would be from two places: data from a previously run experiment, "How is Effort Affected when Luck Impacts Outcomes," which is the first chapter of this dissertation, and a pilot session of this experiment. In fact, seven points was close to the actual standard deviation of Session 1, which was 6, and was equal to 7 across all experimental sessions.

Figure III-2 Sequence of the Game



followed the sabotaging treatments in the other half (Rounds 3 and 5). This scenario allowed for ordering effects to be averaged out across the treatments.

For the subjects in the tournament contracts, each round was a separate contest involving two subjects, with the subject who scored the highest earning the high wage. After each round, the subjects were randomly regrouped with another opponent. The subjects were told that they would have the same contract as their group member.[33] In real-life contests (whether professional, such as a sales bonus or promotion, or in athletics, such as a playoff tournament), one's closest rival often has similar monetary incentives.

Had the subjects been grouped randomly by ignoring the contracts, such a setup would have added a complication to the data analysis. For example, consider a subject in the fixed wage contract who was guaranteed $10 regardless of how he or she performed in the task. If this subject knew with certainty that he or she was paired with another person also facing a $10

[33] During the experiment, the experimenter substituted the term "group member" for opponent in order to use non-emotive language.

wage, then the motivation to perform the task would stem from a personal desire for accomplishment or perhaps to reciprocate (to the experimenter) for being paid for a task.[34] Either way, this subject's decision to perform the task was *efficient* in that his or her choice of effort presumably maximized his or her utility.

Note that in this example, the subjects' choice to provide or not provide effort only affected their utility and that of the principal.[35] If a principal existed, then the subject's effort would be very valuable from the principal's point of view. However, the subject's decision does not affect his or her group members' earnings because both subjects were in the fixed wage contract.

In contrast, if this person does not know the contract of his or her group member, the subject might logically conclude[36] that the group member is in one of the two tournament style contracts that pay more for a win. In this example, the subject is guaranteed $10 regardless of performance because he or she is in the fixed wage contract. If the subject cares about his or her opponent's payoff[37], then the subject may change his or her behavior by ignoring the task to ensure that the opponent earns the high payoff. In this example, this outcome is *inefficient*

[34] This experiment cannot identify the particular motivation for the effort put forth by subjects in the fixed wage contract. Ignoring long-run or repeated games, tournament theory predicts that subjects provide zero effort when they have self-regarding preferences toward the principal. Most subjects performed very well; however, substantial variation existed with regard to effort choice in the fixed wage contract: only five instances occurred of subjects who ignored the task and made zero effort (four of these occurrences were in rounds in which deviant behavior was an available option). On average, the effort of the subjects in the fixed wage contract was 22.8 (Session 1) and 22.1 (Sessions 2–5). These averages are much higher than the zero effort proposed by standard tournament theory.

[35] Conceivably, because no one is playing the role of the principal, the experimenter is thought of as the employer. However, because the experiment uses the double-blind protocol, the desire to reciprocate in this manner should be diminished.

[36] In every session there were an equal number of subjects in each of the three contracts, and since the subject has taken one of the slots in the fixed wage contract, the likelihood that their group member is also in the fixed wage contract is very low only 31% (29%) in session 1 (sessions 2 – 5).

[37] There is evidence that employees have strong preferences for their coworkers, perhaps even stronger than that of their employer. For example, Burton and Near (1995) find that most instances of cheating go unreported by a whistle-blower. Edward Morgan Foster once said, "If I had to choose between betraying my country and betraying my friend, I hope I should have the guts to betray my country," Bouville relates this to whistle-blowing and says, "it seems that whistle-blowing is the choice between betraying one's company and one's humanity (Bouville, 2008, p. 579).

because the subject performed the task if he or she knew with certainty that the group member was also in the fixed wage contract but instead chose to curtail his or her effort out of fear of reducing the group member's payoff.

At the termination of a round, revealing to the subjects the outcomes from each round is customary in most economic experiments.[38] Considerable thought went into deciding whether to follow this practice and reveal the outcomes of each round, such as thinking about whether to display to subjects their group member's scores, whether the group member engaged in cheating/sabotage, and who won the round by scoring the highest. The idea behind revealing this information was to give the subjects relevant information that they could learn from, and to examine whether, with this feedback, the subjects converged to an equilibrium.

However, this paper diverged from the norm and purposely withheld the outcomes of each round. This departure was necessary to keep the subjects' behavior as invariable as possible. Had the design of this experiment revealed the outcomes of each round, then subjects in later rounds would have had information about the behavior of other participants that they did not have in earlier rounds; therefore, subjects might update their subjective beliefs and consequently change their decisions in later rounds.

Consider a case in which a subject is about to begin the fifth round and, despite exerting substantial effort, learned that he or she lost the previous four rounds. This subject might become frustrated that his or her hard work was not rewarded and that another attempt would be futile; consequently, he or she may choose to reduce his or her effort. Psychologists who studied motivation have evidence that repeated failures induce distress, which reduces motivation in future tasks and results in weak performance (Brunstein & Gollwitzer, 1996). This correlation is

[38]For example, in Cox, Robertson, and Smith (1982), the authors tested a strategic equilibrium of bidding theory and used multiple rounds to test whether markets converge to an equilibrium.

especially true when the tasks are similar in nature. In classic expectancy-value theories, such as in Atkinson's (1964) risk-taking model, motivation to perform a particular task is the product of *outcome expectation* times *the valence* (what economists call expected utility) from the task. Repeated failures reduced the outcome expectation, which negatively affects motivation.

Changes in motivation that result from learning the outcomes of previous rounds, and how these motivational changes affect effort, is an interesting topic to explore. However, the topic is outside the scope of this paper and would create order effects rather than treatment effects. These order effects can be eliminated by withholding information on the rounds until the conclusion of Part 1, when all six rounds are completed.

To implement multiple rounds becomes problematic because the subjects need to be properly incentivized to perform each round. The two most common methods for paying the subjects are *Pay All Sequentially* (henceforth, PAS) and *Pay One Randomly* (henceforth, POR). Each of these methods has advantages and disadvantages and the experimenter must select the method that best fits the situation. In the case of PAS, which is most often used in market experiments, the payoffs are realized to ensure that subjects see how much they earned in the last round as well as accumulated earnings throughout the experiment. The disadvantage with PAS is that subjects learning the amount of their earnings could create *wealth effects* which would cause changes in behavior that are not caused by the treatments. For example, if subjects feel that they earned a sufficient amount of money in a previous round, they may curtail efforts in later rounds because they may have met the reservation wage or may feel that they can afford to act more altruistically toward their group members and not engage in cheating or sabotage. Such a change would not necessarily be caused by a treatment effect but could be a byproduct of incentivizing effort across multiple rounds.

In contrast, *POR* informs subjects that they will be playing multiple rounds and that the experimenter will randomly select one for payoff. Because only one round is selected for payoff, the subjects do not accumulate earnings and the wealth effects are eliminated. There are two commonly used methods of implementing POR. The first method displays to the subjects all of the decisions they will be asked to make before the subject is asked to make their choices. This version of POR is used in Holt and Laury (2002) and in Part 2 of my experiment. In the second version of POR, subjects are shown the current decision for the first time immediately before making their choices. Cox et al. (2012) call this version POR_{np} or "pay one randomly with no prior information." POR_{np} is the version used in this portion of the experiment.

In my experiment, each round can be thought of as a decision in which subjects are asked how much effort to provide, and they do not have prior information because they do not know the details of the future round until the previous one has ended. "Both versions of POR are theoretically incentive compatible for all theories that assume the reduction and independence axioms whereas PAS is not" (Cox et al., 2012, p. 12). Because the maintained theory in this paper is based on expected utility, which includes the independence axiom, POR is a theoretically incentive compatible mechanism for payment. After all six rounds were concluded, the subjects then realized their payoffs from Part 1 to eliminate portfolio effects permeating into Part 2 of the experiment.

Part 2

Subjects were exposed to risk on two dimensions. First, tournaments require contestants to supply costly effort up front and they do not know their final compensation until the end of the tournament. Whether risk-averse agents provide less effort up front out of fear that their efforts will go unrewarded or whether they provide relatively greater effort to avoid the low payoff is unclear (Jullien, Salanie, & Salanie, 2001). Second, contestants' decision to engage in deviant behavior was risky because of the 20% chance of detection and punishment resulting in the low wage. *Ceteris paribus*, a risk-averse subject engages less frequently in deviant behavior, thus minimizing their exposure to risk. It is unclear *a priori* whether a risk-averse agent will provide more or less effort compared with a risk-neutral or risk-loving counterpart; however, suspecting that differences may exist that can be easily controlled for by conducting a simple risk elicitation task as detailed below seems reasonable.

To elicit subjects' risk attitudes, Part 2 utilized the Holt and Laury (2002) procedure. The payoffs used in this experiment were three times the baseline amounts of the original Holt and Laury (2002) experiment. The higher payoffs were chosen to keep the cost-to-effort ratio relatively constant between Parts 1 and 2. In addition, because some contracts in Part 1 yielded a low payment to the losers, the higher payoffs ensure that the subjects are sufficiently compensated for their time. The higher payoffs were used in Anderson and Mellor (2008) and Anderson et al. (2012) and retained the pattern of expected values in the payoffs. That is the crux of this mechanism.

A second departure from the original Holt and Laury (2002) design involved the protocol. In this design, each of the 10 lottery choices were associated with a letter (Q–Z) and a corresponding bingo ball numbered 1–10. As shown in Figure III-3, each subject's table displayed the 10 decisions (Q–Z), thus preserving the order of the tasks and the required pattern to determine the switching point between Lottery A and Lottery B. However, the bingo ball number associated with each letter differed across subjects. Therefore, when the experimenter revealed the outcome of the bingo ball number, *ex post,* the matching letter and lottery choice were not the same for each subject. This technique minimized the variance between payment

Figure III-3 Screenshot from the Lottery Choice Experiment

Decision From Bingo Ball	Option A From Die	Option B From Die	Please make your Decisions
Q Ball = 5	$6.00 if the die is 1 or $4.80 if the die is 2-10	$11.55 if the die is 1 or $0.30 if the die is 2-10	Option A Option B
R Ball = 3	$6.00 if the die is 1-2 or $4.80 if the die is 3-10	$11.55 if the die is 1-2 or $0.30 if the die is 3-10	Option A Option B
S Ball = 8	$6.00 if the die is 1-3 or $4.80 if the die is 4-10	$11.55 if the die is 1-3 or $0.30 if the die is 4-10	Option A Option B
T Ball = 9	$6.00 if the die is 1-4 or $4.80 if the die is 5-10	$11.55 if the die is 1-4 or $0.30 if the die is 5-10	Option A Option B
U Ball = 1	$6.00 if the die is 1-5 or $4.80 if the die is 6-10	$11.55 if the die is 1-5 or $0.30 if the die is 6-10	Option A Option B
V Ball = 10	$6.00 if the die is 1-6 or $4.80 if the die is 7-10	$11.55 if the die is 1-6 or $0.30 if the die is 7-10	Option A Option B
W Ball = 2	$6.00 if the die is 1-7 or $4.80 if the die is 8-10	$11.55 if the die is 1-7 or $0.30 if the die is 8-10	Option A Option B
X Ball = 4	$6.00 if the die is 1-8 or $4.80 if the die is 9-10	$11.55 if the die is 1-8 or $0.30 if the die is 9-10	Option A Option B
Y Ball = 7	$6.00 if the die is 1-9 or $4.80 if the die is 10	$11.55 if the die is 1-9 or $0.30 if the die is 10	Option A Option B
Z Ball = 6	$6.00 For Sure	$11.55 For Sure	Option A Option B

Submit

possibilities and helped the experimenter with budgetary considerations. Instead of a die, a bingo ball was used because a 10-sided die revealed the subjects' payoff from the selected row, and using a die in both instances could introduce subject confusion. All other features of this design were identical to those in Holt and Laury (2002).

Questionnaire

At the end of Part 2, the subjects learned that they could earn an additional $5 for answering a questionnaire.[39] The questionnaire began with seven open-ended questions that predominantly asked the subjects (a) to explain their principal motivation for answering the math questions; (b) whether manipulating the scores affected their effort; (c) what factors they considered when deciding to manipulate their score or (d) the other player's score; (e) how they felt about cheating in the real-world and (f) sabotaging in the real world; and the final question, (g) their opinion on which was worse, cheating or sabotage.

Following the open-ended questions, the questionnaire asked 14 additional questions to measure a subject's competitiveness using the Revised Competitiveness Index[40] (Houston et al., 2002). Highly competitive individuals may be motivated beyond monetary incentives and complete the addition questions regardless of their contract simply because they gain utility from the task. The Revised Competitiveness Index uses a five-point rating scale, and the responses ranged from Strongly Disagree to Strongly Agree. The questionnaire ended with four questions that gathered basic demographic information: age, gender, race, and academic major. At the conclusion of the questionnaire, the subjects were called one at a time so they could collect their earnings anonymously and depart.

[39] The questionnaire can be found in Appendix B.2. After the second session (18 subjects), the file containing the questionnaire was found to be corrupted. Therefore, these observations were lost. However, the data file from Parts 1 and 2 remained intact.

[40] For a survey, see the Revised Competitiveness Index (see, for example, Harris & Houston (2010)).

III.3 Theoretical Predictions

This section follows Lazear (1989) and presents a simple model of rank-order tournaments that incorporates deviant behavior. The deviant behavior can take two forms: sabotage and cheating. We start in a similar fashion with Lazear (1989), who assumed that sabotage and honest behavior are the only two strategies available to contestants. After presenting the model with sabotage, we introduce cheating and sabotage as payoff-equivalent strategies. Both strategies reduce the likelihood by the same degree that the other contestant will win the tournament, and both have the same likelihood of being detected.

Directly reducing another subject's score might have psychological costs that are not present when players increase their own scores in a deliberate, but dishonest, fashion. Our purpose is to examine whether subjects perceive the two strategies as equivalent. To do this, only one type of deviant behavior is available to the subjects within a round. Instances of cheating are compared with the number of sabotages across the different rounds and variations in effort levels attributable to these different treatments are analyzed. Cheating and sabotage might be related activities, e.g., complements or substitutes. Considering how they interact and how their combined effects influence effort would be interesting to analyze, and it is my intention to examine this interaction more carefully in future research.

The tournament is between two contestants, *i* and *j*, who compete to produce more output as determined by their respective production functions:

$$q_i = f\left(e_i, s_j\right) + \epsilon_i,$$

where $f(\cdot)$ is the production function, $e_i \geq 0$ is the productive effort by contestant *i*; $s_j \in \{0,1\}$ is the binary choice variable indicating the sabotage decision of contestant *j* inflicted on player *i*; and ϵ_i is an i.i.d. random variable with E(ϵ) = 0 and variance σ^2. The production function is assumed to be additively separable because of the experimental design; however, a multiplicatively separable production function does not alter any of the theoretical predictions put forth in this section.

In one session of the experiment, we have $f(e_i,\ s_j) =\ e_i - 7s_j$, which reflects that an act of sabotage by *j* reduces *i's* production by seven points. In all other sessions the effects of sabotage were doubled; therefore we have $f(e_i,\ s_j) =\ e_i - 14s_j$.

The Player's problem is:

$$\max_{e_i, s_i}\left[W_h Pr(e_i, s_i;\ e_j, s_j) + W_l[1 -\ Pr(e_i, s_i;\ e_j, s_j)] - C(e_i, s_i)\right],$$

where W_h is the winner's payoff, and W_l is the loser's payoff. $C(e_i,\ s_i)$ is the cost of effort and sabotage with *C* increasing and convex in its arguments. $Pr(e_i, s_i;\ e_j, s_j)$ is the probability of *i* winning the tournament conditional on *i*'s and *j*'s effort and sabotage decisions. We write

$$\begin{aligned} Pr(e_i, s_i;\ e_j, s_j) &= Prob(q_i \geq\ q_j), \\ &=\ Prob(f(e_i, s_j) + \epsilon_i -\ f(e_j, s_i) -\ \epsilon_j) > 0, \\ &=\ Prob(f(e_i, s_j) -\ f(e_j, s_i) > \epsilon_j - \epsilon_i). \end{aligned} \tag{1}$$

Let $G(\cdot)$ be the distribution function of the random variable $(\epsilon_j - \epsilon_i)$ so that

$$G[f(e_i, s_j) -\ f(e_j, s_i)] = Prob(f(e_i, s_j) -\ f(e_j, s_i) > \epsilon_j - \epsilon_i).$$

The first-order conditions for contestant *i*'s maximization problem are

$$(W_h - W_l)\frac{\partial Pr(e_i, s_j; e_j, s_i)}{\partial e_i} = \frac{\partial C(e_i, s_i)}{\partial e_i}, \tag{2}$$

$$(W_h - W_l)\frac{\partial Pr(e_i, s_j; e_j, s_i)}{\partial s_i} = \frac{\partial C(e_i, s_i)}{\partial s_i}. \tag{3}$$

Let g be the pdf, then (2) and (3) can be rewritten as:

$$\left[(W_h - W_l)\mathrm{g}\left(f(e_i, s_j) - f(e_j, s_i)\right)\right] \cdot f_1(e_i, s_j) = C_1(e_i, s_i), \qquad (4)$$

$$\left[(W_h - W_l)\mathrm{g}\left(f(e_i, s_j) - f(e_j, s_i)\right)\right] \cdot -f_2(e_j, s_i) = C_2(e_i, s_i). \qquad (5)$$

As in Lazear (1981) we adopt the Nash-Cournot assumptions that each player chooses the optimal effort levels and sabotage decision based on the optimal choices of their opponents. Then, *j* takes e_i and s_i as given in determining her investment. Similar to (1) we can write for player *j*:

$$G\left[f(e_j, s_i) - f(e_i, s_j)\right] = Prob\left(f(e_j, s_i) - f(e_i, s_j) > \epsilon_i - \epsilon_j\right).$$

This leads to *j*'s first order conditions:

$$\left[(W_h - W_l)\mathrm{g}\left(f(e_j, s_i) - f(e_i, s_j)\right)\right] \cdot f_1(e_j, s_i) = C_1(e_j, s_j),$$

$$\left[(W_h - W_l)\mathrm{g}\left(f(e_j, s_i) - f(e_i, s_j)\right)\right] \cdot -f_2(e_i, s_j) = C_2(e_j, s_j)$$

Symmetry implies that when the Nash equilibrium exists that $e_i = e_j$ and $s_i = s_j$ so that P = G(0) = ½. This implies that both contestants have an equal chance of winning.

The existence of a unique solution depends on the dispersion of $\epsilon_j - \epsilon_i$. Since ϵ_i, ϵ_j are iid, we have $E(\epsilon_j - \epsilon_i) = 0$ and $Var(\epsilon_j - \epsilon_i) = Var(\epsilon_j) + Var(\epsilon_i) - 2Cov(\epsilon_j, \epsilon_i) = 2\sigma^2$. For a symmetric Nash Equilibrium to exist, the dispersion must be sufficiently large; i.e. luck must be a significant factor in the tournament. When σ^2 approaches zero, the increased chances of winning by working harder, $\mathrm{g}(f(e_i, s_i))$, must become infinite (see Lazear and Rosen (1981) and Nalebluff and Stglitz (1983)[41]). The intuition is simple, when σ^2 approaches zero, large amounts of effort are required to win the prize. A contestant who supplies no effort and instead does no work will earn the low prize, but avoids the disutility associated with effort. With the

[41] Furthermore, it is necessary for the second order condition for the maximization problem are $\left((W_h - W_l)\mathrm{g}(0)f_{11} - C_{11}\right) < 0$ and $\left((W_h - W_l)\mathrm{g}(0)f_{22} - C_{22}\right) < 0$.

variance of luck being arbitrarily small, there would be no doubt that the low output associated with the contestant is the result of a lack of effort rather than bad luck. Our model of tournaments is too simple to deter this type of violation because it is based on ordinal rank, where companies can implement punishments for low effort. Therefore, a sufficient amount of noise is needed to insure an interior equilibrium[42].

The first-order conditions in equations (2) and (3) have a simple explanation. The left-hand side represents the marginal benefit of wining the contest. $\frac{\partial Pr}{\partial e_i}$ and $\frac{\partial Pr}{\partial s_i}$ represent the marginal increase in probability of winning with respect to a change in the levels of effort and sabotage, respectively. These terms are then multiplied by the net benefit of winning, i.e., the wage spread, $(W_h - W_l)$. The right-hand side is simply the marginal cost of effort or sabotage.

The solution is then characterized by the following first-order conditions[43]:

$$(W_h - W_l)\mathrm{g}(0) = \frac{C_1(e_i, s_i)}{f_1(e_i, s_j)}, \tag{6}$$

$$(W_h - W_l)\mathrm{g}(0) = \frac{-C_2(e_i, s_i)}{f_2(e_j, s_i)}, \tag{7}$$

$$(W_h - W_l)\mathrm{g}(0) = \frac{C_1(e_j, s_j)}{f_1(e_j, s_i)}, \tag{8}$$

$$(W_h - W_l)\mathrm{g}(0) = \frac{-C_2(e_j, s_j)}{f_2(e_i, s_j)}. \tag{9}$$

Equations (6)–(9) have some important implications. First, because costs are increasing in sabotage and output is decreasing in sabotage, $\mathrm{g}(0) > 0$. As long as the cross partial, C_{es}, is not sufficiently negative, both equations (7) and (9) reveal that increasing the wage spread

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[42] For example on August 6 during the summer 2012 Olympics, Taoufik Makhloufi an Algerian runner was suspected of conserving energy in the 800 meter race for the later 1500 meter race in which he had a chance of "medaling." Because foot races do not involve much luck, The International Amateur Athletic Federation (IAAF) disqualified Makhloufi for not providing "a bona fide effort" during competition. The decision was later reversed when it was revealed that Makhloufi had injured his knee.

[43] For a detailed derivation of the results, see Appendix B.3.

increases the level of sabotage (as a reminder, sabotage is a destructive activity, and consequently $f_2 < 0$). The negative signs on the right hand side of equations (7) and (9) indicate that pay equality implies less sabotage. Secondly, using the same logic, equations (6) and (8) imply that effort also increases with the wage spread and decreases with pay equality. We summarize the results as follows.

Proposition 1: As the remuneration spread $(W_h - W_l)$ increases, subjects respond by increasing effort.

(Proof provided in Appendix B.3)

Proposition 2: As the remuneration spread $(W_h - W_l)$ increases, subjects respond by increasing their level of deviant behavior.

(Proof provided in Appendix B.3)

In our model, the agents have a binary choice of whether or not to sabotage. Falk et al. (2008) assumed that sabotage destroyed the entire output of the other person. Of course, this strong form of sabotage is possible in real life. For example, a contestant may attempt to have his or her opponent disqualified from the contest, which leaves the contestant unchallenged in the tournament. However, this form of sabotage certainly violates the condition set forth by Lazear (1989) in which the cross partial, C_{es}, cannot be sufficiently negative, implying that an act of sabotage leads to a dramatic reduction in effort. Instead we assume that sabotage provides aggressors with an advantage by increasing their likelihood of winning the higher prize, but it does not guarantee the saboteur instant victory. In addition to increasing the advantage from 7 points to 14 points, there was a 20% chance that a subject engaging in deviant behavior would be caught manipulating a score and become ineligible for the high prize[44].

We see that when the benefit to sabotage is increased holding the cost of sabotage unchanged, then the frequency of sabotage will increase. The intuition is straightforward and for a more rigorous explanation please sees the appendix B.3. Suppose player *i* believes that player *j* will engage in sabotage, then *i* is more likely to also sabotage when the advantage to sabotage is increased and the probability of getting discovered does not change. Likewise, if player *j* does not sabotage, player *i* is again more likely to sabotage. In either case, *i* is more inclined to sabotage when the benefit of sabotage, or the impact that sabotage has on his or her opponent, is increased, and the cost of sabotage remains fixed.

Hypothesis 1: When the effects of deviant behavior were changed from giving a seven-point advantage to a 14-point advantage, subjects choose to engage more frequently in deviant behavior.

We now turn our attention to the analysis of cheating instead of sabotaging. The two types of deviant behavior are payoff equivalent by design. In the experiment, sabotage and cheating are both binary choices, $s_i, s_j \in \{0,1\}$ and $c_i, c_j \in \{0,1\}$. The output of player *i* was given by the equation $f(e_i,\ s_j) = e_i - 7s_j$ and in the sessions where the effects of sabotage were doubled the equation becomes $f(e_i,\ s_j) = e_i - 14s_j$. Next we need to update the arguments of sabotage (s_i, s_j) accordingly. Note, sabotage was a destructive activity imposed by one player on the other; and therefore, the negative sign indicates that when the other player chooses to sabotage this in turn reduces the probability that the first player will win the contest.

[44] In cases in which both subjects were caught manipulating scores, this scenario was treated as though the subjects ended up in a tie; the computer randomly selected one contestant to receive the high prize and the other earned the low prize.

On the contrary successful cheating, increases both the cheater's likelihood of winning and their output. By replacing an act of sabotage with cheating we update the respective equations by substituting c_i for $-s_i$. The player i's output is then given by $f(e_i,\ c_i) =\ e_i + 7c_i$ and $f(e_i,\ c_i) =\ e_i + 14c_i$, respectively.

The magnitudes of these probabilities are equivalent to the case of sabotage. In other words, the probability of winning the tournament through sabotage or cheating is also equal if one considers c_i equal to $-s_i$. This equivalence allows us to use the theoretical predictions of sabotage in the case of cheating (Propositions 1–2 and Hypotheses 1) with appropriate adjustments.

Hypothesis 2: The levels of sabotaging and cheating are equal.

III.4 Results

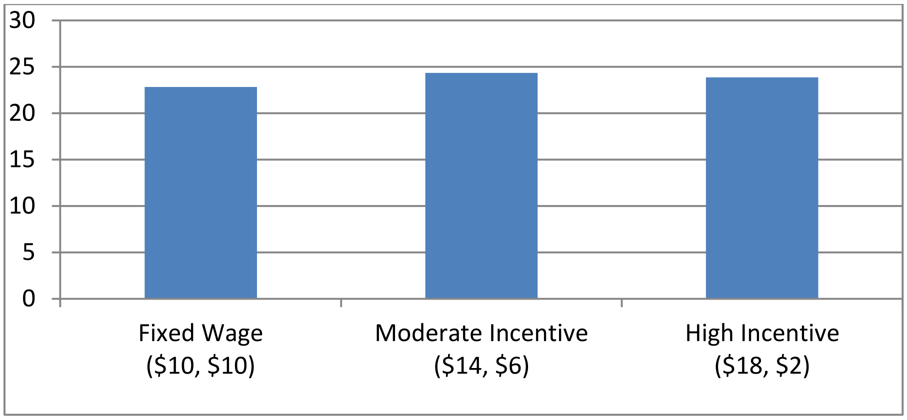
Contracts

The stage is set by analyzing the behavior of subjects across wage contracts. Following Proposition 1, effort should increase monotonically as wage spreads become more unequal. In Session 1 (henceforth, S1), cheating and sabotage gave a subject a seven-point advantage; the average effect of the tournament (positive wage spreads) was a 6% increase in effort over the fixed wage. However, effort did not increase linearly with the wage spread (see Figure III-4, Panel A). Effort was highest in the moderate incentive contract (bar 2, Panel A). To test the null hypothesis that the mean effort levels were the same, a Wilcoxon signed rank test[45] was performed, which showed marginally significant differences (p = .093). The other comparisons were not statistically significant.

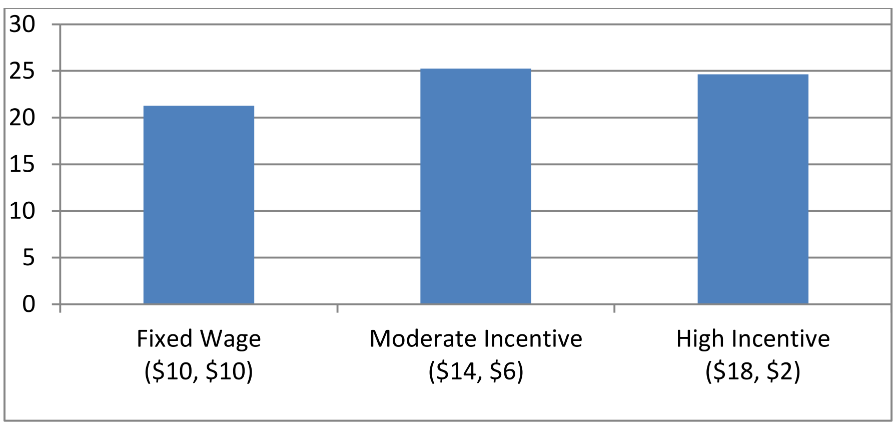
[45] The Wilcoxon signed rank is a nonparametric test that does not require normality. Kernel densities and the Shapiro-Wilk W test for normality were performed and confirmed that the variables were not normally distributed.

Figure III-4 Average effort as influenced by contract type

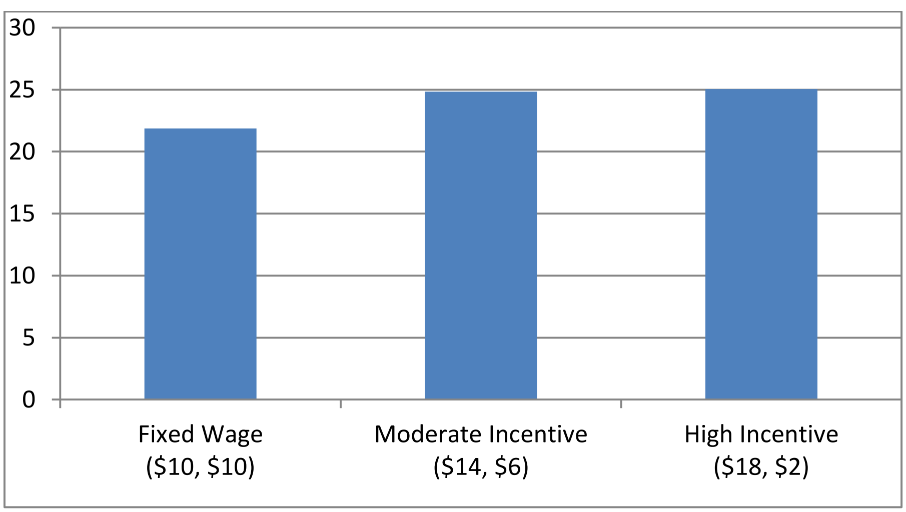
Panel A–Session 1: Average effort as influenced by contract type.



Panel B–Sessions 2–5: Average effort as influenced by contract type.



Panel C–Sessions 2–5 when there was no option for score manipulation.



One might argue that this inverted "U" pattern between effort and wage spread was an anomaly caused by the small sample size in S1 (72 observations per bar); however, this pattern was repeated in Sessions 2–5 (henceforth, S2-5). S2-5 rewarded subjects who engaged in deviant behavior with a 14-point advantage or approximately a two standard deviation lead. Each bar in Figure III-4, Panel B had 144 observations. Just as in S1, the inverse "U" pattern was repeated in S2-5. The tournament with the moderate incentive contract induced the greatest effort, followed by the high incentive contract, and the fixed wage provided the least effort. Pairwise comparisons were conducted using the Wilcoxon signed rank tests, and comparisons between the moderate incentive and the fixed wage and between the high incentive and the fixed wage were highly statistically different (α = .000 and α = .004, respectively). No statistical difference existed between the moderate and high incentive contracts. In S2-5, the average effect of the tournament versus the fixed wage was a 17% increase in effort.

As a robustness check, the total number of questions attempted (regardless of accuracy) was analyzed as a second measure of effort. If subjects increased their effort by increasing their speed, then precision may fall. The inverse "U" pattern was again repeated in both the S1 and S2 – 5 sessions. The moderate and high incentive contracts failed to be statistically different, while the other contract effects were highly significant.

Proposition 1 holds in the perfect monitoring treatment (Rounds 1 and 6), which did not allow subjects to engage in deviant behavior. With this restriction, the highest effort was found in the high incentive contract (Figure III-4, Panel C, Column 3). Although, the high and moderate incentive contracts were not statistically different, these data tend to support the theory that, with perfect employee monitoring, an employer can increase effort by increasing the wage spread. On the contrary, if monitoring was *imperfect* and employees could engage in deviant behavior, then effort reaches a maximum for the moderate incentive contract, and then falls when the remuneration spreads are increased. Despite this reduction in effort, tournaments—that is, positive wage spreads—encourage more effort than a fixed wage regardless of monitoring. However, data from this experiment suggest that employers (or other tournament designers) need to carefully design the tournament because the optimal wage spread may not be the contract with the largest wage spread, as is commonly believed.

Deviant Behavior

The previous section noted that tournaments increase effort overall; however, the principal question of this investigation was whether tournaments encourage unwanted behavior that might ultimately decrease their effectiveness. The short answer to this question is that tournaments foster negative behavior; nonetheless, the effect of deviant behavior on effort is not strong unless the subjects expect that they will become victims of deviant behavior.

After Session 1 concluded, no variation arising from the different treatments was discerned through an inspection of the data. Later, statistical methods were applied and confirmed that subjects did not change their effort from the perfect monitoring treatment to rounds that permitted cheating and sabotage. After analysis of the questionnaire, this conundrum was solved: Question 2 of the open-ended questions asked, “Did the option of manipulating scores affect your effort choice? If so, why?” Twenty-seven subjects (75 percent) responded that the option did not affect their effort (see Table III-3, Panel A). Six subjects responded that the option did affect their effort, but they did not indicate whether it caused them to increase or decrease their effort. Only one subject clearly stated that it caused a reduction in effort, whereas two subjects reported an increase in effort.

Table III-3 Response to Question 2:

“Did the option of manipulating scores affect your effort choice? If so, why?

Panel A – S1-Session

Categories	Frequency Number of Subjects	Percent
Decreased their effort	1	2.78
Increased their effort	2	5.56
Yes, but direction uncertain	6	16.67
No effect	27	75.00

Panel B – S2-5-Session

Categories	Frequency Number of Subjects	Percent
Decreased their effort	6	11.11
Increased their effort	7	12.96
Yes, but direction uncertain	11	20.37
No effect	30	55.56

Seven points—the advantage obtained from cheating or sabotage—was chosen because this number of points was estimated[46] to offer a one standard deviation advantage. Because such a high majority of subjects indicated that score manipulations had no effect, three adjustments were made to the subsequent sessions (S2–5). First, the effects of cheating and sabotage were doubled to 14 points, which represented two standard deviations. Second, the practice round administered prior to the start of the experiment was increased in length from three minutes to six minutes. In Session 1, the practice round lasted only three minutes, and its function was to

[46] See footnote 32.

familiarize the subjects with the screen layout, answering the questions through the computer, and reducing the initial uncertainty that a subject naturally experiences with new software. The practice round was extended to six minutes to replicate an actual round. This way, the subject could gain an accurate expectation of his or her score from a round to obtain a reference for the advantage of 14 points. The final change was to Question 2 of the questionnaire: if the subjects responded that score manipulation affected their choice of effort, the subjects were asked to specify the direction (increase or decrease) of the change.

Despite the incorporation of these changes, a large proportion (56%) of the subjects continued to indicate that score manipulation had no effect on their level of effort (see Table III-3, Panel B). Twenty percent indicated that score manipulation did have an effect on their level of effort, but disregarded the instruction to add the direction of change. Of the balance (24%) approximately an equal number of subjects indicated that the option to manipulate their score did decrease or increase their level of effort. Increasing the effect of deviant behavior from seven points to 14 points (along with the other two changes) substantially decreased the proportion of subjects (from 75% to 56%) who responded that score manipulation had no effect on their effort. The increase of the subjects reported that imperfect monitoring affected their effort was sizeable from 25% to 44%. However, the direction of the effect of imperfect monitoring still remains unclear both from the quantitative data and the subjects' responses to the questionnaire.

Whereas 75% and 56% of the subjects in S1 and S2–5 indicated that score manipulation did not affect their level of effort, most subjects believed their opponent, given the opportunity, would cheat or sabotage (see Table III-4). For example, only 14% of the subjects in S1 believed that their opponent would behave honestly, approximately one-third was uncertain, and 42%

Table III-4 Response to Question 4:

"Given the opportunity, I thought my group member would manipulate a score."

Panel A – S1-Session

Categories	Frequency Number of Subjects	Percent
Strongly Disagree	3	8.33
Disagree	2	5.56
Neither Disagree Nor Agree	11	30.56
Slightly Agree	5	13.89
Strongly Agree	15	41.67

Panel B – S2-5-Session

Categories	Frequency Number of Subjects	Percent
Strongly Disagree	5	9.26
Disagree	4	7.41
Neither Disagree Nor Agree	5	9.26
Slightly Agree	8	14.81
Strongly Agree	32	59.26

strongly believed their opponent would manipulate a score (either by cheating or sabotage). When the incentive to manipulate a score was increased to 14 points, almost three-quarters of the subjects believed that their opponent would attempt to gain the advantage by manipulating a score.

Recognizing this large increase (from 56% -75%) in subjects believing that their opponent would manipulate a score, this paper examines those subjects who changed their beliefs. 17% of the subjects (approximately the same percentage in S1 and S2–5) believed that their opponents would behave honestly. The vast majority of these subjects were in the fixed wage contract, and they correctly realized that their opponent did not have an incentive (or disincentive) to manipulate a score because the payoff was the same regardless of their actions. In S1, 31% of the subjects reported that they were uncertain whether their opponent would manipulate a score. This percent decreased significantly to 9% after the changes to cheating and sabotage took effect in S2-5. The subjects who felt strongly that their opponents would manipulate their score increased from 42% to 59% from S1 to S2-5.

An important piece of the puzzle was uncovered by realizing that subjects' expectations of being victims of deviant behavior increased significantly from 56% to 75% by changing the effects of cheating and sabotage from seven points to 14 points. Most of this increase was explained by subjects who were in tournament style contracts and not in fixed wage contracts. In fact the type of contract that the subjects were in strongly influenced whether subjects believed their group member would take the opportunity to manipulate a score. For example, see table III-5, which shows the proportion of subjects who answered that they would be victims of score manipulation. As one can see, the expectation of being a victim increases significantly with the wage spread. Wilcoxan sign rank test confirmed that subjects' expectations were statistically different across the three contracts $\alpha = 0.00$. In the fixed wage contract, half the subjects believed they would be victims, and the other half believed either that their opponent would play honestly or were uncertain. This provides a baseline of the subjects expectation of being victimized.

Table III-5 Proportion of subjects in S2 – 5 who agreed with Question 4*:

"Given the opportunity, I thought my group member would manipulate a score."

Three Wage Contracts	Proportion of subjects who Did Not Agree**	Proportion of subjects who Agreed***
Fixed Wage	50%	50%
Moderate Incentive	22%	78%
High Incentive	6%	94%

* Question 4 – from the closed response question was a Likert-type questions (with a 5-point response scale).

** Believed their opponent would play honest or uncertain

***Subject who believed they would be victims of score manipulation

In the moderate incentive contract 78 percent believed they would be victims, and the high incentive contract a striking 94% of the subjects believed they would be victims.

These data provide evidence that, as the wage spread increased the subjects' expectation of becoming victims of score manipulation also increased. But, what happened to the actual number of cases of cheating and sabotage? Table III-6 shows that the moderate incentive contract had the highest number of instances of deviant behavior, the high incentive contract had slightly fewer, and the moderate and high incentive contracts both had larger numbers of instances of deviant behavior compared with the fixed wage contract ($\alpha = 0.02$ and 0.05, respectively).

Table III-6 Number of Deviant Behaviors by Contract Type

Three Wage Contracts	Did Not Engage in Deviant Behaviors	Engaged in Deviant Behaviors	Proportion of subjects who engaged in Deviant Behaviors
Fixed Wage	97	47	33%
Moderate Incentive	76	68	47%
High Incentive	78	66	46%

Effort did not increase monotonically with the wage spread with imperfect monitoring, but instead followed an inverse "U" pattern. However, effort levels did increase monotonically with perfect monitoring. No expectation of being victimized existed because the perfect monitoring treatment did not allow for subjects to cheat and sabotage. With imperfect monitoring, subjects' expectation of being victimized was shown to increase with the wage spread. Perhaps this expectation discouraged subjects from working as hard. However, expecting to be a victim of deviant behaviors does not explain the low number of deviant behaviors actually observed in the high incentive contract. The high incentive contract had slightly fewer observations of deviant behaviors, a negative relationship, compared to the moderate incentive. This variation was not statistically different, but one would expect the sign to be in the opposite direction, and that the number of deviant behaviors would be greatest in the high incentive contract. Economic theories predicts high levels of deviant behaviors both because the monetary incentives were the strongest, and also because the expectations of the subjects of being victims were the highest. The high expectations mean that subjects would level the playing field by engaging in deviant behaviors but this finding remains puzzling. To provide insight, I turned to the psychology literature. G*oal setting* is the single most dominant theory in the field, with over a thousand articles and reviews published on the topic in a little over 30 years (Porter, Bigley, & Steers, 2003). Klein et al. (1999) conducts a "meta-analysis" of goal setting by surveying over 80 studies. They find that goal attainment has a strong positive effect on performance and motivation. Perhaps the high expectation of being a victim of deviant behavior not only reduces effort but also motivation; that is, perhaps there is also a loss of interest. This loss of interest may deter effort and reduce the desire of subjects to engage in deviant behavior. This explanation would be consistent with the inverse "U" patterns observed of both effort and deviant behavior. In other words, high levels of being victimized may cause one to curtail effort and to disengage from the task because interest was lost. A loss of interest causes subjects to not actively manipulate scores. However, in the case of moderate incentive, their expectations of being victimized were not as strong and they remained motivated. This motivation enabled the subjects to perform at their best and instilled a strong desire to win, therefore displaying the greatest instances of deviant behavior.

Attention now turns to examining the differences between cheating and sabotage. As previously noted, the experimental design was specifically chosen to make cheating and sabotage payoff-equivalent strategies. If the subjects indeed viewed them as equivalent, then one would expect an equal number of occurrences of cheating and sabotage, which is the basis of Hypothesis 2. However, despite the equivalence, cheating occurred more frequently (49%, 105 observations) compared with sabotage (35%, 80 observations). These differences were highly significant ($\alpha = 0.00$) and the questionnaire provides insight into this result. For example, many subjects shared the opinion of Subject 10, who said, "No, I didn't want to hinder someone else, although they could have hindered me," despite the fact that she strongly believed that her opponents would manipulate the scores. Only about half of the subjects who believed that their opponents would manipulate the scores chose to engage in such activity. Many subjects spoke about taking the "high road" or having been raised with "strong morals." Moreover, even though they could not be identified personally and many felt that the chances of being detected by the audit were low, they obviously felt an emotional burden and chose not to engage in deviant behavior.

In contrast, almost all of the subjects who engaged in deviant behavior reported that they believed that their opponents would manipulate their scores. Perhaps the response was an attempt at exonerating their selfish actions. Only about 5% of the subjects who manipulated their scores believed that their opponent would play honestly. Stating a strong belief that their opponents would manipulate their scores may have justified, if only to themselves because the experiment was double blind, the behavior as acceptable and that they were no longer selfish but were just leveling the playing field. Most of the subjects in this category said something to the effect of, "Yes, I decided to manipulate every time I was able to. The chances of getting caught were really low. Besides, I can't trust my opponent, so . . . better to be safe than sorry" (Subject 40). Other subjects wrote that they made several mistakes and that their performance was low, thus they needed the advantage of cheating or sabotage to increase the likelihood they would get the higher payoffs.

Subjects who manipulated once were much more likely to manipulate again. For example, in S2-5, 63% of the cheaters cheated twice and 64% of saboteurs sabotaged twice (see Table III-7). In addition, subjects who engaged in one type of deviant behavior were also likely to engage in the other type. For example, 69% of cheaters also sabotaged and 94% of saboteurs

Table III-7 Repeat Offenders

	Number of Subjects	Percent
Session 1		
Cheated Twice	9 subjects	56.3
Sabotaged Twice	6 subjects	54.5
If Cheated (twice) →Sabotaged*	5 subjects	55.5
If Sabotaged (twice) →Cheated**	6 subjects	100
Session 2-5		
Cheated Twice	31 subjects	63.3
Sabotaged Twice	23 subjects	63.9
If Cheated (twice) →Sabotaged*	26 subjects	53.1
If Sabotaged (twice) →Cheated**	23 subjects	100

* Subjects who always cheated and sabotaged at least once.
** Subjects who always sabotaged and cheated at least once.

also engaged in cheating (see Table III-8). Despite the fact that cheating and sabotage were payoff-equivalent strategies,[47] the data reveal important differences between cheating and sabotage. This implies that many of the subjects did not perceive the two strategies as being equivalent, thus rejecting Hypothesis 2. For example, there were significantly more instances of cheating than sabotage and, although subjects who cheated were also likely to sabotage, the subjects who sabotaged almost always cheated. For example, a comparison of the subjects who cheated in both rounds (henceforth, repeat cheater) to those who sabotaged in both rounds (henceforth, repeat saboteur) shows that slightly more than half of the repeat cheaters also engaged in sabotage

[47] They both gave the manipulator the same advantage of seven or 14 points, depending on the session and both had the same 20% likelihood of being detected

Table III-8 Panel A – Instances of Score Manipulation

	Frequency	Percent
Session 1		
Instances of Cheating	25	34.7
Instances of Sabotage	17	23.7
If Cheated (once) → Sabotaged*	9 subjects	56.3
If Sabotaged (once)→ Cheated**	9 subjects	81.8
Session 2-5		
Instances of Cheating	80	55.6
Instances of Sabotage	59	41.0
If Cheated (once) → Sabotaged*	34 subjects	69.4
If Sabotaged (once)→ Cheated**	34 subjects	94.4

* Subjects who cheated once and sabotaged at least once.
** Subjects who sabotaged once and cheated at least once.

regardless of the session, whereas all of the repeat saboteurs cheated at least once regardless of the session (see Table III-8). Question 3 used in conjunction with Question 4 of the questionnaire was very enlightening. Question 3 was, "Did you manipulate your own score? What factors did you take into account when making this decision?" Question 4 was identical except that it asked if they manipulated the *other person's* score. Several subjects responded that, for a variety of reasons, they manipulated their own score (cheated) but did not manipulate the other person's score (sabotage). For example, when asked if he or she manipulated the other subject's score, Subject 8 said, "No. That would be a mean thing to do. I wouldn't want them to manipulate my score, so I didn't manipulate theirs." Ironically, Subject 8 chose to cheat on both occasions.

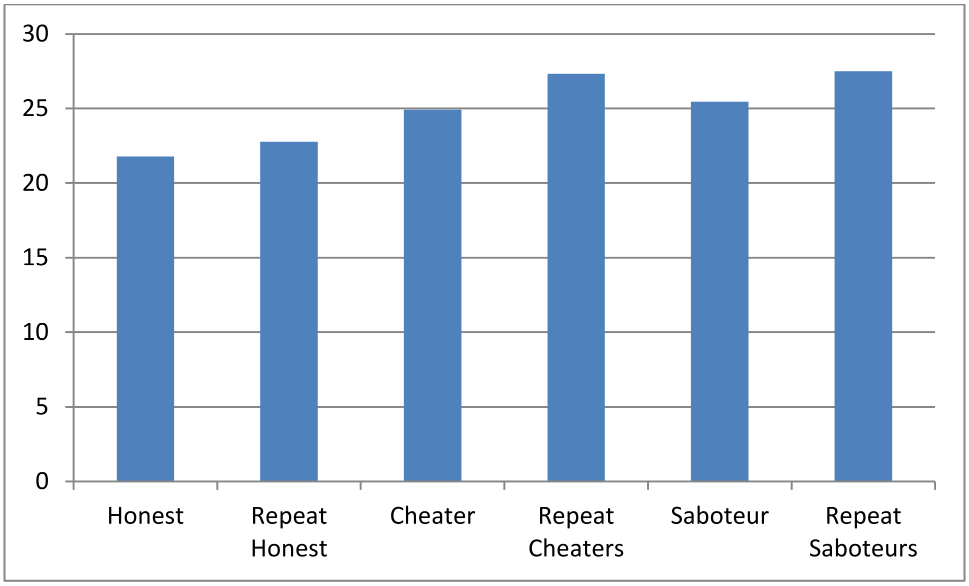
Likewise, other subjects said that they did not want to “hinder” the other person; yet, as previously noted, both strategies were equivalent in giving the same advantage to the aggressor. Winning by cheating meant that a subject harmed the other person because their group member got the lower prize. Evidently, these subjects did not believe it to be important that cheating caused just as much economic harm as sabotage.

Furthermore, I test whether the effort levels of those engaged in deviant behavior are different from those of honest players. The data show that subjects engaged in deviant behavior had higher effort levels than honest players (see Figure III-5). For example, a subject who manipulated his or her own score—a cheater—provided 14% greater effort than a subject in the same round but who did not cheat. This figure was statistically significant ($\alpha = 0.089$, Wilcoxon signed rank test). Likewise, saboteurs provided 12% more effort compared with their honest counterparts, which was also a statistically significant result ($\alpha = 0.036$, Wilcoxon signed rank test). No statistical difference existed between a cheater and a saboteur and between honest players across different rounds. Furthermore, repeat cheaters and repeat saboteurs had 25% and 21% greater effort than honest players, respectively. This provides evidence that those who were the most likely to engage in deviant behavior were also the ones most vested in the task.

The subjects who provided the greatest effort might be expected to also score high on the Revised Competitive Index[48]; such subjects may be so competitive that they sought to win at any cost. However, this was not the case, as repeat cheaters and repeat saboteurs were seen across the entire distribution of this index. As determined by the Revised Competitiveness Index, some were classified as highly competitive, some as average, and some below average. A second premise would be that repeat cheaters and repeat saboteurs were more intrinsically motivated or

[48] The Revised Competitiveness Index is a structured personality instrument and was part of the questionnaire (see appendix). The index consists of 14 Likert-type items (with a 5-point response scale) concerning interpersonal competitiveness in everyday contexts.

Figure III-5 Average effort by subject character in S2 – 5.



obtained greater utility from the task than other subjects. However, the questionnaire showed that half the subjects stated that their primary motivation was *not* intrinsically driven, and when asked to state whether they agreed with the statement that they "put in maximum effort into every round," some subjects strongly disagreed with the statement. Therefore, based on the subjects' responses, intrinsic motivation was rejected as an indicator of the high degree of cheating and sabotage. Gender was also not a factor. However, half the subjects in the experiment who were classified as risk loving (from the risk elicitation task) (Holt & Laury, 2002) were classified as repeat cheaters and repeat saboteurs. Only one of the subjects was in the risk-averse range, and the remaining subjects were in the risk-neutral category. Table III-9 shows the overall distribution of subjects across risk categories, alongside those by Anderson et al. (2012). As is shown, the proportion of choices is similar across both experiments. This indicated

Table III-9 Proportion of Subjects' Risk Classification by Study

Classification	Risk Elicitation Task	Anderson et al. **(2012)**
Risk-Loving	5.6%	7%
Risk-Neutral	20.8%	19%
Risk-Averse	73.6%	73%

that the risk-attitudes of the subjects are consistent with other studies, and that risk-averse subjects are less likely to engage in multiple acts of cheating and sabotage.

III.4 Conclusion

The question of whether tournaments increase the likelihood of deviant behavior over a fixed wage contract was answered affirmatively. The answer to the second question of whether effort decreases with respect to greater deviant behavior remains ambiguous. When deviant behavior was given a seven-point advantage, little statistical variation occurred across the treatments, and most subjects responded that deviant behavior did not affect their effort. Therefore, it was decided to double the advantage one receives from engaging in deviant behaviors to amplify the effect that deviant behavior had on effort. With this change, the number of subjects who responded that deviant behaviors affected their effort increased significantly. Notwithstanding, the increase from 7 point to 14 point advantage it could not be determined if subjects responded to deviant behaviors by increasing or decreasing their level of effort.

The next question, Question 3, is whether the net effect of tournaments increases effort when the possibility of being victimized or being the aggressor of negative activities exists. The answer to this question is also affirmative—with one caveat: tournaments increase effort overall, but effort levels reach a maximum. Increasing the spreads by a moderate amount produced maximum effort; however, overly unequal prizes lead subjects to believe that they will become victims and cause them to slightly decrease effort. Two results were surprising. First, subjects believed that the high incentive contract would have the largest number of instances of deviant behavior; in fact, it was the moderate contract that did. Second, the subjects most likely to engage in deviant behavior were also the subjects making the greatest effort. This finding was not correlated with gender or an intrinsic appreciation for the task. The only correlation occurred with a very low likelihood of being classified as risk averse.

Therefore, the conclusion from this study is to use tournaments and monitor the contest. Because tournaments continue to outperform a fixed wage contract; however a principal should be cognizant that tournaments also increase the likelihood of deviant behaviors.

CHAPTER IV

COMPETITORS WITH ORP

IV.1 Introduction

The foremost advantage of using tournaments is that a principal can dovetail his or her own interests with those of the agents, and thereby reduce or even possibly eliminate shirking. Following the seminal article of Lazear and Rosen (1981), most tournament models rely on agents having self-regarding (or "economic man") preferences in which the agents are assumed to be concerned exclusively with their own material payoffs. That is to say, the agents are indifferent to the material payoffs of their rivals and are emotionless with regard to the competitive nature of the tournament (are unmoved between the act of winning and losing beyond the monetary payoff[49]). This paper asks a fundamental question: are contestants self-interested in environments, which are based on relative performance?

There are many reasons to believe that people care deeply about the earnings of others. Consider for example, three complementary areas of study: psychology, sociology, and economics.

[49] A future paper examines if whether contestants have preferences over how they win; i.e., whether the game was fair or lopsided, and how they won; i.e. did the contestant show sportsmanship or did they prefer to "run the score up."

Psychology (Equity Theory): Originally developed by psychologist J. Stacy Adams in the 1960s, Equity Theory attempts to explain people's reactions to equity. Equity is said to exist whenever the ratio of a worker's rewards (wages) to inputs (effort) equals the ratio of other's rewards and inputs. There are several examples of Equity Theory being applied to what economists would call tournaments. For example, Friedman (1999) examined the wage structure of the National Basketball Association. He noted that because of the salary cap, as teams recruit more expensive superstars, teams have less money to pay their other players. Consequently, a greater number of players will earn the league minimum (including some starters). According to Equity Theory, this inbalance will lead to low morale, and as Friedman notes, low morale was a particular problem for the Houston Rockets, which had three highly paid superstars and a majority of the team receiving the league minimum. Cowherd and Levine (1992) examine the relationship between executive pay and lower level employee performance. They find that the smaller the difference between the pay of the top business units (CEOs and other top managers) and the pay at the bottom, the greater the product quality. They conclude that as lower level employees notice the inequality between the rewards, the lower level employees react by lowering their performance.

Sociology (Distributive-Justice): In sociology, the study of equity has been guided by a dominant framework, "the theory of distributive justice," offered by George Homans[50] (Rubinstein, 1988). Homans draws from the works of Aristotle, who equated justice with a proper ratio of inputs to rewards. "The rewards of each man be proportional to his costs - the greater the rewards, the greater the costs - and the net rewards, or profits of each man be proportional in his investments - the greater the investments, the greater the profit" (Homans,

[50] George Homans was a notable sociologist and is credited as being the founder of behavior sociology (Molm, 2005)

1961, p. 75). Justice requires comparing costs to rewards in much the same way Equity Theory compares inputs to rewards. In "Equity Theory," Homan's definition of justice is rarely challenged (Rubinstein, 1988).

Boulding (1962) has discussed two general principles that lie at the core of Distributive-Justice judgments. The first is the "principle of disalienation," which asserts that no unit of society shall be left without claim. This principle stresses that there should be a social minimum level that no individual is allowed to fall under; i.e., a minimum wage. The second is the "principle of dessert," which states that individuals should receive what they earn. This principle allows for income inequality when one individual contributes more than another (Rubinstein, 1988).

In economics there is a great deal of evidence to support interdependent utilities, or "other-regarding–preferences," in which the other person's satisfaction and/or payoffs enter into one's utility. For example, in the ultimatum game[51] (Güth, Schmittberger, & Schwarze, 1982) the game-theoretic prediction is clear when the model assumes that both players have self-regarding preferences. Economic man is assumed to be totally rational, but limited to caring about maximizing his own material payoff. When this model is used, the prediction is that the receiver will accept any offer because even a small amount is better than nothing. In this simple model, the size of the pie and the perceived intentions of the receiver are inconsequential; the only payoff that matters is one's own. Since the receiver is expected to accept any offer, the proposer will offer the smallest divisible split. However, numerous experimental studies from different

[51] In the traditional ultimatum game two subjects are asked to split a fixed monetary payoff for example $10. The first participant, "the proposer," is told that he or she will determine how the $10 will be split between herself and the other subject, "the receiver." The receiver will decide whether to accept or reject the proposed split. If the receiver accepts the allocation, then the money is divided according to the proposed split. If the receiver rejects the offer, neither subject receives any of the $10.

countries with different stake sizes exist, and different experimental procedures exist that show that people do not behave in line with the game-theoretical “hard-nosed” Nash equilibrium outcome. For an overview, see Thaler (1988), Güth and Tietz (1990), Camerer and Thaler (1995), and Roth (1995). Instead, the vast majority of offers typically average about 40 to 50%, with a 50–50 split being the modal offer. Furthermore, low offers are frequently rejected. Camerer and Thaler (1995) conclude in a review of the literature that these experimental results “are no longer a question.” Fehr and Schmidt (1999) contend that these findings are “robust facts.”

In addition to the empirical support for other-regarding preferences, economics has developed several theoretical models. Examples of models based on other–regarding preferences include the inequality aversion models of Fehr and Schmidt (1999) , as well as Bolton and Ockenfels (2000), the quasi-maximin model of Charness and Rabin (2002), and the egocentric altruism model of Cox and Sadiraj (2007).

If workers (or other agents) have other-regarding preferences and there is perceived inequity, then this will create tensions or distress within the individual. The amount of tension is proportional to the magnitude of the inequity (Porter et al., 2003). Inequity can arise from a broad range of variables (for example, recognition, promotion, favorable treatment), but the focus of most research has been on pay. If inequality exists, then relative–performance schemes, especially winner-take-all tournaments, will magnify the inequality between workers. It is important to note that neither Equity Theory nor Distribute Justice requires egalitarian–fixed wages. For example, a situation in which person A earns more than person B is considered equitable if A is perceived to put in more effort than B. However, tournaments are unlike most other forms of pay, because tournaments by their very nature create a winner and a loser.

Assuming both workers put in similar effort and there can only be one winner, then one may feel overpaid and the other underpaid compared to the amount of effort they provided. And winner-take-all tournaments magnify the wage inequality between the workers, creating the potential for a larger perceived inequitable conclusion. The "principle of disalienation" called for a minimum wage. In a winner-take-all tournament, the loser earns a wage of zero.

Two related articles (written concurrently) are Grund and Sliwka (2005) and Demougin and Fluet (2003). Both articles are based on applying the Fehr and Schmidt (1999) inequality aversion model to tournaments. Agents who lose feel deprived and suffer *envy* (or disadvantage inequity) in addition to having the lower monetary payoff. While the winner suffers *compassion* (or advantageous inequity), thereby reducing the utility from wining and earning the higher prize.

Both papers find a similar result; that in the short run, when two workers compete for a prize, workers with inequality aversion provide more effort than self-interested workers. This is because workers are assumed to dislike envy more than they dislike feeling compassion.[52] Therefore, inequality-averse workers create a surplus for the principal (the employer) because on the net they increase their level of efforts in order to avoid losing and feelings of envy. Grund and Sliwka (2005) refer to this boost in effort as the "incentive effect" of inequity aversion.

In the long run, when workers are mobile and have employment flexibility, the inequity aversion leads to an "inequity cost," which reduces the workers' willingness to participate in tournaments (as when one refuses to play a game because they "hate to lose"). Therefore, in order to attract workers, a compensating wage differential must be offered to properly

[52] This is consistent with laboratory experiments testing Equity Theory, in which the authors found that individuals were more willing to accept "good fortune" and being overpaid rather than being treated unfairly and being underpaid (Lawler, Koplin, Young, & Fadem, 1968).

recompense workers for their added costs,[53] which consequently reduces the efficiency of tournaments.

The current study uses economic experiments to test the theories put forth in Economics, Psychology, and Sociology. Economic experiments are an ideal method in this arena because the aim of this study is to examine how “other-regarding behavior” affects the decision of whether to enter a tournament. These behaviors are almost impossible to detect with traditional data collection methods because these behaviors can be confounded with other behaviors; for example, there may be environmental concerns that create noise, and questionnaires can be unreliable when subjects are asked to explain why they chose selfish actions.

The main findings of this study are that a significant proportion of subjects, 29%, chose to avoid a tournament, of which 17% do so to avert feelings of compassion and guilt. The 17% were twice as likely to be female and characterized as relatively less competitive. The secondary finding involves analyzing effort levels. In one treatment, the subjects could choose their contracts, while in the other treatment the contracts were chosen for them. When the subjects were free to choose their own contracts, effort levels remained constant between rounds, despite the fact that some contracts had higher monetary incentives. However, when the contracts were forced upon subjects, the monetary incentives became salient and effort responded as predicted.

The essay proceeds with Section 2, in which the experimental design is presented. Next in Section 3 results are presented and the essay concludes with some final thoughts.

[53] This is similar to offering a given worker an additional amount of income to motivate them to accept an undesirable job.

IV.2 Experimental Design

Overview

The previous chapters of this dissertation devoted much attention to the experimental protocol; in this chapter the protocol is left to appendix C.1, and interjections of procedures are included when necessary. To decrease cross-task contamination, subjects only had the instructions that pertained to the current round, and to the overall structure of the experiment. At the commencement of the experiment, subjects learned that the experiment consisted of two Parts (see Figure IV-1 for an overview of the experiment). Part 1 of the experiment consisted of three rounds. The order of the rounds was A – B - C in half of the sessions, and in the other half it was A – C – B. It was necessary for Round 1 (A) to precede Rounds 2 and 3 (B and C), but Rounds 2 and 3 alternated to check for time order effects. Therefore, Round 2 will henceforth be called the IRound (Independent round) and Round 3 will henceforth be referred to as the ORound (Other-regarding round). The subjects were told that Round 1 would not be paid, and that a coin flip would determine whether the IRound would be paid or whether it would be the ORound.[54] None of the subjects were told what choices awaited them, nor did anyone know the details of the future rounds. After the subjects participated in all three rounds, the payoffs from Part 1 were revealed, and then the subject immediately began Part 2. After Part 2 ended, the subjects were shown their cumulative payoffs, and then notified that they could earn an additional $5 for completing a questionnaire. In total, 108 subjects participated, each in only one session. Table IV-1 provides an overview of the subjects' characteristics. In terms of standard demographics, the typical subject was male, approximately 22 years of age, African American, a

[54] Cox et al. (2012) reveal that cross-task contamination may still exist when multiple decisions are required.

Figure IV-1 Overview of Experiment

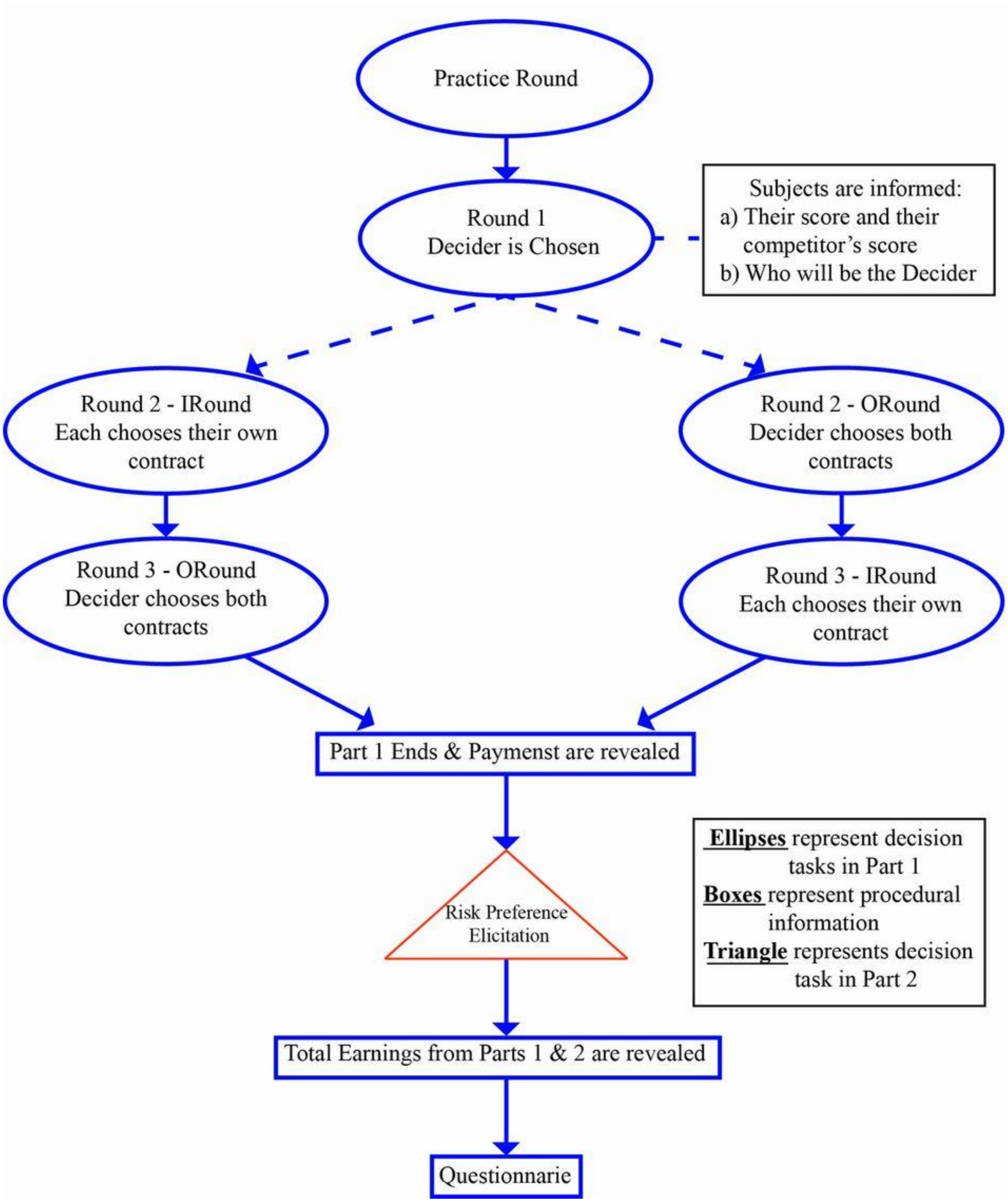


Table IV-1 Participants' Characteristics

	Session 1[1] N	Session 1 Proportion (%)
Gender		
Female	48	44.4
Male	60	55.6
Age (Mean Years)	21.7	—
Race		
African American	65	60.2
Asian	15	13.9
Hispanic	6	5.6
White	14	13.0
Other	9	8.3
Major		
Biological Sciences	22	20.4
Business (Non-Economics)	24	22.2
Economics	8	7.4
Education	5	4.6
Health Professions	4	3.7
Humanities	2	1.9
Math, Computer Sciences, or Physical Sciences;	10	9.3
Psychology	8	7.4
Social Science (Non-Economics or Psychology)	4	3.7
Other	16	14.8
Does Not Apply	5	4.6
Class Standing		
Freshman	6	5.6
Sophomore	24	22.2
Junior	33	30.6
Senior	39	36.1
Other	5	4.6

[1] Total number of subjects equals 108

business major, and an upperclassman. Each session lasted 1.5 hours (from the moment subjects signed in until they were paid), and on average a subject earned $21.15.

Part 1

The design's three-round process was used to determine whether subjects in light of other-regarding preferences chose to avoid tournaments. The experiment began with a practice round, which simulated the task in the three future rounds. The task required the subjects to answer addition questions for six minutes.[55] One point was added to the subject's score for each correct answer, and one point was subtracted for each incorrect answer. The experiment was computerized using the Z-Tree platform (Fischbacher, 2007); scratch paper and pencils were provided, but subjects were prohibited from using a calculator or any other electronic device during the experiment.

After a subject answered a question, a new question immediately appeared on the subjects' computer screen. During the first two minutes of the task, the subjects were required to add two sets of randomly generated numbers between 0 and 100. To ensure increasing marginal cost of effort, once two minutes passed, the computer increased the level of difficulty by requiring the subject to add a third two-digit number. In the remaining two minutes, the computer increased the difficulty once again by adding an additional two-digit number, thus requiring the subjects to compute the sum of four numbers from 0 to 100.

[55] Arithmetic questions have been used in several papers, such as Cason et al. (2010), Eriksson et al. (2008), and Niederle and Vesterlund (2005). This task is commonly used in experiments because performance is not associated with gender, socioeconomic background, or physical conditioning. The task does not involve learning, does require effort (mental concentration), is easily measured, and considerable variability exists across individuals.

Round 1

After the six minute practice round, the subjects proceeded to Round 1. In Round 1 the subjects were randomly paired. Then their relative ability was assessed by having them compete against one another in the addition task. The subjects were not paid in Round 1. The experimenter motivated the subjects by informing them that the subject with the higher score (in each group) would be labeled the "Decider," and that the Decider would earn the privilege of making an important decision that would impact how their group was paid. 108 subjects participated in the experiment, and because each group consisted of exactly two people in which there could only be one winner, 54 subjects would be categorized as Deciders. When Round 1 finished everyone learned their score, the score of their group member, and also their respective roles (Deciders or non-Deciders). Then everyone proceeded to the next round; depending on the experimental session the round was either the IRound or the ORound.

IRound

In the IRound the subjects were told that they would remain paired to the same person as in Round 1 and that they would have to do the addition task again. The subjects were reminded that this round had a 50% chance of being selected for payment, and their payment depended on which contract they chose. Every subject selected the contract they preferred, and their choice was between Contracts A and B. The instructions read:

Contract A: pays $0.80 per point if in Round 2[56] you score higher than your group member
Or $0.00 if you score lower than your group member
Contract B: pays $0.40 per point regardless of whose score is higher.

[56] The instructions never used the term IRound and ORound to refer to Rounds 2 and 3.Also it is important to point out that the contracts remained the same across both rounds.

Notice that since each subject could choose their own contract, each subject had an incentive to truthfully report their preferred individual contract because their choices did not affect either their group member's choices or their group member's earnings. Once all of the subjects made their choices, then the subjects proceeded to the addition task portion of this round. After six minutes the IRound ended, and the subjects moved directly to the next round.

ORound

In the ORound the subjects were told that they would be in the same groups as in Round 1, that they would be repeating the same task, and that if this round was selected, their payment depended on whether contract A or B was chosen. There are two important distinctions between the IRound and ORound. First, only the Deciders would have the opportunity to choose the contracts. And secondly, the subjects did not learn the scores of their group members from the IRound. By not providing the Deciders with this additional feedback, the Deciders only had the information from Round 1 to help them make their decision regarding which contract to choose. Therefore, if a Decider had self-regarding preferences, their contract choice should be consistent with the contract they chose in the IRound.

The Deciders could not communicate with anyone regarding their decision, and they alone chose the contracts for their group. The Deciders were restricted to choosing the same contract for both themselves and their group member. For example, if a Decider chose Contract A, then the subject with the higher score in this round would earn \$0.80 per point, but this would result in the other person earning \$0.00. Had the Decider instead chosen Contract B, then both subjects would earn \$0.40 per point.[57] Notice that the Decider's choice impacts the earnings of

[57] These earnings were only realized if this round was the one randomly chosen at the end of Part 1 for payment.

their group member. A Decider with other-regarding preferences, or more specifically, a Decider who felt compassion (advantageous inequity), may switch her choice because it had a material consequence on the other group member. For example, suppose a subject, confident of their chances of winning the IRound, selected Contract A to maximize their earnings. However, now as a Decider, this subject faces a moral dilemma: if she chooses Contract A again, then she will suffer compassion (guilt) knowing that she has potentially left her group member with nothing from this portion of the experiment. If this compassion is significantly strong, then she will switch her decision and instead choose Contract B.

Part 2

After the third round, the payoffs from Part 1 were revealed to each subject, and the instructions for Part 2 were distributed to the subjects. Part 2 of the experiment was a Holt and Laury (2002) risk elicitation task. For the details of this task, please refer to the Protocol section of this chapter. The purpose of eliciting risk was because Deciders who were categorized as risk averse would, all things being equal, prefer Contract B which ensures a positive payoff proportional to their amount of effort.

Once the task concluded, the subjects' payoffs were revealed and the subjects were informed that there was an optional questionnaire that paid $5. The questionnaire included standard demographic questions (age, gender, etc.) as well as the questions needed for the Revised Competitive Index (Harris & Houston, 2008, 2010). The Revised Competitive Index asks fourteen questions using a 5-point Likert scale in order to measure each subject's enjoyment of competition.[58] Harris and Houston (2010) discuss that competiveness is an enduring and

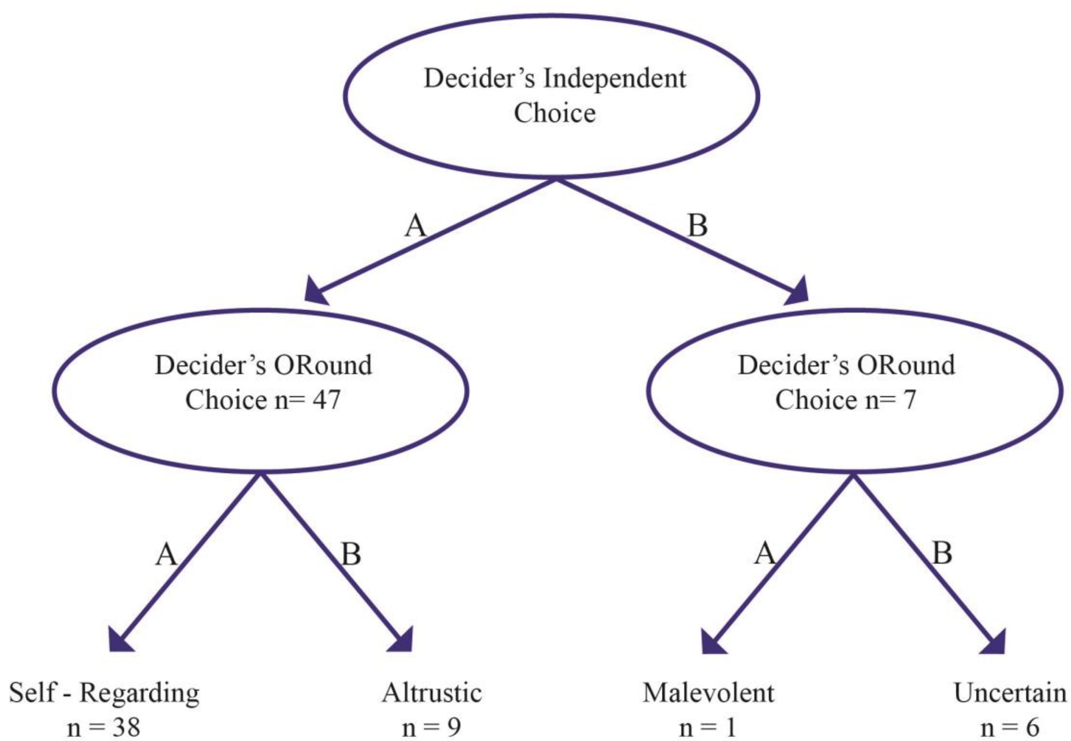
[58] A copy of the complete questionnaire can be found in appendix C.3.

stable personality trait rather than a transient state, and that subjects who score high in this index are more likely to prefer competitive occupations.

Four Types of Subjects: Self-regarding, Altruistic, Malevolent, and Cautious

Based on the choices made by Deciders, the experimental design enabled the experimenter to categorize Deciders into four types. The first is self-regarding. Other-regarding preferences can be of two types; altruistic and malevolent. The Deciders in the fourth type are labeled "cautious" and are comprised of Deciders who chose to play safe. The Deciders' type is determined based on the combination of contract choices over the IRound and ORound; that is, whether they consistently chose Contract A or B or instead alternated between the two contracts; see figure IV-2.

Figure IV-2 Graphical Approach to Determining the Decider's Type



When a Decider chooses Contract A in the IRound, this connotes *confidence* in their ability because they are willing to choose the risky contract that pays $0.80/point if they score higher and $0.00/point if they score lower than their group member. Because in the IRound their choice only affects their own payoff, then this implies that this contract is their preferred contract when the only consideration is their own payoff.

If in the IRound, the Decider chose Contract A, and also chose Contract A in the ORound, then this Decider is characterized as self-regarding. The subject revealed confidence by choosing Contract A in the IRound, but the consideration of their group member's payoff was not sufficient to cause her to switch to Contract B. If on the other hand, a Decider demonstrated confidence in the IRound, but switched from A to B in the ORound, then the Decider is labeled as altruistic. In this case, the other group member's payoff was sufficient to cause the Decider to change her behavior.

Using this same logic, if in the IRound the Decider chose Contract B, which guaranteed $0.40/point, then this must be her preferred contract. And this indicates that her confidence in winning was relatively low because she preferred the lower but guaranteed earnings. If no switch was observed in the ORound, then this is consistent with a subject who prefers to play it safe and is therefore characterized as cautious. If a Decider chose Contract B in the IRound, and then switches to A in the ORound, then this is evidence of malevolent behavior.

Experimental Design Continued:

There are five aspects that need to be considered regarding the experimental design. First, both Contracts A and B[59] result in income inequality. Contract A creates a much larger inequality because it is a winner-take-all tournament that pays the winner a large amount and pays nothing if one loses. Whereas, the inequality from Contract B arises because it is a piece rate that pays each subject based on their own score. Subjects who score higher earn more relative to their group member.

The income inequality consideration could have been eliminated by using fixed prizes rather than uncapped prizes. I considered another approach; for example Contract C could have paid $20 to the winner and $0 to the loser, and D may have paid $10 to both subjects regardless of their score.

Contract C: (tournament) paid $20 to the winner and $0 to the loser
Contract D: (salary) $10 to both subjects regardless of their score.

It is important to note that income inequality arises any time workers are paid according their performance (Neilson & Stowe, 2010). If workers have other-regarding preferences and suffer from compassion or envy, then Contract D may be considered fair because it is egalitarian and pays the same to both subjects. However, this conclusion contradicts "Equity Theory." Equity is said to exist whenever the ratio of a workers' rewards (wages) to inputs (effort) equals the ratio of other workers' rewards and inputs. Because higher effort is not compensated for in Contract D, Equity Theory would find the ratios to be unequal, and therefore conclude D to be inequitable. Neilson and Stowe (2010) apply piece rates to the Fehr and Schmidt (1999) model and show that if subjects compare net incomes (effort costs as well as pay levels) then self-

[59] The only exception is when both subjects have the same score when Contract B is chosen.

interested agents and agents with other-regarding preferences should theoretically choose the same level of effort. The higher effort leads to higher income, but also to higher costs, which leave the social comparisons unchanged.[60]

The second aspect that needs to be considered applies to the effects of an increase in the Decider's score. As the Decider's score increases, it becomes more costly for them to switch to Contract B. If the Decider wins, then they earn $0.40 per point more under Contract A than Contract B. As their score increases, the total amount of extra winnings from Contract A can be substantial, and this would make it harder to act altruistically. Third, when the Decider's score increases then they should grow more confident in their ability to win, and they should be more likely to choose Contract A. Therefore, an increase in the Decider's score causes them to act more selfishly.

Fourth, as the scores of non-Deciders increase, the confidence of Deciders should decrease, and Deciders will more likely choose contract B. And fifth, when the non-Deciders' scores increase, Deciders should feel more compassion since their choices have a greater impact on the non-Deciders. Consider, for example, two scenarios: in Scenario 1, a non-Decider earns a score of 10, whereas in Scenario 2, the non-Decider earns a score of 15. In both scenarios suppose the Decider's score is X, where X is greater than 15. If the Decider chooses Contract A in the ORound and wins the round, then in both scenarios the non-Decider is left with $0.00. However, had the Decider instead chosen Contract B, then the non-Decider would earn $4.00

[60] Neilson and Stowe (2010) consider the case when agents do not consider effort costs and find that agents prefer pay compression. The assumption that agents do or do not consider effort costs does not change any of the findings in my paper, because Contract A creates much greater inequality compared to Contract B. All else the same, an inequality averse agent would prefer Contract B to A. If one believes, as I do, that effort costs should be considered, then this favors using the piece rate contract. In contrast, if one believes that subjects ignore effort costs, then this suggests that Contract D would have also been appropriate.

and $6.00, respectively. Because the Decider has direct control over the contracts, the Decider should feel more compassion in Scenario 2.

The choice between fixed prizes versus uncapped prizes does not affect the Decider's confidence. This exists because the reference point for the Decider is the relative difference in the scores determined from Round 1. The only way to avoid the changes in confidence would have been to withhold the score of her group member. However, this would leave all Deciders with uncertainty concerning their group members' scores, and the Deciders' decisions would be one under uncertainty. With the current design, there is little uncertainty because the scores are stable across rounds. This is made clear during the instructions, and all of the subjects experience this stability prior to choosing any contracts, because all subjects participate in a practice round that lasts 6 minutes and then proceed to Round 1. Although the answers are submitted more slowly in the first couple of minutes of the practice round, by the end the answers are entered at a relatively constant speed, adjusting for difficulty. Much of the learning comes from entering their responses through the computer, which most college students quickly become accustomed to. Therefore, by the time the subjects make their contract decisions in the IRound and ORound, they should be aware that their ability is relatively stable, and since they know their group members' scores, and they may have similar expectations for their group members, then the outcomes do not involve much uncertainty. Even if some uncertainty exists, I still felt that it was preferable than the large amount of uncertainty that would be if the feedback of their group members was not revealed, because the confidence of a Decider can easily be controlled for econometrically.

The increasing cost of altruism (2) and the increasing feeling of guilt (5) could have been eliminated by choosing fixed prizes. With fixed payoffs the monetary differences between any two contracts (e.g. Contacts C and D) would be independent of scores. However, differences with regard to effort will still be present. For example, the higher a Decider's score, the more effort they put forth, and this would remain proportional regardless of the types of contracts chosen.

There are several reasons that justify using uncapped rewards (Contracts A and B). The first reason has been previously mentioned: theoretically, I am on firm ground when subjects take into account effort costs; likewise, both Distributional-Justice and Equity Theory supports the use of piece rates. The second reason was because I wanted to keep effort levels constant across rounds. With fixed prizes (C and D) the complication arises that the rounds may not be comparable. Because Contract D is not based on performance, the optimal effort reduces to zero. Then it cannot be determined if Deciders are switching from Contracts C to D because they suffer compassion or because they are avoiding doing work.

A decision had to be made whether to use uncapped contracts or fixed contracts. Ultimately, I chose uncapped; this is not to say that fixed would have been incorrect. On the contrary, I think it would be interesting to see if modifying this institution changes any of the results.[61]

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[61] However, I do believe that having used a mixture of contracts, such as Contract A with D or Contract C with B, would be troublesome because it adds several confounds. For example, pairing Contract A with D would imply a choice between an uncapped-tournament and a fixed-salary. This introduces uncertainty in Contract A and no uncertainty in B. There are also efficiency considerations that would have to be considered. It would be impossible to tell the subjects prior to the experiment both the details of the size of the prizes in the tournament, and to calibrate them such that the total sums of Contracts A and D come out to be equal. If, on the other hand, Contracts C and B were used, then we would again run into efficiency concerns. There would also be the complication of having to choose a high enough fixed prize to ensure that even high-performing subjects would benefit from entering the tournament.

IV.3 Results

Types of Deciders

The stage is set by analyzing whether there are statistical differences between the four types (self-regarding, altruistic, malevolent, and cautious) of Deciders. As a reminder, the categories were determined by the Decider's decisions throughout the experiment; see Figure IV-2. In the IRound, every subject was free to choose their own individual contract, and their choice did not affect their group member's payoff. If a Decider chose Contract A, then this demonstrated that the Decider was confident. 47 (87%) Deciders were confident and chose Contract A, the riskier but higher paying contract in the IRound. As a reminder in the ORound, only the Deciders were permitted to choose the contract. And the Deciders were restricted by only being able to choose one contract for their group. If a confident Decider chose Contract A in the ORound, then they were categorized as self-regarding. 38 Deciders fit these parameters, which represents 70% of the Deciders.

If, instead, a confident Decider switched from Contract A to B, they were labeled as altruistic. I add more context into the categorizations into "altruism," based on alternative models of other-regarding behavior in appendix C.2. 9 (17%) Deciders were found to be altruistic and changed their contract decisions based on how their choice affected their group member. To provide some meaning, notice that the Decider's choice of contracts in the IRound is similar to the decision a Dictator makes when she determines how much to offer a powerless responder in the Dictator Game.[62] Cherry et al. (2002) examine the generosity of Dictators when the Dictator

[62] As developed by Forsyth et al. (1994), the Dictator Game is a two player game in which Player 1 dictates how to divide a given amount, and Player 2 does not have an opportunity to reject this division. A rational and self-regarding Dictator should not offer anything to Player 2.

is chosen randomly versus being earned through a real-effort task.[63] In the single blind protocol, Dictators share the wealth in about 40% of the observed observations when the Dictator was chosen randomly. While none did so in the low and high stakes treatment when the role of the Dictator was earned rather than given. Only 20% (30%) of the dictators sent positive earnings in the low (high) treatments. In my experiments which are also single blind, the subject who scores the highest in the addition task becomes the Decider. Only when the group members tie is the Decider chosen randomly.[64] This is strong evidence that Deciders are confronted with feelings of compassion when they are making their decisions. In the Dictator game, Dictators may send positive yet unequal offers and potentially reduce their feelings of guilt. On the contrary, the price of acting altruistically in my experiment, a point I discuss further below, is much higher, and yet 17% of the subjects voluntarily switch from Contract A to B.

If a Decider chose contract B, the safe contract, in the IRound when only her payoff mattered, then the Decider was not confident. 6 subjects picked Contract B twice and were therefore labeled as cautious. Only 1 subject switched from Contract B to A, representing a malevolent disposition; however, this subject's response in the questionnaire indicated that it was a mistake and that they had intended to play safe. The types of Deciders are summarized in Figure IV-3.

[63] Subjects earned money by answering questions from a sample section of the Graduate Admissions Test (GMAT)

[64] In a future paper I wish to explore how the changing to randomly choosing the Deciders affects their decisions to act altruistically versus selfish. I would expect that many more Deciders would act selfishly, but I decided to begin with earning the role of Decider because this is the harder of the two situations to find evidence of altruism.

Figure IV-3 Confidence and Types of Deciders

Confident*	Contract A – ORound	Contract B – ORound	Totals
No	1 – Malevolent	6 – Cautious	7
Yes	38 – Self-regarding	9 – Altruistic	47
Total	39	15	54

*Confident indicates that the Decider chose Contract A (Risky Contract) in the IRound

Several tests (Pearson's χ^2, Fisher's Exact test, and McNemar's test[65]) were performed and find that these four groups are statistically different (α = 0.00).

The characterizations of the Deciders into these four types depend on the relative performance being stable throughout the experiment[66] (because it is being assumed that Deciders who choose Contract A are doing so because they are confident and have a strong expectation of winning the following rounds). It is important to see if this expectation is justified by the data. To analyze this, I concentrate on the pairings of the Non-Deciders to their confident Decider. And I calculate how many times the Non-Deciders outperformed their Deciders. There were 47 confident Deciders. Of these Deciders, 40 (85%) of them retained their position in the IRound and 39 (83%) did so in the ORound. It is important to clarify that being confident does not require 100% stability. On the contrary, an element of chance is always present (see Chapter 1 of

[65] Peasron's χ^2 test assesses whether paired observations on two variables, expressed in a contingency table, are independent of each other. It does not work well when the sample size is small and cells are less than 10. For this reason I also performed Fisher's Exact test, which can be used when determine if there are nonrandom associations between two categorical variables. McNemar tests the marginal frequencies of two binary outcomes. These binary outcomes may be two outcome variables from a single group. This test is appropriate because each Decider is making two binary decisions.

[66] More accurately it depends on the Deciders' subjective beliefs that relative performance is stable. However, I do not have a measure on their subjective beliefs.

this dissertation); and no outcome is guaranteed. When a Decider and Non-Decider's ranking switched, that is when a Non-Decider's score exceeded their Decider's score, the median difference was 2 points. This narrow margin coupled with an 83% - 85% stability makes it reasonable to believe that Decider's confidence was justified. For example consider what subject 51 said: "I was expecting the other group member to get a lower score, so I chose contract B to avoid forcing them to get $0.00 in the event that this happened. I did not want my decision to prevent them from earning money".

Multinomial Logistic Regression

Next we try to explain what is driving the differences between the four mutually exclusive types. Because there is no clear ordering of the characteristics, the multinomial logit model is an appropriate model. Table IV-2 reports the estimations when the dependent variable is the Deciders type using the relative risk ratio (RRR) command. Relative risk ratios describe the multiplicative effect of a unit increase in each predictor on the odds of the Decider of being of a particular *Type* (altruistic/cautious) versus self-regarding. For example, the first regressor is NDS, the normalized difference between the Decider's score and their group member's score in Round 1.[67] A sample distribution of NDS is provided in Figure IV-4; as one can see NDS has a heavier-than-normal right tail (positive skewness) indicating that there were relatively few high values of NDS.

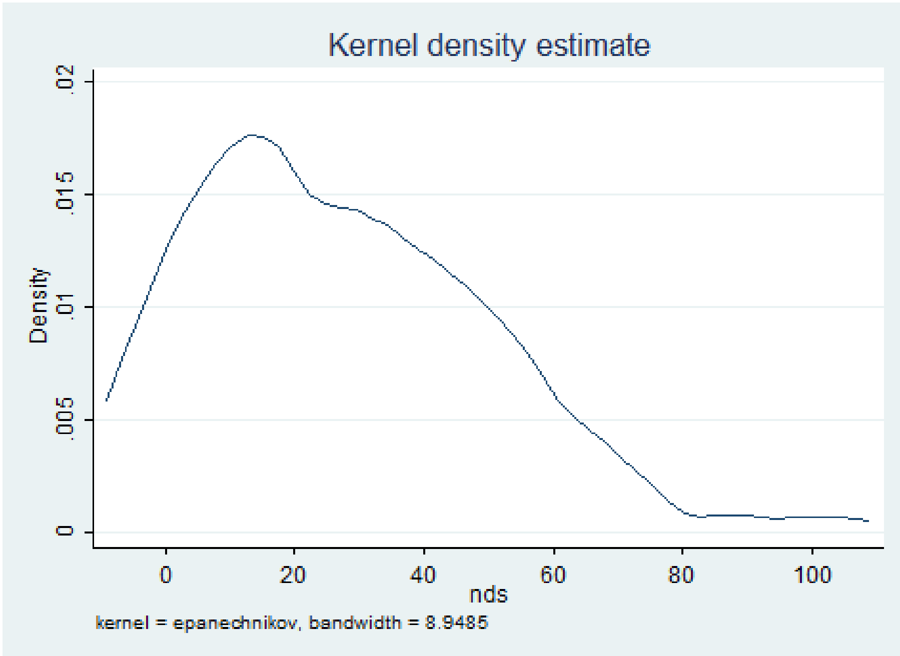
[67] The NDS is calculated by subtracting the Decider's Round 1 score by her respective group member's score also from Round 1. We then divide this number by the largest Round 1 difference in the sample (34), and the final step is to multiply by 100. This generates a number between (0, 100). Zero indicates that the Decider and her group member tied; whereas, an NDS of 100 indicates that the difference between the Decider's score and her group member was 34 points.

Table IV-2 Results from Multinomial-Logit Regressions

		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(2) Decider Altruistic	NDS (Normalized Difference Score)	0.968	0.968*	.967	.967*	0.949	0.949	0.949	0.949
		(0.135)	(0.081)	(0.155)	(0.094)	(0.115)	(0.162)	(0.115)	(0.156)
	HighComp (Highly Competitive == 1)	--	--	0.127**	0.127**	0.140*	0.140*	0.138*	0.138*
				(0.021)	(0.023)	(0.055)	(0.089)	(0.059)	(0.068)
	Female ==1	--	--	--	--	8.55**	8.55*	9.56**	9.56
						(0.042)	(0.070)	(0.041)	(0.138)
	Black ==1	--	--	--	--	6.95*	6.95*	7.599*	7.599
						(0.057)	(0.103)	(0.055)	(0.164)
	Risk Averse ==1	--	--	--	--	--	--	1.474	1.474
								(0.763)	(0.833)
	Constant	0.569	0.569	1.47	1.47	0.271	0.271	0.181	0.181**
		(0.374)	(0.344)	(0.618)	(0.598)	(0.259)	(0.111)	(0.342)	(0.002)
(3) Decider Cautious	NDS (Normalized Difference Score)	0.713*	0.713**	0.716*	0.716**	0.535*	0.535***	0.523*	0.523***
		(0.057)	(0.035)	(0.057)	(0.034)	(0.104)	(0.006)	(0.103)	(0.004)
	HighComp (Highly Competitive == 1)	--	--	0.688	0.688	0.794	0.794	0.940	0.940
				(0.755)	(0.757)	(0.873)	(0.866)	(0.968)	(0.958)
	Female ==1	--	--	--	--	56.07*	56.07**	55.36*	55.36**
						(.077)	(.045)	(0.087)	(0.035)
	Black ==1	--	--	--	--	2.19	2.19	1.88	1.88
						(0.563)	(0.572)	(0.654)	(0.657)
	Risk Averse ==1	--	--	--	--	--	--	0.652	0.652
								(0.759)	(0.740)
	Constant	3.59	3.59	04.44	04.44	3.162	3.162	4.278	4.278
		(0.186)	(0.128)	(0.243)	(0.224)	(0.465)	(0.262)	(0.457)	(0.352)
	R2								
	Observations	53	53	53	53	53	53	53	53
	Heteroskedasticity	NO	YES	NO	YES	NO	YES	NO	YES
	Autocorrelation		YES		YES		YES		YES

Notes: ***, **, * denote statistically significant effects at the 1%, 5%, and 10% level, respectively.

Figure IV-4 Distribution of NDS the Normalized Difference Between the Decider's Score and their Group Member's Score



There are eight regressions reported in Table IV-2. The regressions labeled with odd numbers are the standard multinomial logistic model using the RRR option. The even number regressions control for heteroskedasticity, using the *robust* command,[68] which relaxes the assumption of independent, identically distributed errors. The first row in Table IV-2 shows that NDS has a RRR of approximately 0.96. This indicates that the odds of a Decider being categorized as altruistic decreases by 4% (multiplied by 0.96) with each 1-unit increase in NDS. Although NDS is only marginally significant, it suggests that Deciders take into account the relative difference between their scores and their group members' scores when deciding to act

[68] Regressions were also run using the *cluster(Subject)* command which relaxes the independent assumption, when errors are correlated within subgroups. Since the data is being generated by the same subject across different rounds it is possible that there is autocorrelation. The coefficients and standard errors are identical to the specification using the *robust* command; therefore, there was no need to report these regressions separately.

altruistically. A large NDS indicates that there is a relatively large difference between the Decider's score and her group member's score. All other things being equal, a large difference increases the cost of acting altruistically, either because the Decider's score was higher than average and therefore it was relatively costly to switch to Contract B, or because the group member's score was lower than average, indicating that the Decider should feel less compassion because the group member's total benefit from Contract B is relatively low.

In the first row of the second sub-table, we see that NDS is a strong predictor for determining whether a Decider will behave cautiously or selfishly. As the NDS increases, the risk associated with Contract A decreases because a Decider's likelihood of winning increases, and consequently a Decider should feel more confident. This conclusion is supported by the data; a 1-unit increase, all other things being equal, reduces the likelihood by 29% and 48% in the first and last specification, respectively.

All of the other regressors in Table IV-2 are dummy variables, including the next variable, *HighComp*, which takes on the value of 1 when the Decider is categorized as highly competitive.[69] Highly competitive Deciders are significantly less likely to be categorized as altruistic; however, HighComp does not explain if a subject chooses to behave cautiously versus selfishly. It is consistent that Deciders who enjoy competition have a higher tolerance for compassion (feel less guilty). There is evidence that subjects who score high on the Revised Competitiveness Index are also more likely to engage in competitive activities.[70] Perhaps having a greater exposure to competitive situations has desensitized these Deciders to feelings of compassion. Tournaments rely on the Darwinian philosophy that the strongest, most able person earns the prize. It seems logical that this philosophy would resonate with individuals who score

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[69] The Deciders were ranked according to their *"enjoyment of competition"* score, a subscale, in the Revised Competitiveness Index. The Deciders in the top half were categorized as highly competitive.

[70] See Harris and Houston (2010) for an overview of the Revised Competitiveness Index.

high on the Revised Competitiveness Index, and would therefore act more selfishly than those who score lower.

In Columns (5) and (6) of Table IV-2, we now replicate the previous analysis, but additionally control for demographic information as it relates to gender and race. Females as well as blacks are much more likely to be categorized as altruistic rather than selfish, but only *female* is a significant explanatory variable for determining between cautious and selfish types. One-third of the Deciders were female, and finding a gender difference as it relates to altruism is a quite interesting result.

Many economic experiments have emerged using gender differences as an explanatory variable, but no consensus has been reached.[71] A natural starting place would be the dictator game,[72] but some studies find no results, while others find varying results with either sex viewed as more altruistic. However, two studies have substantial similarities to my experiment and therefore may be able to shed light on the gender differences found herein. The first is Andreoni and Vesterlund's (2001) experiment on gender differences in altruism. They use a modified dictator game with varying incomes and prices. Taken as a whole, men on average passed $2.56, while women passed $2.60. This difference was not statistically different. However, when the price of giving is greater than or equal to one, women pass significantly more than men. The second is Cox and Deck's (2006) study which reconciles many of the disparate findings from previous research. Their study focuses on the cost of generous actions. They begin by realizing that the monetary costs of giving are not the only possible sources of utility or disutility. Cox and Deck incorporate a richer notion of the cost of generous actions by considering the costs of

[71] For a comprehensive review of the results on gender differences, see Croson and Gneezy (2009).

[72] The dictator game is played with two participants who are randomly paired. The allocator must decide how to split a fixed amount of money, such as $10. The receiver does not have a chance to veto. Therefore, the allocator acts as a dictator.

giving across several contexts; for example, they vary the payoff, social distance, and the possibility for reciprocal behavior. Their main conclusion is that women are more responsive to changes in the environment, whereas men are more responsive to the actions of others.

In my experiment the lower bound price was equal to one. This was the case when the Decider tied with their group member in Round 1. Remember, the only information the Deciders had before making their decision was from Round 1. Note that in my experiment for a Decider to be classified as altruistic, they must demonstrate confidence by choosing Contract A in the IRound. Therefore, if a Decider chose Contract B in the ORound, then they were essentially acting as a dictator because they were giving up half of their expected pay in order to give their group member half. This would make the price of giving equal to one. But since there was no instance where a Decider tied with their group member and acted altruistically, then it must be the case that the Decider scored higher than their group member. In this case the Deciders were giving up larger expected payoffs, than their group member was receiving. Therefore, my results are consistent with, Andreoni and Vesterlund (2001) who find that women act more altruistically but only when the price of giving is greater than one.

As to the findings by Cox and Deck (2006), the strongest similarity is that when there is an absence of reciprocal motivation; in other words, the other player cannot respond to the action of the first player, regardless if these actions were generous or ungenerous, then women behave more altruistically. This is consistent with the design of my experiment because the non-Deciders do not have the option to reciprocate towards the Decider.[73] Cox and Deck (2006) find that women behave more altruistically in the low payoff treatment, where the Dictator chooses between the ungenerous action ($0, $20) and the generous action ($7.5, $12.5). These payoffs

[73] If Non-Deciders increase or decrease their effort based on the actions of the Decider, then this could be considered a type of reciprocal behavior.

are remarkably similar to my experiment where the Decider also makes a binary choice. Contract A most likely leaves the non-Decider with $0, but leaves the Decider earning the maximum amount $0.80 per point (on average $24). With Contract B the Decider will again most likely earn a bit more than the non-Decider because of their greater ability (the average amounts were $9, $11). Lastly, Cox and Deck (2006) indicate that women tend to be more generous when the social distance is low. My experiment uses the same protocol as in their low social distance treatment which uses a single blind protocol, which may further explain why we find women acting more generously.

Blacks were more likely to be categorized as altruistic rather than selfish. This is a somewhat puzzling finding. Van Der Merwe and Burns (2008) used a dictator game in South Africa to determine if race and racial identity mattered. They find white proposers make significantly higher offers than black proposers, and black subjects only slightly increased their offers (and was not significant) when race was revealed, even when the black proposer believed the recipient was also black. In my experiment, half the Deciders were black and 70% of the non-Deciders were black. Although no subject (Decider or Non-Decider) was given demographic data, it stands to reason that they could observe that the majority of the subjects were black. Perhaps the discrepancy between the findings is that American students behave differently than South African students and have a greater sense of camaraderie.

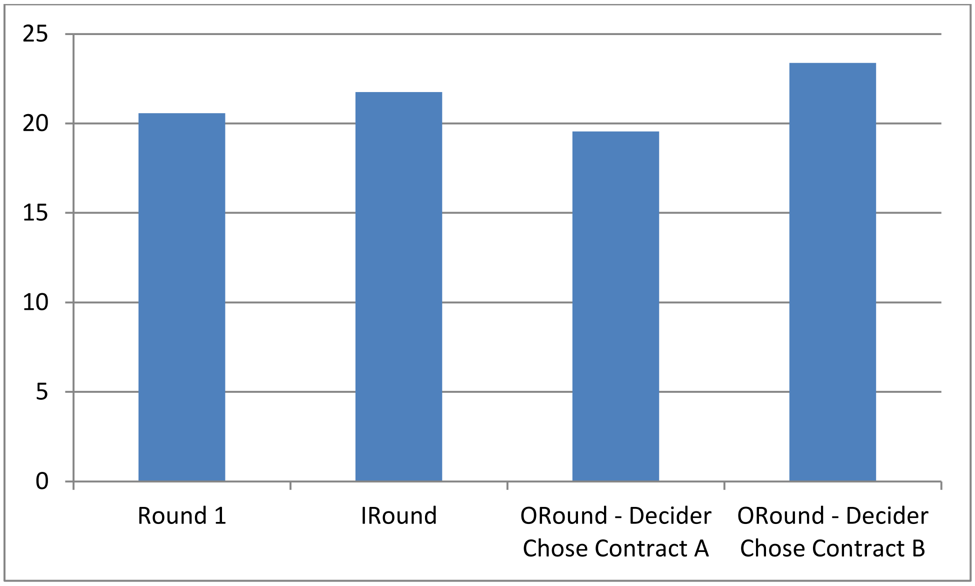
With regard to being categorized as cautious, the coefficient associated with Black is greater than 1. This indicates that being black tended to increase the likelihood that a Dictator would act cautiously, but this variable is not significant. I analyzed if blacks made different choices in the Holt Laury (2002) procedure, but like Harrison and Rutström (2008) I did not find significant differences. I do not associate risk aversion with altruistic behavior; therefore, when I include *Risk Averse* in the model in the last two columns (7) and (8), I am not surprised when it is not found to be a significant predictor of altruistic behavior. However, I would expect that risk aversion would be significant as it pertains to predicting if a Dictator will behave cautiously, because this has to do with their confidence. I would think that risk averse subjects would, all other things being equal, be more likely to take the guaranteed but lower payoffs of Contract B. But again, Risk Averse was not significant. This is most likely associated with the low power associated with this variable. There were only seven risk averse Deciders who behaved altruistically and four who behaved cautiously. Perhaps if the sessions were run with more subjects, this variable would have more explanatory power.

Effort of Non-Deciders

The practice of performance-based rewards (as in tournaments and piece rates) is used to insure that the contracts are salient to maximize effort. Therefore, I would be remiss not to analyze changes in effort. We begin by examining the effort of Non-Deciders. In Figure IV-5, we see that effort in the IRound was only slightly higher than Round1 and was not statistically different (P-Values for Wilcoxan sign rank[74] $\alpha = 0.1087$). This indicates that although Round 1 did not offer a monetary payoff, Round 1 was salient since they put in a substantial amount of effort.

[74] The Wilcoxon signed rank is a nonparametric test that does not require normality. Kernel densities and the Shapiro-Wilk W test for normality were performed and confirmed that the variables were not normally distributed

Figure IV-5 Effort of Non-Deciders



In the ORound the Decider chose either Contract A or B, and then the addition task followed. On the one hand, when a Decider chose Contract A, this caused Non-Deciders to reduce their effort,[75] and consequently their scores decreased. On the other hand, when a Decider chose Contract B, Non-Deciders increased their effort and this increase was statistically significant ($\alpha = 0.088$). This is consistent with economic theory, Equity Theory, and Distributional Justice. These theories predict that agents who expect to lose should reduce their inputs (effort) since their effort will go unrewarded, while on the other hand, maintain at least as much effort when Contract B is chosen because the relative ratios between effort and inputs are more equal.

[75] Although not statistically significant, there was both a reduction in the number of questions attempted and a reduction in the accuracy.

Effort of Deciders

We now examine the effort levels of Deciders. As noted above, the Deciders were characterized into three[76] types: self-regarding, altruistic, and cautious. There were no statistical differences in effort levels between the IRound and ORound. However, this is not surprising for the following reasons. First, the task was specifically chosen because there is little learning from one round to the next. Second, both contracts (A and B) are performance based. And third because each Decider chose their respective contract; the Deciders retain the locus of control,[77] and effort should remain constant over the two rounds.

Both the altruistic and cautious Deciders did not show an improvement in the IRound and ORound compared to Round 1 (see figures IV-6 and IV-7). This is the same non-result we found above for non-Deciders. Perhaps these Deciders did not require the additional monetary incentive to motivate full effort. And as expected since the Deciders had direct influence over their contracts, and all contracts were salient, there were no statistical differences between the IRound and ORound.

[76] The fourth type, malevolent, was dropped because there was only one Decider in this type, and it is believed that this person made a mistake.

[77] The locus of control framework is used to explain whether the outcomes (level of pay) of an individual are controlled internally or externally. The Deciders have an internal locus of control because they believe (or are justified to believe) that they are in control of their own fate because they are allowed to choose their own contracts (Stajkovic & Luthans, 1998, 2003).

Figure IV-6 Effort of Altruistic Deciders

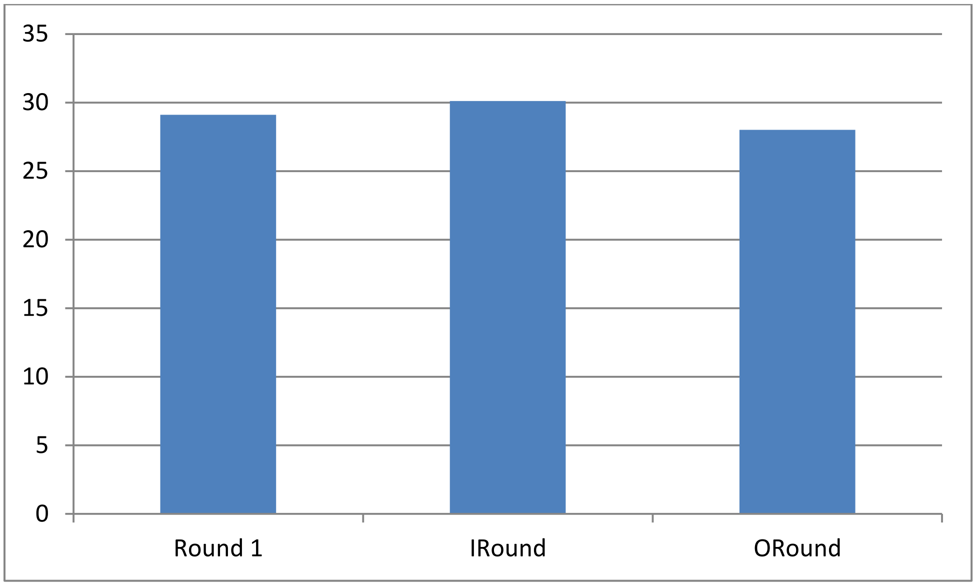
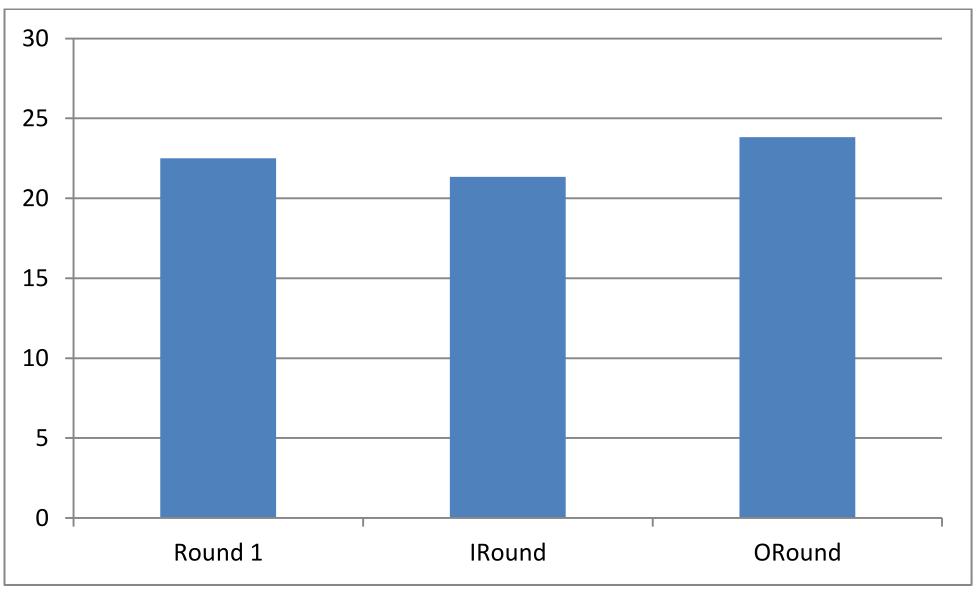
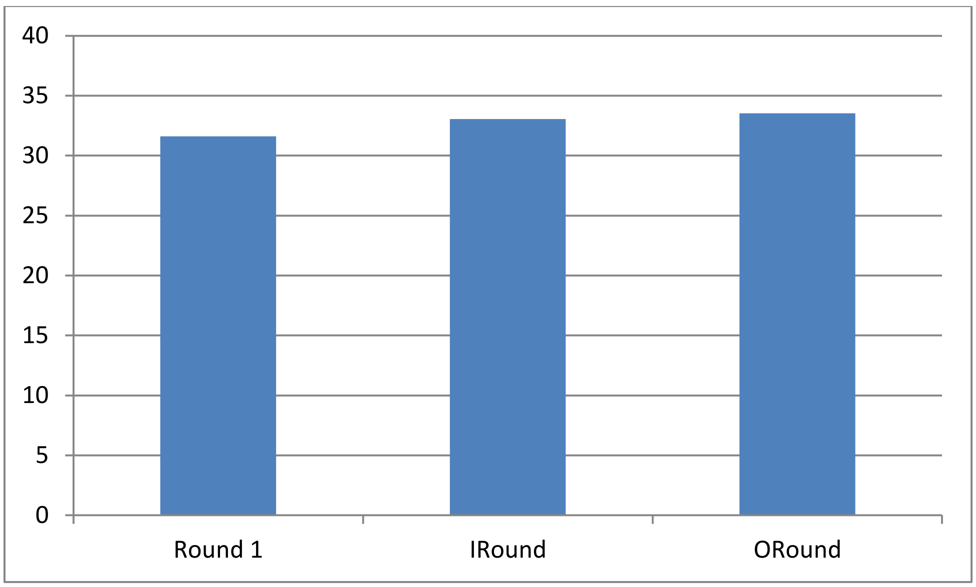


Figure IV-7 Effort of Cautious Deciders



Unlike the other subjects, the Self-regarding Deciders did show an increase in the IRound and ORound compared to Round 1 (α = 0.04 & 0.06). See Figure IV-8. It remains unclear why these subjects increased their scores in the latter two rounds. Perhaps, these rounds were more important because they had monetary rewards, or the improvement may have been the result of additional learning. Since the Deciders had direct influence on their contracts and retained the locus of control in both the IRound and ORound, we do not expect any differences between these rounds, and this is confirmed by the data.

Figure IV-8 Effort of Self-Regarding Deciders



IV.4 Conclusion

Most tournament models rely on subjects having self-regarding preferences. This paper shows that 17% of Deciders voluntarily switch from the higher paying contract to the contract that distributes the earnings more evenly. This indicates that many Deciders would have preferred the higher paying contract, but were not willing to choose this contract if it meant that they were going to leave their group member with zero earnings from this portion of the experiment.

We found that competitive people were much less likely to act altruistically. If people are sorted into occupations that are competitive versus non-competitive, then the workers in competitive environments are also those who feel less compassion. Whether this is a favorable trait for the organization depends on the business culture and the interaction of workers the company hopes to foster. We also found that women act both more altruistically and more cautiously than their male counterparts. Perhaps this is why we do not see as many women as men in highly competitive industries.

Lastly, we found that the effort of Deciders was relatively stable from one round to the next, while that of non-Deciders was dependent on the actions of the Deciders. When Deciders acted selfishly, non-Deciders reduced their effort, while the opposite was true when Deciders acted altruistically. There are many theories that point to interdependent utilities; therefore, it should not come as a surprise that Deciders choose to act more generously in the rounds where the actions have ramifications on others, nor should it be surprising that the effort of the non-Deciders vary in response to the actions of Deciders.

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Appendix A

APPENDIX TO CHAPTER II

A.1 Subject Instructions for How is Effort Affected When Luck Impacts Outcomes

Introduction:

Thank you for volunteering to participate in this experiment.

The decisions you will be asked to make will be explained in subsequent instructions.

You will earn money, which will be paid to you in cash at the end of the experiment.

Complete Privacy:

This experiment is set up so that no one, including the experimenters and the other participants, will ever know the decisions or earnings of anyone participating today.

You will collect your earnings, from a numbered mailbox for which only you will have the key.

Your privacy is guaranteed because neither your name nor your student ID number will appear on any form that records your decisions or your earnings. You are the only person who will know your mailbox number.

Random Pairing:

The experiment will begin by assigning all participants to a group, and each group will consist of 2 people. All assignments will be randomly determined by the computer. As mentioned in the privacy statement above, your identities will remain private, but your group member will be someone from this room. Both of your final payoffs will depend in part on your own decisions, but also on the decisions of your group member.

General Instructions:

The experiment consists of 2 Parts. Part 1 has 6 rounds and Part 2 has only 1 Round. Each round is independent of the other rounds and will not affect either previous or later rounds. You will only be paid for **1** round from each Part. Since Part 1 has multiple rounds, I will roll a die randomly selecting which round everyone will be paid on for Part 1.

We will begin the experiment with each person selecting a ball from a bucket. I will show everyone that there are an equal number of Red, White, and Blue balls in the bucket. Therefore, each person has the same likelihood of picking a certain color. Before I explain the purpose of the balls, I would like to explain the nature of the tasks for the first 6 rounds.

Part1 (Rounds 1-6):

The task involves solving simple arithmetic questions. You will have 5 minutes to answer questions. Each question will be in the following format:

$$12 + 45 + 50 =$$

Notice that each question will involve three separate numbers ranging from 01 to 99.

When you have solved the problem simply type your answer into the box and click submit.

The computer will then give you a new question to answer.

Each of you has scratch paper and a pencil to help you; however, you will not be allowed to use a calculator or any other electronic device.

Scoring:

You will get one point for each correct answer and minus one point for each incorrect answer.

For example if in 5 minutes you answered 3 questions correct and 2 questions incorrect then your score is equal to 1 (3 – 2).

Luck:

Depending on the round you are in, there will be an element of "Luck."

Luck will affect your score!

For some questions, the computer will predetermine if the answer to the question is correct "Good Luck" or incorrect "Bad Luck"; therefore, the answer you submit to these questions has no bearing on whether the computer will score the question as correct or incorrect.

This is because whether the computer grades the questions as correct or incorrect will be decided randomly before you have even answered the question.

You will not know beforehand if the question will be affected by "Luck;" however once you have submitted your answer a message will appear that will read:

- "Good Luck has affected your Score, one point has been added"
- "Bad Luck has affected your Score, one point has been deducted"
- If no message appears then this question was not affected by "Luck."

You will know prior to starting the round the likelihood that luck will play a role in that round, and everyone in the room will have the same degree of "Luck." "Luck" will change from round to round, but not within a single round.

Example:

Suppose the "Luck" element is set to 0.33. Then on average 1 out of 3 questions will be determined by "Luck." A question that is affected by "Luck" has an equal likelihood of being scored as correct or incorrect.

On the other hand if the "Luck" is set to 0.0. Then there is no "Luck" and all the questions will be scored correct or incorrect based on your submitted answer.

Contracts:

Shortly you will select one ball from a container. Each ball has an equal chance of being chosen and will determine which contract type you are in during the experiment.

To ensure fairness I will not tell you beforehand which ball is associated with which type of contract until everyone has selected a ball and entered their color into the computer program. However, you will be told your contract type prior to the start of Round One.

And, you will always be grouped with someone who chose the same color ball as you did. You will remain in this contract through all 6 rounds of Part 1.

<u>The 3 Contract types are:</u>

Contract 1: The person with the lower score will get $10 (Low Prize) and the person with the higher score will also get $10 (High Prize).

Contract 2: The person with the lower score will get $6 (Low Prize) and the person with the higher score will get $14 (High Prize).

Contract 3: The person with the lower score will get $2 (Low Prize)and the person with the higher score will get $18 (High Prize).

Example:

Suppose you and the other person in your group chose a ball that is associated with Contract 2, and that you answered 7 correct and 2 incorrect, then your score would be 5. And suppose the other person had a score of 4. Then if this round is selected you would receive $7 and the other person would receive $3.

Ties: If in a given round, you and the other person should have the same score, then the computer will randomly decide which of you will receive the "Low prize" and which one of you will receive the "High Prize" depending on the contract you preselected.

Payments on Rounds 1-6 depend on 4 Things:

1) Which round is randomly selected for payment at the end of Part 1.

2) Your Score within the selected Round

3) Your Group member's score in the Round

4) The contract type of the round

Layout of the Screen:

On the following page is a screenshot of what your screen MAY look like.

Middle of the screen:

- The Current Question is displayed
- Along with a box where you will submit your answer to this question
- Luck message if it applies
- You will have to click the "Submit" button to lock in your answer

Top left corner of the screen:

- A summary of your progress Within the current round which has the following information:
    - The previous Question along with the correct answer
    - Followed by the answer you submitted
    - The number of "Correct" & "Incorrect" questions you have submitted up to that point
    - Followed by your Current Score

Top Right Corner of the screen:

- The Time remaining in the Round is displayed. When the timer reaches zero the round will end and you will not be able to answer any more questions in this round.
- A history of your progress Across rounds, which will display if you will receive the "Low Prize or the High Prize" if that round is selected.

Remaining time [sec]: 26

Previous Question	45	+	89	+	48	=	182

Your Answer	32
Current Correct	0
Current Missed	1
Current Score	-1

Rounds	If this round is seleced you will receive the:
1	Low Prize
2	High Prize
3	High Prize
4	Low Prize
5	High Prize

99	+	31	+	48	=	

Luck:
1) No Message
2) Bad Luck has affected your Score, one point has been deducted
3) Good Luck has affected your Score, one point has been added

OK

Part 2: (Note to reader, the subjects will not be given these Instructions until after Part 1 is completed)

Below you see a screenshot of this task. As you can see you will have to make 10 decisions, deciding between "Option A" and "Option B." You will make your 10 decisions by clicking on your choice in the final column. But only one will be used in the end to determine your earnings. Please allow me to explain how these choices will affect your earnings for this part of the experiment.

The First Roll:

Notice that in the 1st column labeled "Decision from 1st Die" there are 10 Letters Q – Z. And that each of letter is associated with a specific Die number. Everyone will have the same 10 Decisions labeled Q – Z and in the order seen below; however, the Die number associated with each letter may be different. For example in the first Row Q is associated with a roll from the 1st Die = 5 someone else may have Q associated with another roll. The die is used to select which one of the ten decisions will be used. And although the numbers may not be in the order as shown in this example, no die number will ever be repeated so that every row has an equal chance of being picked.

The Second Roll:

The 2nd role will be used to determine what your payoff is for the option you chose, A or B, for the particular decision selected

Now, please look at Decision 1 at the top. Option A pays $6.00 if the throw of the ten sided die is 1, and it pays $4.80 if the throw is 2-10. Option B yields $11.55 if the throw of the die is 1, and it pays 0.30 if the throw is 2-10. The other Decisions are similar, except that as you move down the table, the chances of the higher payoff for each option increase. In fact, for Decision 10 in the bottom row, the die will not be needed since each option pays the highest payoff for sure, so your choice here is between $6.00 or $11.55.

To summarize, you will make ten choices: for each decision row you will have to choose between Option A and Option B. You may choose A for some decision rows and B for other rows, and you may change your decisions and make them in any order.

Please raise your hand if you have a question.

Decision From 1st Die	Option A From 2nd die	Option B From 2nd die	Please make your Decisions
Q Die = 5	$6.00 if the die is 1 or $4.80 if the die is 2-10	$11.55 if the die is 1 or $0.30 if the die is 2-10	Option A Option B
R Die = 3	$6.00 if the die is 1-2 or $4.80 if the die is 3-10	$11.55 if the die is 1-2 or $0.30 if the die is 3-10	Option A Option B
S Die = 8	$6.00 if the die is 1-3 or $4.80 if the die is 4-10	$11.55 if the die is 1-3 or $0.30 if the die is 4-10	Option A Option B
T Die = 9	$6.00 if the die is 1-4 or $4.80 if the die is 5-10	$11.55 if the die is 1-4 or $0.30 if the die is 5-10	Option A Option B
U Die = 1	$6.00 if the die is 1-5 or $4.80 if the die is 6-10	$11.55 if the die is 1-5 or $0.30 if the die is 6-10	Option A Option B
V Die = 10	$6.00 if the die is 1-6 or $4.80 if the die is 7-10	$11.55 if the die is 1-6 or $0.30 if the die is 7-10	Option A Option B
W Die = 2	$6.00 if the die is 1-7 or $4.80 if the die is 8-10	$11.55 if the die is 1-7 or $0.30 if the die is 8-10	Option A Option B
X Die = 4	$6.00 if the die is 1-8 or $4.80 if the die is 9-10	$11.55 if the die is 1-8 or $0.30 if the die is 9-10	Option A Option B
Y Die = 7	$6.00 if the die is 1-9 or $4.80 if the die is 10	$11.55 if the die is 1-9 or $0.30 if the die is 10	Option A Option B
Z Die = 6	$6.00 if the die is 1 - 10	$11.55 For Sure	Option A Option B

Submit

A.2 Questionnaire

Instructions: Use the following response scale in answering the items below. Make sure to read each item carefully and circle the number that best represents your answer.

1 = Strongly Disagree
2 = Slightly Disagree
3 = Neither Disagree Nor Agree
4 = Slightly Agree
5 = Strongly Agree

1.	I get satisfaction from competing with others.	1	2	3	4	5
2.	I am a competitive individual.	1	2	3	4	5
3.	I will do almost anything to avoid an argument.	1	2	3	4	5
4.	I try to avoid competing with others.	1	2	3	4	5
5.	I often remain quiet rather than risk hurting another person.	1	2	3	4	5
6.	I find competitive situations unpleasant.	1	2	3	4	5
7.	I try to avoid arguments.	1	2	3	4	5
8.	In general, I will go along with the group rather than create conflict.	1	2	3	4	5
9.	I don't like competing against other people.	1	2	3	4	5
10.	I dread competing against other people.	1	2	3	4	5
11.	I enjoy competing against an opponent.	1	2	3	4	5
12.	I often try to out perform others.	1	2	3	4	5
13	I like competition.	1	2	3	4	5
14.	I don't enjoy challenging others even when I think they are wrong.	1	2	3	4	5

15. What is your AGE?
16. What is your sex?

- Male
- Female

17. Which of the following categories best describes you?

- White
- African-American
- African
- Asian-American
- Asian
- Hispanic-American
- Hispanic
- Mixed Race
- Other

18. What is your major?

- Accounting
- Economics
- Finance
- Business Administration, other than Accounting, Economics, or Finance
- Education
- Engineering
- Health Professions
- Public Affairs or Social Services
- Biological Sciences
- Does not apply
- Math, Computer Sciences, or Physical Sciences
- Social Sciences or History
- Humanities
- Psychology
- Other Fields
- Does not apply

19. What is your class standing?

- Freshman
- Sophomore
- Junior
- Senior
- Masters
- Doctoral
- Does not apply

20. Please select the category below that best describes the total amount of INCOME earned in 2010 by the people in your household. Include yourself, your spouse and any dependents. Do not include your parents or roommates unless you claim them as dependents. Consider all forms of income, including salaries, tips, interest and dividend payments, scholarship support, student loans, parental support, social security, alimony, and child support, and others.

- Don't Know
- $15,000 or under
- $15,001 - $25,000
- $25,001 - $35,000
- $35,001 - $50,000
- $50,001 - $65,000
- $65,001 - $80,000
- $80,001 - $100,000
- 100,001 - $150,000
- over $150,000
- Prefer not to answer

21. Do you work part-time, full-time, or neither?

- Part-time
- Full-time
- Neither

A.3 Theory and Proofs

Theoretical Prediction 1: As remuneration spreads $(W_h - W_l)$ increases, subjects respond by increasing their effort.

Proof:

We begin with the equilibrium result that

$$\left(\frac{1}{\sigma\sqrt{2\pi}}\right)(W_h - W_l) = C'(e_i)$$

Let,

$\alpha^a = (W_h^a - W_l^a)$ and $\beta^a = C'(e_i)$

$\alpha^b = \left(W_h^b - W_l^b\right)$ and $\beta^b = C'(e_i)$

$k = \left(\frac{1}{\sigma\sqrt{2\pi}}\right)$

These expressions can be simplified so that:

$$\alpha^a k = \beta^a$$

$$\alpha^b k = \beta^b.$$

If $\alpha^b > \alpha^a$, then $\beta^b > \beta^a$, and this is true if and only if $e_i^b > e_i^a$, since marginal cost is increasing, which concludes the proof.

Theoretical Prediction 2: As luck (represented by σ) increases, subjects respond by decreasing effort.

Further, given that σ is found in only one place in (3), the marginal cost of effort must decrease (see appendix for proof).

Proof:

Let,

$\gamma^a = \left(\frac{1}{\sigma^a\sqrt{2\pi}}\right)$ and $\beta^a = C'(e_i)$

$\gamma^b = \left(\frac{1}{\sigma^b\sqrt{2\pi}}\right)$ and $\beta^b = CC'(e_i)$ where $\gamma^a > \gamma^b$

$k = (W_h - W_l)$

These expressions can be simplified so that:

$\gamma^a k = \beta^a$

$\gamma^b k = \beta^b$

When $\gamma^a > \gamma^b$, then it follows that $\gamma^a k < \gamma^b k$, which implies that $\beta^a < \beta^b$. And this is true if and

only if $e_i^a < e_i^b$, since marginal cost is increasing, which concludes the proof.

Appendix B

APPENDIX TO CHAPTER III

B.1 Subject Instructions for How Deviant Behavior Affects Effort

Introduction:

Thank you for volunteering to participate in this experiment.

The decisions you will be asked to make will be explained in subsequent instructions.

You will earn money, which will be paid to you in cash at the end of the experiment.

Complete Privacy:

This experiment is set up so that no one, including the experimenters and the other participants, will ever know the decisions or earnings of anyone participating today.

You will collect your earnings, from a numbered mailbox for which only you will have the key. Your privacy is guaranteed because neither your name nor your student ID number will appear on any form that records your decisions or your earnings. You are the only person who will know your mailbox number.

Random Pairing:

The experiment will begin by assigning all participants to a group, and each group will consist of 2 people. All assignments will be randomly determined by the computer. As mentioned in the privacy statement above, your identities will remain private, but your group member will be someone from this room. Both of your final payoffs will depend in part on your own decisions, but also on the decisions of your group member.

General Instructions:

The experiment consists of 2 Parts. Part 1 has 6 rounds and Part 2 has only 1 Round. Each round is independent of the other rounds and will not affect either previous or later rounds. You will only be paid for **1** round from each Part. Since Part 1 has multiple rounds, I will roll a die randomly selecting which round everyone will be paid on for Part 1.

We will begin the experiment with each person selecting a ball from a bucket. I will show everyone that there are an equal number of Red, White, and Orange balls in the bucket. Therefore, each person has the same likelihood of picking a certain color. Before I explain the purpose of the balls, I would like to explain the nature of the tasks for the first 6 rounds.

Part1 (Rounds 1-6):

The task involves solving simple arithmetic questions. You will have 6 minutes to answer questions. Each question will be in the following format:

$$12 + 45 + 50 + 00 =$$

Notice that each question will involve four separate numbers ranging from 00 to 99. Within a round the questions will begin easy and get progressively harder.

When you have solved the problem simply type your answer into the box and click “OK”.

The computer will then give you a new question to answer.

Each of you has scratch paper and a pencil to help you; however, you will not be allowed to use a calculator or any other electronic device.

Scoring:

You will get one point for each correct answer and minus one point for each incorrect answer.

For example if in 6 minutes you answered 3 questions correct and 2 questions incorrect then your score is equal to 1 (3 – 2).

Luck:

For some questions, the computer will add an additional point “Good Luck” or subtract a point “Bad Luck”; therefore, luck will affect your score. You will not know whether the questions you are currently working on is a “Luck” question or a normal question, but on average one out of every five questions will be “Luck” questions. “Luck” questions have an equal chance of being “Good Luck” or “Bad Luck”.

“Good Luck” questions will always add 1 point. If you answer the question incorrectly you will earn 1 point. If you answer it correctly you will earn 2 points.

“Bad Luck” questions will always subtract 1 point. If you answer the question incorrectly you will lose 2 points. If you answer it correctly you will lose 1 point.

Normal questions are scored in their usual fashion: +1 for correct questions and -1 for incorrect questions.

After answering a “Luck” question, you will be shown the following massage:

- “Good Luck has affected your Score, one point has been added”
- “Bad Luck has affected your Score, one point has been deducted”
- If no message appears then this question was not affected by “Luck.”

In this way you will know if the previous question was a “Luck” question or a normal question.

Summary:

On average 1 out of 5 questions will be determined by “Luck.” Remember a question that is affected by “Luck” has an equal likelihood of being scored as correct or incorrect.

Contracts:

Shortly you will select one ball from a container. Each ball has an equal chance of being chosen and will determine which contract type you are in during the **entire** experiment.

To ensure fairness I will not tell you beforehand which ball is associated with which type of contract until everyone has selected a ball and entered their color into the computer program. However, you will be told your contract type prior to the start of Round One.

And, you will always be grouped with someone who chose the same color ball as you did. You will remain in this contract through all 6 rounds of Part 1.

The 3 Contract types are:

Contract 1: The person with the lower score will get $10 (Low Prize) and the person with the higher score will also get $10 (High Prize).

Contract 2: The person with the lower score will get $6 (Low Prize) and the person with the higher score will get $14 (High Prize).

Contract 3: The person with the lower score will get $2 (Low Prize) and the person with the higher score will get $18 (High Prize).

Example: Suppose you and the other person in your group chose a ball that is associated with Contract 2, and you answered 7 correct and 2 incorrect, then your score would be 5. And suppose the other person had a score of 4. Then if this round is selected you would receive $14 and the other person would receive $6.

Ties: If in a given round, you and the other person should have the same score, then the computer will randomly decide which of you will receive the "Low prize" and which one of you will receive the "High Prize" depending on the contract you preselected.

Groupings:

You will always be in a group of 2 people. After each round, you will be randomly paired again with someone in the room, and both of you will always have the same contract.

Score Manipulation:

In some rounds you can manipulate either your score or your group members score:

Your Score: A box will appear asking you, if you want to **increase** your score by 7 points. The box will remain on the screen on for the entire round until you check the box or the time expires. If you check this box and also confirm by clicking "OK," then your score will automatically increase by 7 points.

Other Player's Score: Everything is the same as above, except this time the question will ask you, if you want to **decrease** the other player's score by 7 points.

Warning – If you choose either of these options there is a chance you will be caught. If in the round that is randomly selected you are caught manipulating a score, then you automatically earn the low payoff. If you are not caught or choose not to manipulate the score, then your payoff is determined by who has the higher score. You will be made aware when the other player manipulates their score. Also, if you are both caught cheating then the tie-rule will be used, and the computer will randomly choose who gets the "Low" and "High" prize.

Payments on Rounds 1-6 depend on 6 Things:

1) Which round is randomly selected for payment at the end of Part 1.

2) Your Score within the selected Round

3) Your Group member's score in the Round

4) The contract type of the round

5) Whether either you or your group member manipulated scores, and if the answer is "yes," whether either of you get caught.

Layout of the Screen:

On the following page is a screenshot of what your screen MAY look like.

Middle of the screen:

- The Current Question is displayed
- Along with a box where you will submit your answer to this question
- Luck message if it applies (In this example the previous question was bad luck, and thus the player would lose 1 point although the question was answered correctly.
- You will have to click the "OK" button to lock in your answer

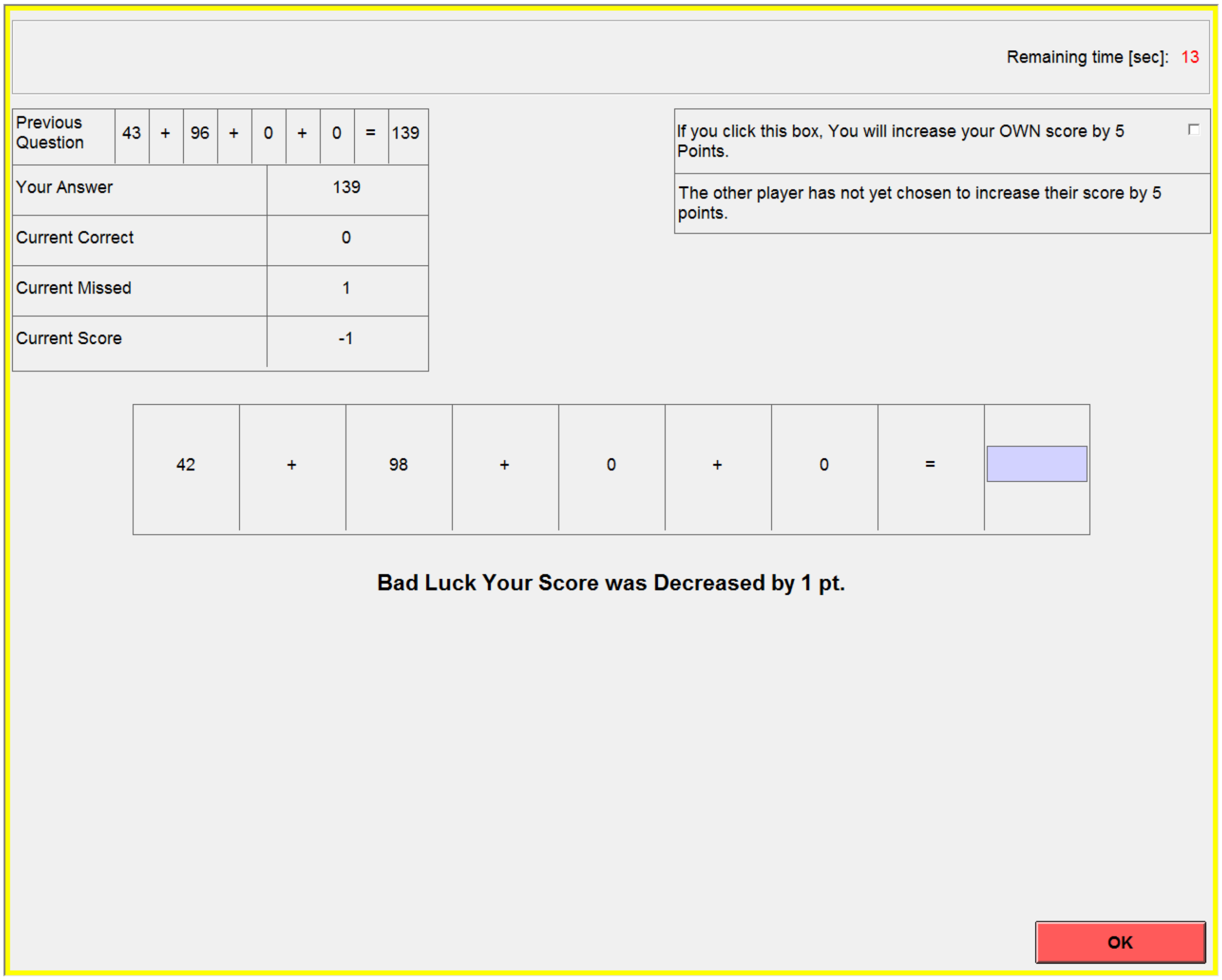
Top left corner of the screen:

- A summary of your progress Within the current round which has the following information:
    - The previous Question along with the correct answer
    - Followed by the answer you submitted
    - The number of "Correct" & "Incorrect" questions you have submitted up to that point
    - Followed by your Current Score

Top Right Corner of the screen:

- The Time remaining in the Round is displayed. When the timer reaches zero the round will end and you will not be able to answer any more questions in this round.
- If the round allows for manipulation then this box will appear. And it will display your option to manipulate, and whether the other player in your group has chosen

this option. Note that the screen will only update when you click "OK" at the bottom of the screen.



(Note to reader, the subjects will not be given these Instructions until after Part 1 is completed)

Part 2:

Below you see a screenshot of this task. As you can see you will have to make 10 decisions, deciding between "Option A" and "Option B." You will make your 10 decisions by clicking on your choice in the final column. But only one will be used in the end to determine your earnings. Please allow me to explain how these choices will affect your earnings for this part of the experiment.

Bingo Ball:

Notice that in the 1st column labeled "Decision from Bingo Ball" there are 10 Letters Q – Z. And that each of letter is associated with a specific Ball number. Everyone will have the same 10 Decisions labeled Q – Z and in the order seen below; however, the Ball number associated with each letter may be different. For example in the first Row Q is associated with a Ball number = 5 someone else may have Q associated with another number. The ball is used to select which one of the ten decisions will be used. And although the numbers may not be in the order as shown in this example, no Ball number will ever be repeated so that every row has an equal chance of being picked.

Die:

The Die will be used to determine what your payoff is for the option you chose, A or B, for the particular decision selected

Now, please look at Decision 1 at the top. Option A pays $6.00 if the throw of the ten sided die is 1, and it pays $4.80 if the throw is 2-10. Option B yields $11.55 if the throw of the die is 1, and it pays 0.30 if the throw is 2-10. The other Decisions are similar, except that as you move down the table, the chances of the higher payoff for each option increase. In fact, for Decision 10 in the bottom row, each option pays the highest payoff for sure, so your choice here is between $6.00 or $11.55.

To summarize, you will make ten choices: for each decision row you will have to choose between Option A and Option B. You may choose A for some decision rows and B for other rows, and you may change your decisions and make them in any order.

Please raise your hand if you have a question.

Decision From Bingo Ball	Option A From Die	Option B From Die	Please make your Decisions
Q Ball = 5	$6.00 if the die is 1 or $4.80 if the die is 2-10	$11.55 if the die is 1 or $0.30 if the die is 2-10	Option A Option B
R Ball = 3	$6.00 if the die is 1-2 or $4.80 if the die is 3-10	$11.55 if the die is 1-2 or $0.30 if the die is 3-10	Option A Option B
S Ball = 8	$6.00 if the die is 1-3 or $4.80 if the die is 4-10	$11.55 if the die is 1-3 or $0.30 if the die is 4-10	Option A Option B
T Ball = 9	$6.00 if the die is 1-4 or $4.80 if the die is 5-10	$11.55 if the die is 1-4 or $0.30 if the die is 5-10	Option A Option B
U Ball = 1	$6.00 if the die is 1-5 or $4.80 if the die is 6-10	$11.55 if the die is 1-5 or $0.30 if the die is 6-10	Option A Option B
V Ball = 10	$6.00 if the die is 1-6 or $4.80 if the die is 7-10	$11.55 if the die is 1-6 or $0.30 if the die is 7-10	Option A Option B
W Ball = 2	$6.00 if the die is 1-7 or $4.80 if the die is 8-10	$11.55 if the die is 1-7 or $0.30 if the die is 8-10	Option A Option B
X Ball = 4	$6.00 if the die is 1-8 or $4.80 if the die is 9-10	$11.55 if the die is 1-8 or $0.30 if the die is 9-10	Option A Option B
Y Ball = 7	$6.00 if the die is 1-9 or $4.80 if the die is 10	$11.55 if the die is 1-9 or $0.30 if the die is 10	Option A Option B
Z Ball = 6	$6.00 For Sure	$11.55 For Sure	Option A Option B
			Submit

B.2 Questionnaire

INSTRUCTIONS: Please answer truthfully to the best of your ability. Remember that the computer has not recorded your name of student ID so your answers are confidential.

PAYMENT: We will pay you $5 for answering the question, please answer carefully.

Open ended Questions:

1. What would you say was your primary motivation(s) for answering the math questions?
2. Did the option of manipulating scores affect your effort choice, if so why?
3. Did you manipulate your own score? What factors did you take into account when making this decision?
4. Did you manipulate the other person's score? What factors did you take into account when making this decision?
5. How do you feel about cheating in the real world? Have you ever done it?
6. How do you feel about sabotaging an opponent? Have you ever done it?
7. Which do you think is worse, cheating or sabotage, and why?

Instructions: Use the following response scale in answering the items below. Make sure to read each item carefully and circle the number that best represents your answer.

1 = Strongly Disagree
2 = Slightly Disagree
3 = Neither Disagree Nor Agree
4 = Slightly Agree
5 = Strongly Agree

1.	I put in maximum effort in every round.	1	2	3	4	5
2.	Cheating to gain an advantage is wrong.	1	2	3	4	5
3.	Harming others to gain an advantage is wrong.	1	2	3	4	5

Instructions: Use the following response scale in answering the items below. Make sure to read each item carefully and circle the number that best represents your answer.

1 = Strongly Disagree
2 = Slightly Disagree
3 = Neither Disagree Nor Agree
4 = Slightly Agree
5 = Strongly Agree

1.	I get satisfaction from competing with others.	1	2	3	4	5
2.	I am a competitive individual.	1	2	3	4	5
3.	I will do almost anything to avoid an argument.	1	2	3	4	5
4.	I try to avoid competing with others.	1	2	3	4	5
5.	I often remain quiet rather than risk hurting another person.	1	2	3	4	5
6.	I find competitive situations unpleasant.	1	2	3	4	5
7.	I try to avoid arguments.	1	2	3	4	5
8.	In general, I will go along with the group rather than create conflict.	1	2	3	4	5
9.	I don’t like competing against other people.	1	2	3	4	5
10.	I dread competing against other people.	1	2	3	4	5
11.	I enjoy competing against an opponent.	1	2	3	4	5
12.	I often try to out perform others.	1	2	3	4	5
13	I like competition.	1	2	3	4	5
14.	I don’t enjoy challenging others even when I think they are wrong.	1	2	3	4	5

15. What is your AGE?

16. What is your sex?

- Male
- Female

17. Which of the following categories best describes you?

- White
- African-American
- African
- Asian-American
- Asian
- Hispanic-American
- Hispanic
- Mixed Race
- Other

18. What is your major?

- Accounting
- Economics
- Finance
- Business Administration, other than Accounting, Economics, or Finance
- Education
- Engineering
- Health Professions
- Public Affairs or Social Services
- Biological Sciences
- Math, Computer Sciences, or Physical Sciences
- Social Sciences or History
- Humanities
- Psychology
- Other Majors
- Undeclared

19. What is your class standing?

- Freshman
- Sophomore
- Junior
- Senior
- Masters
- Doctoral
- Does not apply

B.3 Theory and Proofs Continued

Derivation of Lazear's (1989) first order conditions

$$(W_h - W_l)\frac{\partial Pr\left(e_i, s_i; e_j, s_j\right)}{\partial e_i} = C_1(e_i, s_i), W_h \geq W_l, e_i \geq 0 \text{ and } s_i \,\epsilon\, \{0,1\}$$

where W_h is the "high prize" and W_l the "low prize," e_i is the productive effort put forth by contestant *i*, and s_i is the binary decision to sabotage contestant *j*. $Pr\left(e_i, s_i;\ e_j, s_j\right)$is the probability of *i*'s win conditional on *j's* effort and sabotage decisions, and $C_1(e_i, s_i)$ is the marginal cost of effort and sabotage with the usual properties that *C* increasing and convex in its arguments.

We can rewrite $Pr\left(e_i, s_i;\ e_j, s_j\right)$ so that

$$Pr\left(e_i, s_i;\ e_j, s_j\right) = \mathrm{G}\left[f\left(e_i, s_j\right) - f\left(e_j, s_i\right)\right]$$

and letting g be the pdf it follows that

$$\frac{\partial Pr}{\partial e_i} = \mathrm{g}\left[f\left(e_i, s_j\right) - f\left(e_j, s_i\right)\right]\left[f_1\left(e_i, s_j\right)\right]$$

similarly for sabotage

$$\frac{\partial Pr}{\partial s_i} = \mathrm{g}\left[f\left(e_i, s_j\right) - f\left(e_j, s_i\right)\right]\left[-f_2\left(e_j, s_i\right)\right]$$

when costs are the same $C_i(\cdot) = C_j(\cdot)$ symmetry implies that when the Nash equilibrium exists $e_i^* = e_j^*$ and $s_i^* = s_j^*$ are mutual best responses, and each contestant wins with probability $\frac{1}{2}$ so that P = G(0) = $\frac{1}{2}$. Nash equilibrium yields $e_i = e_j$ and $s_i = s_j$, and therefore,

$$(W_h - W_l)\mathrm{g}(0) = \frac{C_1(e_i, s_i)}{f_1(e_i, s_j)} \qquad (1)$$

$$(W_h - W_l)\mathrm{g}(0) = \frac{-C_{s_i}(e_i, s_i)}{f_2(e_j, s_i)} \qquad (2)$$

$$(W_h - W_l)\mathrm{g}(0) = \frac{C_{e_i}(e_i, s_i)}{f_{e_i}(e_i, s_j)} = \frac{-C_{s_i}(e_i, s_i)}{f_{s_i}(e_j, s_i)} = \frac{C_{e_j}(e_j, s_j)}{f_{e_j}(e_j, s_i)} = \frac{-C_{s_j}(e_j, s_j)}{f_{s_j}(e_i, s_j)}.$$

Proposition 1: Increasing the Wage Spread Leads to Effort Increases

We begin with the result that $(W_h - W_l)\mathrm{g}(0) = C_1(e_i, s_i)$. Since we know that $\mathrm{g}(0) > 0$, we set $\mathrm{g}(0)$ equal to a constant *k*, so that $(W_h - W_l)k = C_1(e_i, s_i)$. $C_1(e_i, s_i)$ is strictly increasing in e_i for all values of s_i.Let,

$\alpha^a = (W_h^a - W_l^a)k$ and $\beta^a = C_1(e_i^a, s_i)$

$\alpha^b = (W_h^b - W_l^b)k$ and $\beta^b = C_1(e_i^b, s_i)$

These expressions can be simplified so that:

$\alpha^a k = \beta^a$

$\alpha^b k = \beta^b$.

If $\alpha^b > \alpha^a$, then $\beta^b > \beta^a$. This can be expressed as $C_1(e_i^b, s_i) > C_1(e_i^a, s_i)$, and this is true if and only if $e_i^b > e_i^a$, which concludes the proof.

Proposition 2: Increasing the Wage Spread Leads to Increased Levels of Sabotage

Proposition 2: As the remuneration spread $(W_h - W_l)$ increases, subjects respond by increasing their level of deviant behavior.

Similarly for sabotage $(W_h - W_l)k = C_2(e_i, s_i)$. $C_2(e_i, s_i)$ is strictly increasing in s_i, for all values of e_i. Let,

$k = \frac{C_2(e_i, s_i)}{(W_h - W_l)}$.

If we let $(W_h - W_l) > (W_h^a - W_l^a)$, then $k = \frac{C_2(e_i, s_i)}{(W_h - W_l)} = \frac{C_2(e_i, s_i^a)}{(W_h^a - W_l^a)}$, then $C_2(e_i, s_i) > C_2(e_i, s_i^a)$. And then it must be the case that $s_i > s_i^a$, and this concludes the proof.

<u>Hypothesis 1: Increasing the Advantage of Sabotage (without changing the cost) Increases the Likelihood that a Subject will Engage in Sabotage</u>

Hypothesis 1: When the effects of deviant behavior are changed from giving a seven-point advantage to a 14-point advantage, subjects choose to engage more frequently in deviant behavior.

The logic for the next section proceeds as follows: First, we show that agents engaging in sabotage when there is a seven point advantage will do so when sabotage offers a fourteen point advantage. Second, we show that agents who do not sabotage when there is a fourteen point advantage will not do so at seven points. And third we show that agents who are indifferent between sabotage or not sabotage at seven points, will definitely sabotage at fourteen points. These three conclusions support Hypothesis 1.

Let $a_k^i = G\left[f\left(e_i, ks_j\right) - f\left(e_j, ks_i\right)\right]$,

$$= G\left[\left(e_i - ks_j - e_j + ks_i\right)\right],$$

$$a_0^i = G\left[\left(e_i - e_j\right)\right],$$

$$a_7^i = G\left[\left(e_i - e_j\right) - 7\left(s_j - s_i\right)\right],$$

$$a_{14}^i = G\left[\left(e_i - e_j\right) - 14\left(s_j - s_i\right)\right],$$

where *k* is the numerical point advantage of sabotage, and a_0^i is *i*'s payoff when neither player sabotages as well as when both subjects sabotage.

And the player's problem can be expressed as follows:

$$\mathrm{U}(\bar{e}_i, s_i, k) = \left[W_h a_k^i + W_l\left(1 - a_k^i\right) - C(\bar{e}_i, s_i)\right].$$

The probability matrix and the costs for player *i* are provided below:

	$s_j = 0$	$s_j = 1$	Player *i* Costs
$s_i = 0$	$\mathrm{G}[(e_i - e_j)] = a_0^i$	$\mathrm{G}[(e_i - e_j - k)] = \alpha_k^j$	$C(e_i, 0)$
$s_i = 1$	$\mathrm{G}[(e_i - e_j + k)] = \alpha_k^i$	$\mathrm{G}[(e_i - e_j)] = a_0^i$	$C(e_i, 1)$

<u>Part 1: Show that if agent *i* sabotages at seven points, then agent *i* does so at fourteen points.</u>

Suppose $\mathrm{U}(\bar{e}_i, s_i = 1) > \mathrm{U}(\bar{e}_i, s_i = 0)$, when *k* = 7. Then

$$\left[W_h a_7^i + W_l\left(1 - a_7^i\right) - C(e_i, 1)\right] > \left[W_h a_0^i + W_l\left(1 - a_0^i\right) - C(e_i, 0)\right]. \qquad (1)$$

When *k* = 14, and $s_i = 1$ will give:

$$\mathrm{U}(\bar{e}_i\ s_i = 1, k = 14) = \left[W_h a_{14}^i + W_l\left(1 - a_{14}^i\right) - C(e_i, 1)\right] \qquad (2)$$

and $s_i = 0$ will give:

$$\mathrm{U}(\bar{e}_i\ s_i = 0, k = 14) = \left[W_h a_0^i + W_l\left(1 - a_0^i\right) - C(e_i, 0)\right] \qquad (3)$$

Note that the right hand side of (1) is the same as (3). We subtract (2) from the left-hand side of (1) and we get:

$$= \left[W_h\left(a_7^i - a_{14}^i\right) + W_l\left(1 - a_7^i - 1 - a_{14}^i\right)\right]$$

$$= \left[W_h\left(a_7^i - a_{14}^i\right) + W_l\left(a_{14}^i - a_7^i\right)\right]$$

$$= (W_h - W_l)\left(a_7^i - a_{14}^i\right) < 0.$$

Because $(W_h - W_l) > 0$, then it must be true that $\left(a_7^i - a_{14}^i\right) < 0$. We can rewrite

$$\left(a_7^i - a_{14}^i\right) = G\left[\left(e_i - e_j\right) - 7\left(s_j - s_i\right)\right] - G\left[\left(e_i - e_j\right) - 14\left(s_j - s_i\right)\right] < 0.$$

Since G is increasing in s_i, $a_{14}^i > a_7^i$. Therefore, if player *i* sabotages at *k* = 7, then $s_i = 1$ is preferred to $s_i = 0$ when *k* = 14.

<u>Part 2: Show that agents who do not sabotage when there is a fourteen point advantage will not do so at seven points</u>

Suppose $\mathrm{U}(\bar{e}_i\ s_i = 1, k = 14) < \mathrm{U}(\bar{e}_i\ s_i = 0, k = 14)$. Then

$\left[W_h a_{14}^i + W_l\left(1 - a_{14}^i\right) - C(e_i, 1)\right] < \left[W_h a_0^i + W_l\left(1 - a_0^i\right) - C(e_i, 0)\right]$.

When k = 7, we will now show that $\mathrm{U}(\bar{e}_i\ s_i = 1, k = 7) < \mathrm{U}(\bar{e}_i\ s_i = 0, k = 7)$

$\mathrm{U}(\bar{e}_i\ s_i = 1, k = 7) - \mathrm{U}(\bar{e}_i\ s_i = 1, k = 14)$

$= \left[W_h a_7^i + W_l\left(1 - a_7^i\right) - C(e_i, 1)\right] - \left[W_h a_{14}^i + W_l\left(1 - a_{14}^i\right) - C(e_i, 1)\right]$

$= \left[W_h\left(a_7^i - a_{14}^i\right) - W_l\left(a_7^i - a_{14}^i\right)\right]$

$= (W_h - W_l)\left(a_7^i - a_{14}^i\right) < 0.$

We know that $(W_h - W_l) > 0$, and we have shown that $\alpha_{14} > a_7^i$. Therefore it follows that:

$\mathrm{U}(\bar{e}_i\ s_i = 1, k = 7) < \mathrm{U}(\bar{e}_i\ s_i = 1, k = 14) < \mathrm{U}(\bar{e}_i\ s_i = 0, k = 14) = \mathrm{U}(\bar{e}_i\ s_i = 0, k = 7)$.

$\mathrm{U}(\bar{e}_i\ s_i = 1, k = 7) < \mathrm{U}(\bar{e}_i\ s_i = 0, k = 7)$.

And this concludes Part 2.

<u>Part 3: Show that agents who are indifferent between sabotage or not sabotage at seven points, will definitely sabotage at fourteen points</u>.

Rewriting the payoff matrix found above in Part 1, we have:

	$s_j = 0$	$s_j = 1$
$s_i = 0$	$W_h a_0^i + W_l\left(1 - a_0^i\right) - C(e_i, 0)$ A	$W_h \alpha_k^j + W_l\left(1 - \alpha_k^j\right) - C(e_i, 0)$ B
$s_i = 1$	$W_h a_k^i + W_l\left(1 - a_k^i\right) - C(e_i, 1)$ C	$W_h a_0^i + W_l\left(1 - a_0^i\right) - C(e_i, 1)$ D

where α_k^j equals $= G\left[\left(e_i - e_j - k\right)\right]$, and A, B, C, and D refer to the appropriate matrix cell.

Player *i* is indifferent between sabotaging and not sabotaging when the utility from cell A = C and B = D.

We begin by subtracting A from C:

C – A = $W_h\left(a_k^i - a_0^i\right) + W_l\left(1 - a_k^i - 1 + a_0^i\right) - C(e_i, 1) + C(e_i, 0)$

$= \ (W_h - W_l)\left(a_k^i - a_0^i\right) - [C(e_i, 1) - C(e_i, 0)]$

C = A when $(W_h - W_l)\left(a_k^i - a_0^i\right) = \ C(e_i, 1) - C(e_i, 0)$

Using the same logic we show when B = D

$$W_h\alpha_k^j + W_l\left(1 - \alpha_k^j\right) - C(e_i, 0) = \ W_h a_0^i + W_l\left(1 - a_0^i\right) - C(e_i, 1)$$

$$C(e_i, 1) - C(e_i, 0) = W_h\left(a_0^i - \alpha_k^j\right) - W_l\left(a_0^i - \alpha_k^j\right)$$

$$(W_h - W_l)\left(a_0^i - \alpha_k^j\right) = C(e_i, 1) - C(e_i, 0)$$

Since our assumption is that player *i* is indifferent between sabotage and not sabotage at $k = 7$; we get,

$$(W_h - W_l)\left(a_0^i - \alpha_7^j\right) = C(e_i, 1) - C(e_i, 0)$$

Since $\alpha_{14}^j < \alpha_7^j$ it follows that

$$(W_h - W_l)\left(a_0^i - \alpha_{14}^j\right) > (W_h - W_l)\left(a_0^i - \alpha_7^j\right) = C(e_i, 1) - C(e_i, 0)$$

and we get the result that

$$(W_h - W_l)\left(a_0^i - \alpha_{14}^j\right) > \ C(e_i, 1) - C(e_i, 0).$$

We can expand this result so that we get:

$$W_h a_0^i - W_h\alpha_{14}^j - W_l a_0^i - W_l\alpha_{14}^j > \ C(e_i, 1) - C(e_i, 0)$$

$$= \ W_h a_0^i - W_h\alpha_{14}^j - W_l a_0^i - W_l\alpha_{14}^j - W_l + W_l > \ C(e_i, 1) - C(e_i, 0)$$

$$= \ W_h a_0^i + W_l - W_l a_0^i - C(e_i, 1) > \ W_h\alpha_{14}^j + W_l - W_l\alpha_{14}^j - C(e_i, 0)$$

$$= \ W_h a_0^i + W_l\left(1 - a_0^i\right) - C(e_i, 1) > W_h\alpha_{14}^j + W_l\left(1 - \alpha_{14}^j\right) - C(e_i, 0).$$

This is the result we wanted, which is cell B > D at *k* = 14.

When we go back to the assumption that C = A when *k* = 7, then we have

$$(W_h - W_l)\left(a_7^i - a_0^i\right) = C(e_i, 1) - C(e_i, 0)$$

since $a_{14}^i > a_7^i$, we have that

$$(W_h - W_l)\left(a_{14}^i - a_0^i\right) > (W_h - W_l)\left(a_7^i - a_0^i\right) = C(e_i, 1) - C(e_i, 0)$$

and therefore we have our result that C > A at *k* = 14. And this concludes Part 3.

Appendix C

APPENDIX TO CHAPTER IV

C.1 Experimental Protocol

GSU undergraduate students who had expressed interest in participating in economic experiments by previously voluntarily sending their e-mail addresses to our laboratory list were eligible to participate. Students were randomly sent e-mail invitations to participate in the experiment by the center's recruitment software. In total, six sessions were run, each with 18 subjects (108 total subjects), and all subjects participated in only one session.

All sessions were run on Tuesdays and Thursdays and began at approximately 2:30 pm. Subjects were handed consent forms prior to entering the laboratory and asked to take their time and read at their own pace. Once subjects had read and signed the consent form, they were allowed to enter the laboratory and sit at an available computer[78] terminal. At each workstation, subjects found: scratch paper and a mechanical pencil, and introductory instructions which gave a general overview of the experiment and had the instructions pertaining to Round 1.

Once everyone had taken their seat, a PowerPoint presentation began which summarized the instructions they had at their desk. Once the presentation was over, and subjects had an opportunity to ask questions, a practice round was administered. The Practice Round lasted for

[78] The sessions were computerized using the Z-Tree platform (Fischbacher, 2007).

six minutes and was identical to future rounds, except that this round would not be used for payments. Following the Practice Round, the experimenter asked if there were any further questions. Once any questions/concerns were answered the experiment proceeded to Round 1.

At the end of six minutes, Round 1 ended, and the outcomes of Round 1 were displayed on each subject's computer for 30 seconds. That is, the subject's score was compared to his or her opponent's score, and the player with the higher score was called the Decider. In the case of a tie, the computer broke the tie by randomly selecting one player to be the Decider. After the 30 seconds, the experimenter handed out the instructions to the next round, which in half of the sessions were the IRound and in the other half the ORound. Assuming the IRound followed Round 1, then after the subjects read the instructions, the experimenter summarized the instructions and then activated the program to start the IRound.

At the end of the IRound, subjects were not informed of the outcomes of this round. All other procedures were identical to Round 1. For example, the experimenter handed out the instructions to the ORound and the experimenter summarized these instructions. The ORound was administered, and at the end of six minutes, the ORound finished. The experimenter then flipped a coin in full view and the flip was captured on a web camera and projected in the front of laboratory. If the coin came up "heads" then the IRound was paid; otherwise the ORound was paid. All subjects saw their earnings before the experiment moved on to Part 2.

In Part 2, the instructions to the Holt and Laury (2002) risk elicitation were handed out to the subjects. It is important to note that the payoffs used in this experiment were three times the baseline amounts of the original Holt and Laury (2002) experiment. The higher payoffs were chosen to keep the cost-to-effort ratio relatively constant between Parts 1 and 2. In addition, because some subjects would possibly earn $0.00 from Part 1, the higher payoffs ensure that the subjects are sufficiently compensated for their time. The higher payoffs were used in Anderson and Mellor (2008) and Anderson et al. (2012) and retained the pattern of expected values in the payoffs. That is the crux of this mechanism.

A second departure from the original (2002) design was involved in the protocol. In this design, each of the 10 lottery choices was associated with a letter (Q–Z) and a corresponding bingo ball numbered 1–10. As shown in Table 3, each subject's table displayed the 10 decisions (Q–Z), thus preserving the order of the tasks and the required pattern to determine the switching point between Lottery A and Lottery B. However, the bingo ball number associated with each letter differed across subjects. Therefore, when the experimenter revealed the outcome of the bingo ball number, *ex post,* the matching letter and lottery choice were not the same for each subject. This technique minimized the variance between payment possibilities and helped the experimenter with budgetary considerations. Instead of a die, a bingo ball was used because a 10-sided die revealed the subjects' payoff from the selected row, and using a die in both instances could introduce subject confusion. All other features of this design were identical to those in Holt and Laury (2002).

After every subject had submitted their choices in Part 2, the experimenter selected a bingo ball numbered 1 – 10. Each number corresponded to a letter (Q – Z), and each letter was paired with one of the 10 lottery choices. As shown in Table 3, each subject's table displayed the 10 decisions (Q–Z), thus preserving the order of the tasks and the required pattern to determine the switching point between Lottery A and Lottery B. However, the bingo ball number associated with each letter differed across subjects. Therefore, when the experimenter revealed the outcome of the bingo ball number, *ex post,* the matching letter and lottery choice were not the same for each subject. This technique minimized the variance between payment possibilities and helped the experimenter with budgetary considerations.

After selecting the bingo ball, which was displayed for all to see, the experimenter rolled a 10 – sided die. Again, the web camera revealed the roll. The experimenter then entered the bingo ball number and the number of the die into the server. Each workstation, then immediately displayed the earning from Part 2, along with the total payoffs from the experiment. Only then, did subjects learn that there was a questionnaire that paid an additional $5.00. The subjects were told that it would take approximately 10 – 15 minutes to begin calling names, and that the questionnaire was very valuable for the experimenter; and therefore were asked to please take their time.

The experimenter went into a nearby room and began calculating the payoffs. An assistant stayed in the room monitoring the subjects. Once the experimenter was prepared to receive subjects, the subjects were called one at a time according to the number on their workstation. The subjects received their money and signed a receipt form. Afterwards they were free to go. The total time of the experiment from the moment the first subject signed-in to the moment the last subject was paid approximately was one hour and a half. This concluded the experiment.

C.2 Altruism Based on Alternative Models of Other-Regarding Behavior

Fehr-Schmidt Model of Inequality Aversion

The F&S model is a popular model based on the presumption that individuals dislike outcomes that they perceived to be inequitable. Because the purpose of this section is to provide context for the categorization of "altruism" in my experiment, I restrict the F&S model to the simpler two-agent case when the Decider's ("my") money payoff m is greater than the non-Decider's ("your") money payoff y. Therefore, the model can be as expressed as follows:

$U_m(m, y) = m - \beta(m - y).$

where $0 \leq \beta \leq 1$. The negative sign in the second term captures how much the Decider dislikes getting more than the non-Decider. The Decider's utility diminishes as the gap between the two players' payoffs widens and can be interpreted as a type of compassion or guilt. β represents the intensity of this emotion. For example, if $\beta = 0$, then the utility function will simplify to the standard self-regarding model for which the agent is assumed to care exclusively for his or her own monetary payoff. $\beta = 0.5$ implies that the Decider is just indifferent between keeping one dollar to herself and giving this dollar to her group member.

Next I estimate a lower bound of β for the typical altruistic Decider. The average altruistic Decider and non-Decider scored 30 points and 21 points, respectively, in Round 1. As a reminder, the Decider's choice was between:

Contract A: pays $0.80 / point to the group member with the higher score, and $0.00 otherwise.

Contract B: pays $0.40 / point to both group members.

A Decider classified as altruistic must choose Contract A in the IRound (demonstrating confidence) and switch to Contract B in the ORound. If the Decider believes with certainty that she will win the ORound, then if she chooses Contract A, she believes she will earn \$24 ($\$0.80 \times 30$) but her group member will earn \$0.00. In contrast, if she chooses Contract B, then she will be willing to give up half her payoff, or \$12 ($\$0.40 \times 30$) to give her group member $\$8.40(\$0.40 \times 21)$. If Contract B is chosen, then Contract B can be rationalized as weakly preferred to Contract A.

Contract A

$$U_m(m,y) = 24 - \beta(24 - 0)$$
$$= 24 - 24\beta$$

Contract B

$$U_m(m,y) = 12 - \beta(12 - 8.4)$$
$$= 12 - 3.6\beta$$

Solve for β

$$24 - 24\beta \leq 12 - 3.6\beta$$
$$\beta \geq 0.59$$

A β of this magnitude implies that the Decider is willing to give more than a dollar to give a dollar to the non-Decider.

<u>Bolton-Ockenfels ERC Model</u>

Similar to the F&S model previously described, the ERC model also relies on inequality aversion to explain deviations from self-regarding behavior. The fundamental difference between the ERC model and the F&S model is that the agent in the ERC model is assumed to prefer situations in which the average payoff is as close as possible to his or her own payoff, whereas in the F&S model, agents dislike differences in the payoffs to any individuals. For example, according to the ERC model, an agent is indifferent between a situation in which all subjects earned the same payoff or a situation in which some were rich and some were poor as long as the agent received the average. According to the F&S model, the agent clearly prefers the first situation. In a two-player scenario, the two models are similar except for the shape of the indifference curves. The F&S model assumes that the indifference curves are piecewise linear, whereas in the ERC model, the motivation function is everywhere continuous and twice differentiable, and is expressed as:

$$v = v\left(m, \frac{m}{m+y}\right)$$

where *v*(.) is (Bolton & Ockenfels, 2000, pp. 171-172) globally non-decreasing and concave in income *m*, strictly concave in relative income $\frac{m}{m+y}$, and has a partial derivative with repect to relative income with the property $v_2\left(m, \frac{1}{2}\right) = 0$ for all *m* (Cox & Sadiraj, 2012, p. 929).

Substituting the values from the experiment for the typical altruistic Decider, the motivation function becomes:

Contract A

$$v = v\left(24, \frac{24}{24+0}\right)$$

$$= v(24, 1)$$

Contract B

$$v = v\left(12, \frac{12}{12+8.4}\right)$$

$$= v\left(12, \frac{12}{20.4}\right)$$

Both contracts can be rationalized using the ERC model, but the partial derivative shows that the agent prefers the relative payoff to be equal to ½. Therefore, a Decider will only give up some of her own payoff if the relative payoff gets closer to ½, which is precisely the case with Contract B.

Charness-Rabin Quasi-Maximin Model

The C&R model is a more complicated model that represents the weighted average of three components. It assumes that people are concerned with their own money payoff, m. The second and third components are part of a social welfare condition that assumes that people are concerned for helping the worst-off person as well as caring for efficiency by maximizing the total surplus. The C&R utility function is as expressed as follows:

$$\mathrm{U}(m, y) = (1-\gamma)m + \gamma[\delta \min\{m, y\} + (1-\delta)(m+y)]$$

where $\gamma \in [0,1]$ and measures how much the agent cares about pursuing her own self-interest versus the social welfare conditions. If $\gamma = 0$, then the function reduces to the self-regarding model. $\delta \in [0,1]$, and is a parameter that measures the degree of concern for the other two arguments. Setting $\delta = 1$ corresponds to a pure Rawlsian maximin rule whereby the agent wishes to maximize the worst-off person. Likewise, by setting $\delta = 0$, the agent is a utilitarian.

When applying the C&R model to the experiment, classifying a Decider as self-interested is not possible; however, the model concurs with my characterizations of altruism and is demonstrated by substituting the values of my experiment and then discussing the two Dictator types.

Contract A

$$U(m,y) = (1-\gamma)24 + \gamma[\delta\min\{24,0\} + (1-\delta)(24+0)]$$

$$= (1-\gamma)24 + \gamma[\delta\min\{0\} + (1-\delta)(24)]$$

Contract B

$$U(m,y) = (1-\gamma)12 + \gamma[\delta\min\{12,8.4\} + (1-\delta)(12+8.4)]$$

$$= (1-\gamma)12 + \gamma[\delta\min\{8.4\} + (1-\delta)(20.4)]$$

Self-interested Decider

To be classified as self-interested, a Decider must have picked Contract A in both the IRound and the ORound. According to the C&R model, choosing Contract A can be rationalized for two reasons. On the one hand, γ may have a small value that indicates that the Decider puts a relatively larger weight on her own payoff, which is consistent with self-regarding behavior. On the other hand, γ may take on a large value; however, the δ parameter may be close to zero, indicating a preference for efficiency. My experiment cannot discriminate between a desire for efficiency or selfish behavior.

Altruistic Decider

If Contract B is selected, then two of the arguments decrease (own payoff and efficiency) and only one increases (the minimum payoff). Therefore, it is consistent that a Decider who chooses Contract B does so because she cares about the worst-off player is consistent. Because a Decider is never the worst-off player, then this decision can be rationalized as Rawlsian Maximin, which in this case implies altruistic behavior.

Cox-Sadiraj Egocentric Altruism Model

The Egocentric Altruism model (C&S) is a special model of other-regarding preferences because it is represented by a utility function and indifference maps that are most similar to the conventional utility functions used in Economics. In other words, it has all of the regularity properties expected such as monotonicity and strict convexity of the indifference curves.

Similar to the B&O model previously discussed, selecting either Contract A or B is consistent with the C&S model. As an example, I use the simple two-agent Cobb Douglas utility function (Cox & Sadiraj, 2012, p. 930). The utility function can be expressed as:

$\mathrm{U}(m, y) = my^{\theta}$

where θ is the altruism parameter and is ≥ 0. Substituting the values from my experiment we get:

Contract A

U(24, 0)

$\mathrm{U}(24, 0) = 24 * 0^{\theta} = 0$

Contract B

U(12, 8.4)

$\mathrm{U}(12, 8.4) = 12 * 8.4^{\theta}$

Therefore, the C&S model can rationalize a Decider's choice to pick Contract B.

Another way to get the intended result is to note that egocentricity implies that the agent prefers the contract that allocates the larger of the two payoffs to herself when given a choice between two money allocations (*a*,*b*) and (*b*,*a*):

$\mathrm{U}(a, b) \ > \ \mathrm{U}(b, a)$ for all a and b such that $b > a \geq 0$.

Contract A

U(24, 0)

Contract B

U(12, 8.4)

Both contracts give the Decider a larger amount and, therefore, satisfy the condition that the Decider will allocate the larger payoff to herself ($a > b$). The ultimate decision of which Contract is preferred depends on the convexity of the indifference curves. Selecting Contract B indicates that the Decider must have very convex indifference curves, showing a strong weighting for the other person, which is indicative of a taste for altruism.

Therefore, all of the models detailed in this appendix are consistent with the categorization of a Decider as altruistic if she chooses Contract A in the IRound and then chooses Contract B in the ORound.

C.3 Questionnaire

INSTRUCTIONS: Please answer truthfully to the best of your ability. Remember that the computer has not recorded your name of student ID so your answers are confidential.

PAYMENT: We will pay you $5 for answering the question, please answer carefully.

Open ended Questions:

8. What would you say was your primary motivation(s) for answering the math questions?
9. If you were the Decider, what factors did you take into account when choosing Contract A ($0.80 to the winner and $0.0 otherwise) or Contract B (where you each got $04.0 per point) in round 2 (against the human opponent)?
10. If you were the Decider, what factors did you take into account when choosing Contract A ($0.80 to the winner and $0.0 otherwise) or Contract B (where you each got $04.0 per point) in round 3 (against the Fixed mark)?
11. If you were NOT the Decider, did the Decider's choice influence your effort, and if so please explain why?
12. Would you say you tried harder in either rounds 2 or 3, if so please explain why?

Instructions: Use the following response scale in answering the items below. Make sure to read each item carefully and circle the number that best represents your answer.

1 = Strongly Disagree
2 = Slightly Disagree
3 = Neither Disagree Nor Agree
4 = Slightly Agree
5 = Strongly Agree

1.	I put in maximum effort in every round.	1	2	3	4	5
2.	I would have preferred the piece rate against the human opponent.	1	2	3	4	5
3.	I would have preferred the piece rate against the Fixed Mark	1	2	3	4	5

Instructions: Use the following response scale in answering the items below. Make sure to read each item carefully and circle the number that best represents your answer.

1 = Strongly Disagree
2 = Slightly Disagree
3 = Neither Disagree Nor Agree
4 = Slightly Agree
5 = Strongly Agree

1.	I get satisfaction from competing with others.	1	2	3	4	5
2.	I am a competitive individual.	1	2	3	4	5
3.	I will do almost anything to avoid an argument.	1	2	3	4	5
4.	I try to avoid competing with others.	1	2	3	4	5
5.	I often remain quiet rather than risk hurting another person.	1	2	3	4	5
6.	I find competitive situations unpleasant.	1	2	3	4	5
7.	I try to avoid arguments.	1	2	3	4	5
8.	In general, I will go along with the group rather than create conflict.	1	2	3	4	5
9.	I don’t like competing against other people.	1	2	3	4	5
10.	I dread competing against other people.	1	2	3	4	5
11.	I enjoy competing against an opponent.	1	2	3	4	5
12.	I often try to out perform others.	1	2	3	4	5
13	I like competition.	1	2	3	4	5
14.	I don’t enjoy challenging others even when I think they are wrong.	1	2	3	4	5

15. What is your AGE?
16. What is your sex?

- Male
- Female

17. Which of the following categories best describes you?

- White
- African-American
- Asian
- Hispanic
- Mixed Race
- Other

18. What is your major?

- Economics
- Business Administration, other than Economics
- Education
- Engineering
- Health Professions
- Public Affairs or Social Services
- Biological Sciences
- Math, Computer Sciences, or Physical Sciences
- Social Sciences or History
- Humanities
- Psychology
- Other Majors
- Undeclared

19. What is your class standing?

- ○ Freshman
- ○ Sophomore
- ○ Junior
- ○ Senior
- ○ Masters
- ○ Doctoral
- ○ Does not apply

C.4 Subject Instructions for Competitors with ORP

(Below are the instructions when the IRound preceded the ORound. Please contact author for other case).

Introduction:

Thank you for volunteering to participate in this experiment. The decisions you will be asked to make will be explained in these instructions. In addition, you will earn money, which will be paid to you in cash at the end of the experiment in private.

Random Pairing:

The experiment will begin by assigning all participants to a group, and each group will consist of 2 people. All assignments will be randomly determined by the computer. You will never learn the identity of your group member, but your group member will be someone from this room. Both of your final payments will depend, in part, on your own decisions but also on the decisions of your group member. The amount of money you can earn will be explained in more detail later.

Overview:

The experiment consists of 2 Parts. Each part is independent of the other and will not affect each other in any way; however, both parts will be paid.

Part 1 has 3 rounds. You will only earn money for one of the rounds (either round 2 or 3). This will be determined by a coin flip. You cannot earn money in round 1, but you can earn the right to make a decision that will affect your group's payment which will impact how you are paid.

Part 2 has only 1 Round. You will know exactly how much you earned in part 1 before you start part 2.

Part 1:

General Instructions:

The task involves solving simple arithmetic questions. You will have 6 minutes to answer questions. Each question will be in the following format:

12 + 45 + 50 + 00 =

Notice that each question will involve four separate numbers ranging from 00 to 100. Within a round, the questions will begin easy and get progressively harder.

When you have solved the problem, simply type your answer into the box and click “OK.”

The computer will then give you a new question to answer.

Each of you has scratch paper and a pencil to help you; however, you will not be allowed to use a calculator or any other electronic device.

Scoring:

You will get one point for each correct answer and lose one point for each incorrect answer. For example if in 6 minutes you answered 3 questions correct and 2 questions incorrect, then your score is equal to 1 (3 – 2).

Layout of the Screen:

On the following page is a screenshot of what your screen MAY look like.

Middle of the screen:

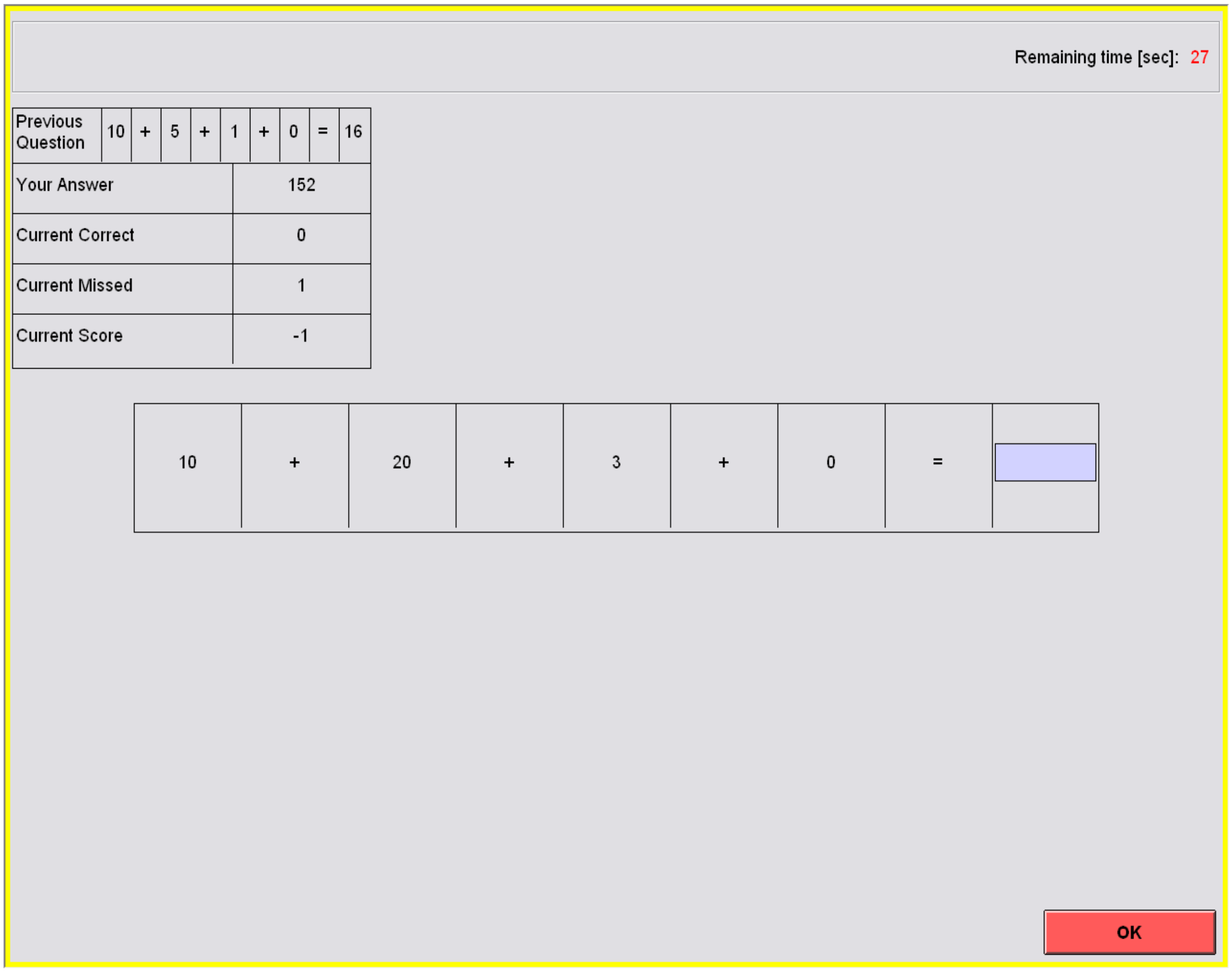
- The Current Question is displayed
- Along with a box where you will submit your answer to this question
- You will have to click the "OK" button to lock in your answer located at the bottom right hand side of the screen

Top left corner of the screen:

- A summary of your progress within the current round which has the following information:
    - The *Previous Question* along with the correct answer
    - Followed by *Your Answer*
    - The number of "Correct" & "Incorrect" questions you have submitted up to that point
    - Followed by your *Current Score*

Top Right Corner of the screen:

- The Time remaining in the Round is displayed. When the timer reaches zero, the round will end, and you will not be able to answer any more questions in this round.


Remaining time [sec]: 27
Previous Question
10 + 5 + 1 + 0 = 16
Your Answer 152
Current Correct 0
Current Missed 1
Current Score -1
10 + 20 + 3 + 0 =
OK

Round 1:

In round 1, each group (you and one other person) will have 6 minutes to answer the addition questions described above. After the 6 minutes have ended, a summary screen will display your score and your group member's score. The person with the higher score will be called the *Decider*. The decider will earn privilege of making a decision later in the experiment, (either in Round 2 or in Round 3) that will impact how both you and your group member is paid.

(Note to reader, the subjects will not be given these Instructions until after Round 1 is completed)

Part 1 Cont. Round 2

The 6 minute clock will be reset, and the addition task will begin again. You will be paired with the same person as in Round 1. This round has a ½ chance of being selected for payment, which will be determined by a coin flip in front of you.

Payment:

Your payment depends on which contract you choose, either contract A or B. Your contract does not affect the payment of your group member, as they will be able to make their own choice.

Contract A: pays $0.80 per point if in this round you score HIGHER than your group member and $0 if you score LOWER than your group member.

or

Contract B: pays $0.40 per point regardless of who scores higher or lower in this round.

(Note to reader, the subjects will not be given these Instructions until after Round 2 is completed)

Part 1 Cont. Round 3:

The 6 minute clock will be reset, and the addition task will begin again. You will be paired with the same person as in Round 1 and 2. This round has a ½ chance of being selected for payment, which will be determined by a coin flip in front of you.

In this round, only the Decider (chosen in Round 1) will be able to choose the contracts, and the decider must choose the same contract for both group members. That is, both group members will have identical contracts. If you are not the decider, then you must wait patiently until the Decider has made their selection and the addition task begins.

If the Decider choses Payment:

Your payment depends on which contract the Decider chooses as this choice will be the same for both group members.

Contract A: pays $0.80 per point to the group member with the HIGHER score and $0 to the person to the group member with the LOWER score.

or

Contract B: pays $0.40 per point regardless of who scores higher or lower in this round.

Payments on Rounds 2-3 depend on 4 Things:

1) Which round is randomly selected for payment at the end of Part 1. This will be determined by a coin flip.

2) If contract B was chosen than you will get $0.40 per point from this round

If Round 2 is selected

and

4) If contract A was chosen than you will get pays $0.80 per point if in this round you score HIGHER than your group member and $0 if you score LOWER than your group member.

If Round 3 is selected

and

4) If contract A was chosen by the Decider, then you will get pays $0.80 per point to the group member with the HIGHER score and $0 to the person to the group member with the LOWER score.

(Note to reader, the subjects will not be given these Instructions until after Part 1 is completed)

Part 2:

Below you see a screenshot of this task. As you can see you will have to make 10 decisions, deciding between "Option A" and "Option B." You will make your 10 decisions by clicking on your choice in the final column. But only one will be used in the end to determine your earnings. Please allow me to explain how these choices will affect your earnings for this part of the experiment.

Bingo Ball:

Notice that in the 1st column labeled "Decision from Bingo Ball" there are 10 Letters Q – Z. And that each of letter is associated with a specific Ball number. Everyone will have the same 10 Decisions labeled Q – Z and in the order seen below; however, the Ball number associated with each letter may be different. For example in the first Row Q is associated with a Ball number = 5 someone else may have Q associated with another number. The ball is used to select which one of the ten decisions will be used. And although the numbers may not be in the order as shown in this example, no Ball number will ever be repeated so that every row has an equal chance of being picked.

Die:

The Die will be used to determine what your payoff is for the option you chose, A or B, for the particular decision selected

Now, please look at Decision 1 at the top. Option A pays $6.00 if the throw of the ten sided die is 1, and it pays $4.80 if the throw is 2-10. Option B yields $11.55 if the throw of the die is 1, and it pays 0.30 if the throw is 2-10. The other Decisions are similar, except that as you move down the table, the chances of the higher payoff for each option increase. In fact, for Decision 10 in the bottom row, each option pays the highest payoff for sure, so your choice here is between $6.00 or $11.55.

To summarize, you will make ten choices: for each decision row you will have to choose between Option A and Option B. You may choose A for some decision rows and B for other rows, and you may change your decisions and make them in any order.

Please raise your hand if you have a question.

Decision From Bingo Ball	Option A From Die	Option B From Die	Please make your Decisions
Q Ball = 5	$6.00 if the die is 1 or $4.80 if the die is 2-10	$11.55 if the die is 1 or $0.30 if the die is 2-10	Option A Option B
R Ball = 3	$6.00 if the die is 1-2 or $4.80 if the die is 3-10	$11.55 if the die is 1-2 or $0.30 if the die is 3-10	Option A Option B
S Ball = 8	$6.00 if the die is 1-3 or $4.80 if the die is 4-10	$11.55 if the die is 1-3 or $0.30 if the die is 4-10	Option A Option B
T Ball = 9	$6.00 if the die is 1-4 or $4.80 if the die is 5-10	$11.55 if the die is 1-4 or $0.30 if the die is 5-10	Option A Option B
U Ball = 1	$6.00 if the die is 1-5 or $4.80 if the die is 6-10	$11.55 if the die is 1-5 or $0.30 if the die is 6-10	Option A Option B
V Ball = 10	$6.00 if the die is 1-6 or $4.80 if the die is 7-10	$11.55 if the die is 1-6 or $0.30 if the die is 7-10	Option A Option B
W Ball = 2	$6.00 if the die is 1-7 or $4.80 if the die is 8-10	$11.55 if the die is 1-7 or $0.30 if the die is 8-10	Option A Option B
X Ball = 4	$6.00 if the die is 1-8 or $4.80 if the die is 9-10	$11.55 if the die is 1-8 or $0.30 if the die is 9-10	Option A Option B
Y Ball = 7	$6.00 if the die is 1-9 or $4.80 if the die is 10	$11.55 if the die is 1-9 or $0.30 if the die is 10	Option A Option B
Z Ball = 6	$6.00 For Sure	$11.55 For Sure	Option A Option B

Submit

VITA

Alexander Paris Brumlik was born in Florida on January 21, 1978. He holds a Bachelor of Science from the University of Central Florida and a Master of Arts in Economics from Georgia State University. Alexander began Georgia State's doctoral program in 2007, with a focus in Public Economics and Experimental Economics. He has worked as a graduate research assistant to Dr. James C. Cox, Dr. Kurt Schnier, and Dr. Erdal Tekin. He has taught undergraduate economics courses at Southern Polytechnic University, Atlanta Metropolitan University, and Oxford College of Emory University.

Alexander received his Ph.D. in Economics from Georgia State University in August 2013. He has accepted a position as assistant professor at King University in Tennessee.