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Non-Pecuniary Factors Affecting Employee Productivity

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ABSTRACT

This dissertation is a collection of three essays which utilize experimental methods to examine three non-pecuniary factors which influence employee work effort. The first looks to understand how a telecommuting environment may affect productivity. In an effort to cut costs and improve worker morale, corporations are increasingly turning to telecommuting. Conflicting reports exist though on the effects that working outside the office has on productivity which directly affects a company's bottom line. Creative and mundane individual tasks were used to mimic two extreme work climates. Results of this study indicate that the telecommuting environmental effects may have positive implications on productivity of creative tasks and negative implications on productivity of mundane tasks. The second looks to understand how workers faced with a social dilemma similar to a work-team environment respond to observing punishment of someone else. Punishment has been shown to be an effective reinforcement mechanism. Intentional or not, punishment will likely generate spillover effects that extend beyond one's immediate decision environment, and these spillovers are not as well understood. We seek to understand these secondary spillover effects in a controlled lab setting using a standard social dilemma: the voluntary contributions mechanism game. We find that spillovers from punishment lead to either more or less cooperative behavior depending on the history of play. If subjects have direct experience with a punishment mechanism, they will contribute more to the public good after observing others' punishment. The reverse is true of those who observe others' punishment but have no exposure to direct experience with punishment. The final essay explores how social distance between an employee and a manager may affect the work effort of employees when they are competing for rewards given out by their manager. In an employer/employee relationship, this difference in social distance between the employer and the various employees leads to a disadvantageous situation for the socially distant workers when raises, promotions,

special considerations etc. are given. Since social distance is present in most organizations, understanding how employees' work effort changes in response to changes in social distance is of upmost importance. In prior literature, this disadvantage has always been assumed to lead to lower effort than the advantaged worker. Experimental tests of these lines of tournament theories have shown this is the case when the disadvantage is induced. It remains unclear however if this result holds when the disadvantage is not induced. The results indicate that females are much more sensitive to social distance between themselves and a manager where those socially close to the manager contribute much more than females who are not while there is no difference in the behavior of males based on social ties.

CHAPTER 1

INTRODUCTION

One of the primary goals of economists is to understand the behavior of entities under varying incentive schemes. The traditional methodology involves studying how monetary incentives affect behavior. A classical profit maximizing firm provides a good example of this. A firm will maximize profits where it will continue some action until the marginal benefits of doing so are less than the marginal costs. These costs and benefits can be easily represented in monetary units. If the focus is shifted to individual behavior, the same cost benefit analysis can be used where an individual is now maximizing utility, but it is now more difficult to ascertain how the individual views the costs and how they view the benefits. The baseline for a "rational" agent is to think that they make decisions just as a firm does by maximizing monetary payoffs. It is well understood though that money is not the only determinant which alters behavior. Experimental Economics has made large strides in furthering our understanding of these non-pecuniary factors and is an especially useful tool when considering how the profit motives of a firm interact with the utility motives of an employee who may not always act in a money-maximizing manner. It is in the firm's best interest to understand how these non-pecuniary factors alter behavior so they can meet their objective of profit maximization. If a firm understands these factors better, they may be able to costlessly alter their policies to either increase the productivity of their workforce or abolish inefficient practices. Since experimental economics has been the major tool used in economics to uncover these factors and is still a somewhat new field, non-pecuniary motives are still not well understood. This dissertation will utilize experimental methods to further our understanding of the non-monetary determinants of employee work effort in three distinct settings. The first looks to understand how work effort may vary depending on how structured the environment is. The aim of this essay is to give managers a more

accurate estimate of productivity changes they can expect if they allow their workforce to telecommute where telecommuting represents an unstructured environment and office-based workers a structured one. The second essay uncovers the behavioral effects from observing punishment in a work-team setting. Punishment is a tool most managers can use to encourage greater work effort. The direct effects of punishment are well understood, but in many cases there are observers of the punishment. It is unknown how these observers will react to seeing punishment. The third and final essay focuses on the work effort of employees who are competing for a reward when varying social distance between the employees and the manager is present. Since social distance is present in many office environments, understanding how this affects work effort is vital. The rest of this chapter will aim to provide broad introduction for each essay where the basic relevant literature will be reviewed and the essay's place in this literature examined. Each subsequent chapter will delve further into the more specific literature for that particular essay.

In a basic principal-agent framework, an agent is hired by a principal to perform work that is costly to the agent but benefits the principal. A friction is present when either the relevant actions of the agent and/or the relevant information available to the agent aren't observable to the principal. This gives rise to the common principal-agent problem, or more simply, the goals of the principal do not match those of the agent. In order to align the interests of the agent with those of the principal, incentives are often given to the agent by the principal. In this setting, it is usually assumed that the two agree on an incentive mechanism before work is performed. In most instances, a salary is given to the worker which may not be directly tied to their productivity (non-piece rate pay schemes) with some chance of positive or negative reinforcement in the future. Specifically, the "reinforcement" is either given before or after an employees' choice of work effort. When researchers examine pre-effort rewards, they are looking to see if workers reciprocate with work effort after being given a fixed wage. The idea of post-effort reinforcement is that workers will work harder to either garner a reward or avoid punishment. Thus, the incentive mechanisms have been traditionally seen as a method of coaxing higher productivity from the employee which in return reveals information about the agent (worker ability or value of outside option) which partially solves the principal-agent problem. In previous experimental research, both of these incentive mechanisms have been broadly explored.

The first lab experiments involved a very simple setting with one principal, one agent

and one task and were conducted on animals in the early 1980's (Battalio, Green and Kagel, 1981). In these experiments, the animals, rats and pigeons, expend real effort in order to gain income (food). In line with the predictions, a key result that emerged from this line of research was that the animals reduced their labor supply as the wage decreased.

Swenson (1988) conducted one of the first, if not the first, experiment on how incentives affect humans with a piece-rate pay scheme. A piece-rate pay scheme pays workers a certain rate for every piece of measurable productivity completed. Swenson's participants supplied real effort through a typing task and were paid based on the work completed where their earnings were taxed at various rates. This study also confirmed that the higher the tax (lower wages), the lower the productivity. This basic result is in line with subsequent studies with real effort tasks using a piece-rate pay scheme (Sillama, 1999a, 1999b and Dickinson, 1999). Though the direct effect of monetary incentives is evident, what isn't clear is how the environment affects work effort if monetary incentives are held constant. This is an important omission that is deserving of more attention.

In line with the prior studies, the first essay below looks at the work effort of a single employee under a piece-rate pay scheme but diverges from these by examining the environmental factors. More specifically, the intent of the design was to inform corporations how productivity may be altered once workers are outside of the office, or when the workers telecommute. In this setting, workers are paid a piece-rate wage for work performed in a structured or unstructured environment. Since very little is known about how the environment affects productivity, this research isolates the environment as an explanatory factor for changes in productivity by keeping the pay received constant in both settings. In the experiment, subjects performed tasks, which were either creative or mundane in nature, inside (structured environment) or outside the lab (unstructured environment). These tasks were chosen since most jobs performed by telecommuters require some combination of creative and repetitive work. The results of this study suggest that males are influenced more by the structure of the environment than females.

The next essay will consider the reinforcing device of punishment. In addition to rewards, managers can also use punishment as a reinforcement mechanism. Since Becker's (1968) seminal work formalizing crime as an economic decision, economist's interest in punishment mechanisms has grown. The basic premise is that crime can be characterized as a cost vs. benefits analysis and punishment, in turn, can have a measurable negative impact on utility

where behavior can be modified by simply increasing the costs of some undesirable action. This is especially relevant in a principal agent framework where punishment can be used to align the interests of the agents with the principal.

In the traditional Becker model, severity and probability of punishment which have the same expected value should have the same effect in deterring unwanted behavior. Anderson and Stafford (2003) examine if the above presumption of no difference in behavior results if equal expected value holds. In their study, subjects in an experiment are asked play a public goods game where low contributors are punished where punishment varies in the severity or the probability of enforcement. They find that increasing the severity of punishment is more effective than increasing the probability by the same amount in expected values.

Dickinson (2001) introduced external punishment into a work team environment in addition to rewards. Contributions to a public good are seen as equivalent to team effort exerted where in his design, high contributions are awarded and low contributions are punished. This mimics team effort because in a public goods game, incentives are set up so that a completely self interested person should contribute nothing to the group and keep everything. In a typical public goods game, a voluntary contributions mechanism (VCM) is used (Isaac, McCue and Plott, 1985). In a VCM, subjects are divided into groups and given an allocation of tokens and must decide how many of these tokens to give to the group and how many to keep. Tokens are converted into money where tokens kept have a return rate higher than those contributed to the group, though all members in the group share in the returns of contributions that any member makes to the group. In this paper, he finds that the reinforcement mechanisms are effective in eliciting higher effort and punishment is more effective than rewards.

Andreoni, Harbaugh and Versterlund (2003) use a single principal and a single agent to study incentives but also use punishment in addition to rewards in proposer-responder games. In these games a proposer (employee) chooses a division of a fixed pie where the amount chosen mimics work effort excerpted which the responder (manager) sees. The responder can then reciprocate by punishing or rewarding, punishing only, rewarding only, or neither where these actions have some cost to the responder. The proposer's profit is then the amount they kept plus any reward or minus any punishment given by the responder while the responder's payoff is the amount received minus the costs of any reward or punishment given to the proposer. They find that offers increase with the inclusion of incentive mechanisms and

show that the most effective incentive mechanism is one which includes both the option to offer a reward or punish.

Though the focus has been on behavioral changes of the one being punished, it is very unlikely that punishment affects only this person. In many instances there are observers to the punishment who may also be affected. The effect on these observers is less well known. More specifically, little is known on the psychological effects of observing someone else is punished when you yourself know the specific severity and probability of own punishment. This is an important addition since our current knowledge may lead us to under- or over-estimate the effectiveness of punishment mechanisms. The second essay is a joint project with David Dickinson and Cortney Rodet which does exactly this. Subjects in an experiment are asked to make contributions in a VCM after they observe if someone outside of their group received external punishment or not. We include in our design the possibility that the observer will be punished or not and vary the order with which this possibility is implemented. We find that the initial reaction of subjects observing punishment is to contribute fewer tokens to the group, though with experience in a punishment mechanism observing punishment leads to higher contributions to the group.

The prior studies are useful to understand how single agents act in varying schemes, in many instances though there are multiple agents. The final essay will examine how multiple agents compete for a reward given out by their manager when the social distance between themselves and their manager varies. There has also been a vast amount of literature when considering multiple agents competing for an award. In most instances, this competition has been studied in the tournament literature. Bull, Schotter and Weigelt (1987) experimentally test the predictions of Lazear and Rosen (1981) that a properly design piece-rate pay scheme induces the same effort as a tournament pay scheme. In their design, two workers were competing for a prize, M, by contributing some costly amount of tokens where contributions were seen as equivalent to effort excerpted in a work environment. The reward, M, was automatically given to the person with the highest observed effort level while $m<M$ was given to the person with the lowest. They find that the general predictions of Lazear and Rosen hold, but the variance of work effort is much higher in the tournament pay scheme than the piece-rate one. Since workers are typically not heterogeneous, they also introduce asymmetry in the workers by having different cost structures where one worker has higher costs of contributing effort than another worker. They find that the "advantaged" worker

contributed about the level of effort predicted, but the "disadvantaged" worker contributed much higher levels of effort than predicted.

The above studies use monetary incentives as a means of coaxing higher effort. The ultimatum game is the canonical example of when agents do not maximize monetary benefits which was first tested by Güth, Schmittberger, and Schwarze (1982). In the traditional ultimatum game there are two players sequentially bargaining over how to split a fixed pot of money. The first player, or proposer, proposes a split of the money and the second player, or responder, either accepts or rejects this split where acceptance leads to the allocations proposed and rejection leads to both players receiving nothing. A money-maximizing responder should accept any amount above zero and be indifferent between accepting and rejecting when zero is offered. The proposer should realize this and offer the smallest amount possible. The results of this game do not support the theory that players in are only money maximizers as offers are much greater than zero and rejections of offers greater than zero are quite common. This led to an explosion of research to understand how non-monetary preferences interacted with financial incentives.

One of the lines of research which does this is the trust/investment/gift exchange game (Fehr, Kirchsteiger, and Riedl, 1993 and Berg, Dickhaut and McCabe, 1995). These experiments are designed to examine if and how much a worker reciprocates work effort after being paid up-front. In these games, a first mover (manager) chooses how much to give a second mover (employee) where the amount sent is multiplied by a multiplier (typically 3) to mimic a productive activity. The task of the second mover is then to decide how much is sent back. Thus, the payoff of the first mover is how much they decide to keep plus how much they receive from the second mover while the payoff of the second mover is determined by how much they receive from the first mover (multiplied by some factor) minus how much they send back to the first mover. If the second mover is completely self interested, they should keep everything and send none back. The first mover should realize this and send nothing in the first move. It has been accepted, though, that in these games, first movers send positive amounts and second movers reciprocate by sending money back.

Reciprocity is one aspect present in most work settings while varying social distance between employees and managers is another. In this setting, some employees are in the in-group with their manager while others can be considered as part of the out-group. Chen and Li (2009) look at a series of games to determine the role group identity plays in how

subjects viewed others in allocation decisions. Their results show a very strong in-group bias for items such as charity, envy, punishment and social concerns.

Brandts and Sola (2010) combine the trust game with social distance. In their setting, a first mover passes tokens to two second movers which are multiplied by a multiplier and where one of the second movers is a personal friend of the first mover while one is not. The second movers must then decide how much to pass back. In their game, they vary the multiplier such that there is the potential more tokens passed to the first or second mover, depending on the treatment. Their results indicate that first movers give more to their friends and their friends reciprocate by giving more back regardless of the multiplier used. Second movers who are not friends with the first movers do not react negatively to this discrimination. Their finding suggests that social distance in a work place is productivity enhancing.

Evidence exists though that this may not always be the case. Bandiera, Barankay, and Rasul (2009) ran a field experiment where the workers were paid a piece rate wage to pick soft fruit and managers are paid a salary to assign rows to workers and make sure the workers have empty containers to fill. They use different measurements of social distance to determine the strength of the social ties between specific workers and managers. To determine how social distance affects a manager's preference to help socially close workers, they introduce a pay incentive to the managers where they are paid based on worker productivity. Once this pay incentive is used, they find that overall productivity rises. Their finding means that social distance was hindering productivity which was bottlenecked by management in their scenario.

The literature is still incomplete when it comes to the different types of reward structures a manager may offer and the effect these reinforcement methods have on employee behavior with the presence of in- and out-groups. Fully understanding this relationship will allow corporations to maximize compensation in a manner that will preserve the bottom line as well as help to complete the picture of social distance in the workplace as well as provide a complement the existing literature. The final essay addresses these needs and tests how workers compete for a reward with the presence of social distance between themselves and their manager. It does so in a carefully controlled lab environment where workers must decided how many tokens to contribute to their manager and how many to keep where tokens contributed to their manager is akin to work effort. Once the manager sees both

the allocations sent to her and which employee is in her own group and which is in the other group, she decides on the reward allocation to give. One of the major focuses of previous studies of unfair tournaments was to understand how discrimination affected work effort. Experimentally, unfair tournaments were used where an "advantage" was induced for some workers since it is difficult to test things such as racial or gender discrimination in the lab and even more so in the field. Since the focus of the present study is to look at social distance between the worker and the manager, a real person will decide the reward allocations and thus the discrimination will be endogenously determined. This addition will allow this research to have broader implications for understanding discriminatory behavior since it provides a nice avenue to determine if/how participants internalize the previously induced discrimination. In this manner, the prior studies on advantaged and disadvantaged workers provide a test bed for this study. The results indicate that females are more sensitive to social ties than males as female in-group workers contribute much more to their manager than female out-group workers while there is no difference in male's behavior.

CHAPTER 2

THE EFFECTS OF TELECOMMUTING ON PRODUCTIVITY: AN EXPERIMENTAL EXAMINATION

2.1 Introduction

Despite the importance of telecommuting as an alternative work method,[1] current economic literature has not deeply investigated how working outside the office affects productivity. Part of the reason for this is likely due to the practical difficulties in finding good measures for productivity which are necessary for such analysis. Additionally, separating environmental factors from other factors relevant to productivity may be just as imposing a task (Nalbantian and Schotter, 1997). The management literature has much more to say on the topic, but may leave as many questions as it answers. In a survey of the business literature on telecommuting, Bailey and Kurland (2002) summarize many benefits from previous studies, but conclude that "empirical research to date has been largely unsuccessful in identifying and explaining what happens when people telework." It seems that at least some managers are sympathetic to Bailey and Kurland's assessment as Mokhtarian and Salomon (1997) find that a gap remains in the number of people who telecommute and the number of people who wish to do so. To provide a more complete picture on the topic, the current study uses a novel, experimental design combined with survey methods to understand how the telecommuting environment affects a worker's productivity in mundane and creative tasks.

The Bailey and Kurland study mentioned above outlines the business literature's claims of the many benefits to companies from allowing their workforce to telecommute.[2] The majority

[1] A recent AP article claims that over 26 million people in the U.S. telecommute at least once a week. http://www.msnbc.msn.com/id/25007346/

[2] For additional benefits see Nilles, 1975; Mokhtarian, 1991; Piskurich, 1996 and Cascio, 2000.

of these studies, which use self-reported surveys to measure productivity (Bailyn, 1988; Belanger, 1999; Hill et al. 1998), typically find that telecommuting increases productivity. In these self-reported surveys, the question meant to approximate productivity is centered on asking the workers directly if they are more productive in their current telecommuting environment or in the office.

Though these surveys are useful as a first approximation, there are many potential issues that arise from self-reported data that can be averted through a careful experimental design. For example, there may be instances of inaccurate reporting or simply over-optimistic estimates by the employees. This is pointed out by Hill et al. (1998) when they state that their survey methods do not capture enough dimensions of the virtual office and that future work should include observations in addition to a questionnaire. These studies also point out that teleworkers report (with the exception of Hill et al. 1998) working more hours per week than their office counter-parts. This introduces an additional difficulty when trying to accurately measure how the work setting affects productivity.

In addition to the above limitations, the studies used surveys that typically only provided a "yes" or "no" answer from the workers. For appropriate cost/benefit analysis, it is not only important to determine if telecommuting increases/decreases productivity, but by how much.

Even in a rare non-survey study, such as DuBrin (1991), other complications inherent in the field data leave room for doubt about their measures of productivity. The main concern with this approach is the probable selection bias. It is very likely that the workers allowed to telecommute had already gained the trust of management by showing high productivity and/or a strong work ethic. Comparing these "better" workers in the non-office environment with those not allowed to telecommute may not provide a true measure of productivity differences due to the work environment, but rather the desire of management to keep or reward the better employees. Another equally valid criticism is the inability of these studies to isolate time effects. For instance, there may be short-term effects from a change of environment or long-term effects such as learning or other unobservable factors that contribute to productivity not related to the environment.

Reports that more generally outline the negative aspects associated with remote workers, like a 2001 USA Today piece, look at issues ranging from compromised company information (company information traveling through less secure networks) to telecommuters feeling

isolated from their coworkers.[3] Other articles, like a CNN article titled *Bosses worry if telecommuters are really working*,[4] directly contradict the claim that telecommuting increases productivity.

A plausible reason management may mistrust an employee working outside the office may be due to the increased value of an outside option. There are two main reasons this could affect telecommuters. First, there will likely be better or more outside options at home in the form of distractions not present in the office. Second, it is much more difficult for managers to monitor teleworkers. This extra freedom can be thought of as a more attractive outside option for the employee since the costs of shirking have been diminished. The result of these outside options is manifested in management's mistrust of employees' work effort. There is existing literature showing the effects outside options have on subject's decisions in both of these realms.

The findings of these studies follow the expected pattern. Dickinson and Villeval (2008) show that when the employment relationship was not personal, agents do respond to increased monitoring in providing more effort up to some threshold. Engel (2008) found that by simply giving subjects an exogenous outside option in a real task experiment the subjects chose to work less. The outside option was simply a distraction available to the subjects if they did not want to simply stare at the computer screen should they choose to not exert effort towards the real task.

In the previously mentioned review of the telecommuting literature, Bailey and Kurland (2002) state:

> In this exploratory stage of research into the telecommuting adoption process, the identification of the variables most relevant to that process, and the most effective ways of measuring those variables, are still uncertain.

In light of the seemingly contradictory findings and in recognition of the above statements from Bailey and Kurland, this study will use experimental methods to bridge the gap in the literature to determine if the increased value of the outside option outweighs the cited benefits gleaned from telecommuting. In the simple design, subjects are asked to perform real-effort tasks either inside or outside the lab. The two different tasks are either creative

[3] http://www.usatoday.com/careers/news/2001-06-25-tele-usat.htm

[4] http://www.cnn.com/2008/LIVING/worklife/09/09/bosses.worry.telecommute.ap/index.html

or mundane in nature to mirror most work settings where telecommuting is implementable. This technique allows contextual elements, thought to be important, to be progressively layered on to determine which ones are and by how much. In essence, this study can be seen as a baseline upon which future investigations can build.

The results suggest that there is a negative effect on productivity of working outside the office when the task is mundane but a positive effect on productivity of working outside the office when the task involves more creativity. These results are driven by certain segments of the population.

The rest of the paper is organized as follows: Section 2 describes the experimental design, section 3 describes the results and the last section concludes the paper with a discussion of the main findings and further areas of research.

2.2 Experimental Design

The experiment described below will consider those jobs that are traditionally thought of as good candidates for telecommuting involving the work of single individuals with a defined output. It is vital that subjects perform actual, easily measurable tasks in this initial study. This design is driven by the quest to isolate the effects the work setting has on productivity.

Subjects were recruited from the student population at Florida State University using a standard recruiting process[5] and were randomly assigned to participate in the XS/FS lab on campus or outside the lab. The experiment was conducted via a website programmed with Joomla. Subjects performing the task outside the lab were sent an email describing how to log onto the website along with a brief description of the structure of the experiment.[6] Subjects in the lab were given this same information on a sheet of paper with changes relevant to location and time differences. A copy of this script is included in the Appendix. Subjects outside the lab were instructed that they had 24 hours to complete the experiment meaning they could log in at the time of their choosing while subjects in the lab could take as much time as they wanted, but were recruited for a two hour experiment and none took

[5] This process entails sending out a customary recruitment email 48 hours before the experiment begins to all subjects in the subject pool.

[6] They were not told what the tasks were, just a description of how long each round is and how many rounds along with information on what order everything will appear. This was to insure that subjects were aware of the length of the experiment to eliminate selection bias or incomplete results.

the entire two hours.[7] Subjects could log in or log out as they chose[8] which gave those subjects in the non-office environment the most benefit from being in this setting. To keep information symmetric for both sets of subjects, those in the lab were not allowed to ask questions about anything other than computer/login problems. It is important to note that the *only* things that changed in the experiment between treatments were location and minor changes in instructions to account for these differences.

Once subjects logged on to the webpage, they saw a consent form which they had to accept before moving on. After they agreed to participate, they filled out the pre-experiment questionnaire. When this was completed, they were presented with instructions for Part 1 which can be found in the Appendix. After acknowledging they read the instructions, subjects started Part 1, which consisted of five 5-minute rounds.

In each of the rounds of Part 1, the subjects could choose to perform the primary task or the secondary task, in whichever combinations they chose, by clicking on the appropriate tab labeled "task I" (primary task) or "task II" (secondary task).[9] The primary task involved typing a set of four random characters that appeared on their screen. The characters were a mix of letters (capitalized and non-capitalized) and numbers. Once they typed a set, they clicked a submit button or hit the enter key to see the next set. The subjects were paid \$.02 for each correctly typed set. This task most closely mimics the work done by data entry personnel, though it has broader implications and approximates most "non-creative" work.[10] There was also a table at the bottom of the screen that showed how many strings of characters they attempted, how many they typed correctly and their cumulative payment, all updated in real time. Beside this table was a clock that showed time remaining in seconds.

In addition to the primary task, subjects also had an outside option to play tic-tac-toe labeled "task II". In this case, the participants played this game against the computer as an alternative to the primary task. Having a secondary task was shown to be an important addition in measuring productivity when subjects perform real tasks (Engel, 2008).[11] In the

[7] Most subjects in the lab completed the experiment within an hour with no one exceeding 1 hour 10 minutes.

[8] Almost all subjects only logged in once or twice.

[9] Subjects could spend all of their time on task I, all of their time on task II or some combination in between.

[10] For instance, like filing reports

[11] other references to the importance of outside option in different settings see Lei, Plott, and Noussair (2001); Palfrey and Pevnitskaya (2008)

tic-tac-toe section, subjects saw the outcomes of the games they played updated in real time. The payment for this task was \$.01 for each game won. It can be debated that a payment may not be necessary since this game is typically considered fun. The concern was that not everyone will find this game fun or subjects may get bored with it over the course of the experiment. The small payment somewhat guards against these concerns. On average, subjects played the game about 3 times per round thus, subjects understood to make the most money, they should spend their time doing the primary tasks. A screenshot of Part 1 is given in the Appendix.

After five minutes, the round ended where subjects were reminded of how much money they had earned. They would not move to the next round until they hit the "next" button (and wording indicating such was on this screen as well). They repeated this for a total of five rounds.

As soon as Part 1 was completed, subjects were presented with instructions for Part 2 and allowed to participate in Part 2. Instructions for Part 2 are given in the Appendix. There were five 3-minute rounds for this portion. The screen layout was the same as in Part 1 where the subjects could choose between the primary task or the secondary task in any combination they chose by clicking on the tabs labeled "task I" or "task II". Since task II was the same in parts 1 and 2 the reader can refer to the above description of tic-tac-toe. The primary task in part 2 is commonly known as the "unusual uses test." In this task, subjects saw a word or phrase describing an object along with space to type a use for which the object was not intended. An example was given to them of a tin can. Valid unusual uses would be to use the can as a flower pot, tie a string between two cans and use it as a phone, cut it up and use it as a pinwheel, etc. A use that would not count would be to use the can as a container for fruits or vegetables since this is what the object was intended for. The objects they were presented with, and had to think of unusual uses for, were a brick, a pen, a paper clip, a newspaper and a cardboard box.

This test has been used in psychology since 1962 (Torrance, 1962) to measure divergent thinking and is still in use (Severson et al. 2005). Divergent thinking simply looks at someone's ability to overcome the urge to only see the intended uses of an object. This fits the description of creativity used by many authors (Woodman et al., 1993) as the ability to think of novel and useful concepts.

After the participants typed a use, they clicked on the submit button or hit the "enter"

key to record their entry and bring up another empty box to type in. The subjects were paid \$.05 for each valid use. The criterion for validity was judged by two independent graders. The same two judges were used for all the grading tasks. The judges were given the same instructions as the subjects along with definitions of the words and were asked to determine if the uses qualified as valid. If the subject came up with a valid use not thought of by anyone else in their session (about 10 people per session), this use was determined to be unique and earned the subject an additional \$.05.

This incentive scheme was employed to induce subjects to come up with as many different uses as possible. This task most closely mimics the majority of the work not covered by the first primary mundane task. There are many measures of creativity that can be employed, but this task gives a clean numerical measure of productivity and closely resembles the work expected in creative careers.

After 3 minutes, the round ended. Subjects could not continue to the next round until they hit the "next" button. This was repeated five times. Because of the structure of part 2, payment to the subjects was available one week after they participated in the experiment. Paying subjects in this manner has the added benefit of keeping the incentives the same for subjects in and outside the lab.

After part 2 was completed, subjects were asked to fill out a short behavioral questionnaire. The questionnaire was mainly developed around a previous study that identified characteristics of people who chose to telecommute (Belanger, 1999).

2.3 Results

There were a total of 125 people that participated in the experiment, 63 in the lab and 62 outside the lab. 52% of the population was male and the average age was 21. Earnings averaged \$18.91 including a \$10 participation fee for an experiment that lasted about 50 minutes (for those in the lab).[12]

A between subjects design was used along with the questionnaire to carefully determine the effect telecommuting has on productivity. In a firm, a manager will often have to approve of an employee telecommuting and they may have been able to observe that employee's work ethic or other relevant characteristics over a period of time. Thus those employees

[12]There was a total of 40 minutes for the tasks in part 1 and 2 which meant subjects in the lab spent about 10 minutes for instructions, questionnaires etc. It's uncertain how long those outside the lab took.

approved for telecommuting may be carefully selected whereas in this study, subjects will be randomly assigned to be telecommuters. The questionnaires will help to determine if there are subgroups or populations which exhibit different results and may well be better candidates to telecommute in a field environment. When relevant, the questions on the questionnaire were worded in terms of school work so that students could relate to the items of interest. The following hypotheses were developed using the questionnaire questions which are guided by the previous literature.

Hypotheses

Unless noted otherwise, the main study used to form the following hypotheses will be Belanger (1999). Her research empirically identifies, via a survey, the traits of a typical teleworker. The logic for forming hypotheses around this paper is as follows. If telecommuters are more productive outside the office than inside the office, as the management literature states, then simply identifying the characteristics of the typical telecommuter should provide testable predictions for this study. There are two very different tasks subjects were asked to complete that may lead to very different results, but since none of the previous literature provides a distinction between the two, the hypotheses below will not differentiate between them.

The first hypothesis will follow the predictions based on the economics literature stated above. The literature points out that the value of the outside option will lead to decreased productivity when work is performed outside the lab.[13]

Hypothesis 1: Subjects will be less productive outside the lab.

The rest of the hypotheses will look at how the work setting may affect different groups. Hypothesis 2 will look specifically at how the environment may affect the productivity of the two genders. Belanger (1999) points out that females are more likely to telecommute. If managers have some insight into who the more productive individual may be once they are outside the office, then on average we can expect higher productivity from females outside

[13]Though none of the previously cited papers use tic-tac-toe specifically, the general trend observed leads to the conclusion that as the value of an outside option increases, work effort declines. This makes intuitive sense because a more valuable outside option can essentially be thought of as an increased cost of exerting work effort.

the lab since they are allowed to telecommute more often while the reverse can be inferred for males.

Hypothesis 2: Females will be more productive outside the lab while males will be less productive outside the lab.

The next group is titled procrastinators. To gather this data, the subjects were asked when they began studying for an exam. Those who answered they studied the day of or the night before the exam were labeled as procrastinators. In a telecommuting environment with the absence of direct monitoring, it may be pertinent that procrastinators not be allowed to telecommute as they may not finish projects on time.

Hypothesis 3: Procrastinators will be less productive in the non-lab environment while non-procrastinators will not be affected by the environment.

The next group of interest is those that claim they need more personal control over situations. Belanger (1999) states that companies use personal control as an incentive to provide more worker satisfaction. The group that answers they need more personal control may be a group that is best suited for working outside the office while the reverse may be true for their counterparts.

Hypothesis 4: Those who need more personal control will be more productive outside the lab while those who don't need personal control will be more productive in the lab.

The final group looked at is those who answered they are more productive on or off campus when doing schoolwork. This question mimics what the current telecommuting literature uses as a proxy for productivity. In line with this literature, it will be hypothesized that those who said they were more productive on campus and are in the lab should be more productive than their counterparts in the non-lab environment. The same is true of those who answered they are more productive off campus.

Hypothesis 5: Those who claim to be more productive on campus will be more productive in the lab while those who say they are more productive off campus will be more productive outside the lab.

Data Entry Task

The primary task in part 1 was typing random sets of characters. The typing task totaled 25 minutes and was split into 5, five minute rounds. In part 1, the mean number of sets typed per round was 73 in the lab which was not significantly different than the 70 typed outside the lab (using a t-test, t= -1.57). To get a more complete picture of the data, figure 2.1 shows how much more (or less) productive a subject is in the lab than someone outside the lab. The percentage increase (or decrease) in productivity by being in the lab is organized by the least to the most productive subjects in each treatment from left to right. Any point on the graph is the productivity of a subject in the lab minus the productivity of a subject outside the lab where both subjects productivity rank by treatment is the same. As an example, the far left point of the graph compares the productivity of the least productive subjects in the lab with the least productive subject outside the lab. This graph sheds insight into how the environment affects workers at differing productivity levels. When organized in this manner, it seems apparent that those in the lab are more productive than those outside the lab most of the time as the trend is above the 0% line more often than not. Thus, even though there is no statistical difference in the overall means, it appears that the environment affects the least and most productive subjects more than the average. If the subjects were divided into thirds based on their productivity, the most pronounced difference is in the group comprising the lower third where those in the office are on average 10% more productive than those outside the office. This difference is confirmed statistically at the $p < 0.05$ with a t-test ($t = -2.24$). There is a smaller 4% difference in the upper third group which is statistically significant at the $p < 0.10$ ($t = -1.72$) and no statistical difference in the middle group. Returning to the hypotheses, Table 2.1 shows the means of the groups of interest in hypotheses 2-5 and presents results of t-tests.

Figure 2.1 shows how much more productive someone in the lab is vs. their same productivity counterpart outside the lab for the typing task.

What becomes evident from looking at Table 2.1 and Figure 2.1 is that those outside the lab are often statistically less productive. Though a t-test serves as a good preliminary test, there are many things left uncontrolled which a careful regression analysis can correct. The regressions are structured to analyze how the environment affects a particular group and the

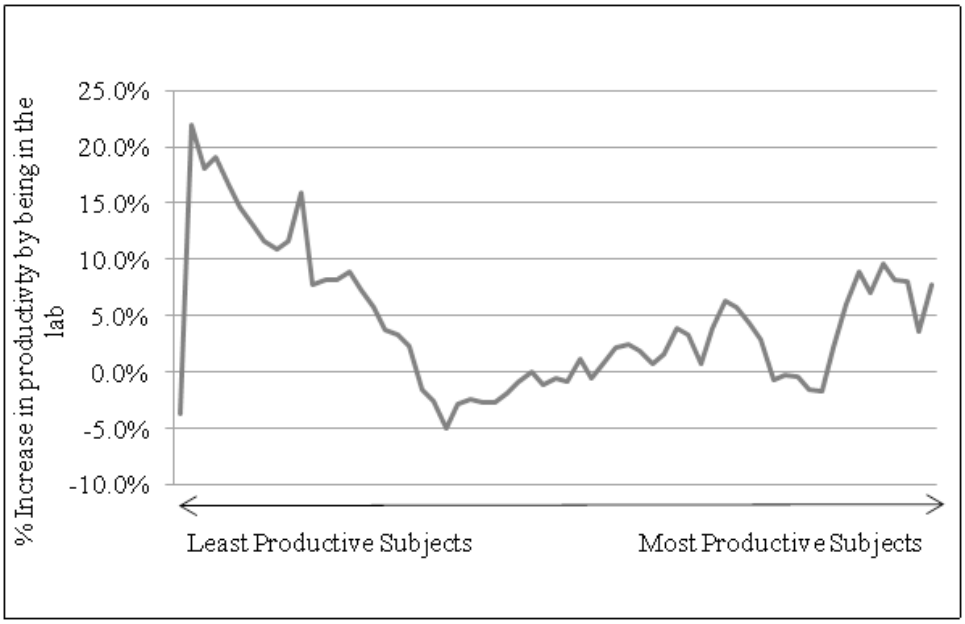


Figure 2.1: Productivity of those Inside vs Outside the Lab: Typing Task

results will be organized in a manner that is easy to interpret the effects of the environment. The results are pooled and the variable of most interest is the one that differentiates how the environment affects productivity, which for the groups, is the group identifier interacted with the dummy variable associated with the lab.

The first regression (column 1 in Table 2.2) was carried out as follows. The dependent variable was the number of sets typed per round. The explanatory variables of most interest were dummy variables for the 4 groups mentioned previously in the hypothesis section, a variable for lab and control variables for gpa and round (1-5). The variable for round will account for changing behavior over time, while gpa will be a rough measure of aptitude. Because of the obvious correlation in error terms and dependent variables across subjects, a random effects model was used and errors were clustered around each individual. The regressions in column 2 in Table 2.2 and all of Table 2.3 were run in the same manner except instead of using a dummy for lab the two interaction effects of the group of interest with lab was included. This interaction effect will tell us if there is a correlation between productivity of that group and the lab. Tables 2.2 and 2.3 present the results of the regression analysis.

Table 2.1: Means and t-tests of Hypothesis 2-5: Mundane Task

		In the lab Mean	*Outside the lab* Mean	$T-stat$ for difference
Hypothesis 2	Male	72.59	65.34	−2.97***
	Female	72.65	73.52	0.34
Hypothesis 3	Procrastinate	65.48	68.26	0.84
	Do not Procrastinate	75.70	70.37	−2.56***
Hypothesis 4	Need Personal Control	73.68	72.63	−0.39
	Do not need Personal Control	72.01	67.04	−2.07**
Hypothesis 5	More Productive on Campus	66.77	69.7	−1.27
	Less Productive on Campus	78.89	73.31	−1.98**
*** p<0.01, **p<0.05, * p<0.1				

In Table 2.3, the in-group is that group that is identified as the characteristic in the column heading. As an example the in-group corresponding to the column labeled "male", is males and the out-group is females.

Table 2.2: Regressions for Hypothesis 1

	No Interactions	Interactions for level of productivity
Lab	4.304 (3.045)	
Lab*Lower third		3.874* (2.254)
Lab*Middle third		1.075 (1.836)
Lab*Upper third		3.459 (3.226)
Male	−2.433 (3.085)	−2.489 (1.545)
Procrastinate	−6.895** (3.321)	−3.508** (1.643)
Need Control	1.752 (2.963)	−2.019 (1.504)
Productive Campus	−4.312 (3.297)	−0.784 (1.476)
Errors clustered around individual, Control variables not shown: gpa and round *** $p<0.01$, ** $p<0.05$, * $p<0.1$		

Looking at Table 2.2 , the first two rows show the correlation between a specific group and their productivity difference from being in the lab. These regressions are in line with

Table 2.3: Regressions for Hypotheses 2-5

	Male	Procrastinate	Need Control	Productive on campus
Lab*In Group	7.987** (4.067)	−0.614 (5.196)	0.282 (4.224)	4.160 (3.517)
Lab*Out Group	0.374 (4.385)	6.172* (3.661)	7.113* (4.105)	4.539 (5.381)
Male	−6.184 (4.153)	−2.344 (3.063)	−2.577 (3.078)	−2.409 (3.025)
Procrastinate	−6.771** (3.289)	−3.163 (4.569)	−7.372** (3.292)	−6.923** (3.276)
Need Control	1.892 (2.971)	1.436 (2.953)	5.082 (4.063)	1.752 (2.967)
Productive Campus	−4.921 (3.218)	−4.747 (3.287)	−4.223 (3.271)	−4.136 (4.110)
In Group refers to the corresponding column dummy variable when it is equal to 1 and Out Group refers to the corresponding column dummy variable when it is 0. Errors clustered around individual, Control variables not shown: gpa and round *** $p<0.01$, **$p<0.05$, * $p<0.1$				

Figure 2.1 in that all the coefficients corresponding to the variable for lab except one are positive. Out of these groups, four are significant at the 10% level or better. Thus, there is statistical verification for hypotheses 1 and 3 while hypotheses 2 and 4 predict the incorrect direction for change in productivity outside the lab.

Results for the typing task:

Result 1: Subjects in the bottom third productivity-wise were about 6% more productive in the lab than their non-lab counterparts. This result is not statistically verified for the middle third and only statistically verified with a t-test for the upper third productivity.

Result 2: Males are about 10% more productive in the lab than outside the lab while there is no statistical verification that females are more or less productive in the lab.

Result 3: There is no statistical evidence to support the claim that procrastinators are more productive in the lab while there is support to show that those who do not procrastinate are about 8% more productive in the lab.

Result 4: Those who do not need personal control are about 10% more productive in the lab though the environment has no statistical effect on the productivity of the group who desire more personal control.

Result 5: Those who claim to be less productive on campus are shown to be more productive on campus than off, though this statistical difference goes away once more factors are controlled for. There is no statistical evidence to support the claim of thos who claim they are more productive on campus.

Tables 2.1-2.3 along with Figure 2.1 support the general result that being outside the lab has a significant negative impact on the productivity of most subjects when asked to perform the typing task. This general result holds regardless of the group looked at or the method of analysis used.

2.3.1 Creative Task

The primary task in part 2 was coming up with unusual uses for common objects. There was a total of 15 minutes split into five, 3 minute rounds. The validity of the subjects' submissions was judged by two independent judges. The number of valid uses reported will be the average number of the two judges. In the lab, subjects produced on average 6.05 valid uses while outside the lab, surprisingly, subjects averaged 6.96. A t-test shows this difference is statistically significant at $p < 0.01$ ($t = 2.81$).

Figure 2.2 shows how much more productive someone in the lab is vs. their same productivity counterpart outside the lab for the creative task.

As in part 1, this difference needs closer inspection to determine if the environment affects some proportions of the population differently. Figure 2.2 is similar to Figure 2.1 in that it shows the percentage increase in productivity by being in the lab organized by the least to the most productive subjects from left to right. Figure 2.2 highlights that those outside the lab are more productive at every level of productivity as the trend line comparing the two groups of subjects never goes above the 0% line. Also, as in the typing task, it seems apparent that the environment affects the upper and lower productivity groups the most.

If the subjects are again split into thirds based on their productivity, t-tests confirm the statistically significant difference of the lowest productivity subjects ($t = 3.21$) and the highest productivity subjects ($t = 2.04$), but the difference is not statistically significant for the middle third ($t = 1.54$). Table 2.4, similar to Table 2.1 , presents the means and t-statistics for the groups identified in hypotheses 2-5.

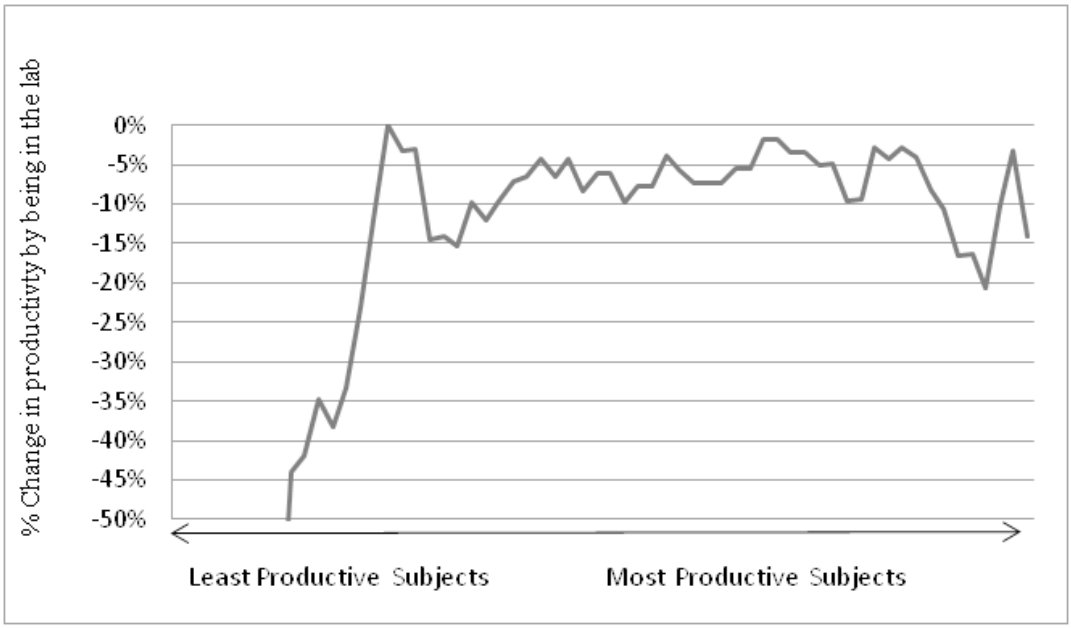


Figure 2.2: Productivity of those Inside vs Outside the Lab: Creative Task

Table 2.4 shows that there is statistical support for the increased productivity of the subjects outside the lab in four of the eight groups. It is again important to conduct a more careful regression analysis controlling for the many factors a simple t-test is unable to capture. Regression results are presented in Tables 2.5 and 2.6 using the same methodology as in part 1 with the exception that now the dependent variable is the average number of valid uses per round.[14]

The first thing to notice from Tables 2.5 and 2.6 is that all of the coefficients for lab are negative. This means that being in the lab has a negative impact on productivity when subjects perform the creative task. This is the opposite of the result for the typing task. Out of these coefficients, there are six that are significant at the 10% level or better. These results are in support of hypotheses 2 and 4, while they go against hypothesis 1.

[14]It was also verified that productivity in the first task did not affect productivity in the second task.

Table 2.4: Means and t-tests of hypotheses 2-5: Creative Task

		In the lab Mean	*Outside the lab* Mean	$T-stat$ for difference
Hypothesis 2	Male	6.19	7.65	3.35***
	Female	5.87	6.38	1.07
Hypothesis 3	Procrastinate	6.05	6.97	2.49***
	Do not Procrastinate	6.08	6.88	1.30
Hypothesis 4	Need Personal Control	5.80	7.29	2.99***
	Do not need Personal Control	6.21	6.69	1.15
Hypothesis 5	More Productive on Campus	5.86	6.39	1.33
	Less Productive on Campus	6.50	7.63	2.13**
*** p<0.01, **p<0.05, * p<0.1				

Table 2.5: Regressions for Hypothesis 1

	No Interactions	Interactions for level of productivity
Lab	−0.905* (0.523)	
Lab*Lower third		−1.145*** (0.378)
Lab*Middle third		−0.705*** (0.221)
Lab*Upper third		−0.963* (0.555)
Male	1.092** (0.537)	0.250 (0.236)
Procrastinate	0.246 (0.618)	0.307 (0.281)
Need Control	0.241 (0.536)	0.235 (0.240)
Productive Campus	−1.099* (0.591)	−0.009 (0.274)
Errors clustered around individual, Control variables not shown: gpa and round *** $p<0.01$, **$p<0.05$, * $p<0.1$		

Results for the creative task:

Result 6: Overall, subjects are more productive outside the lab when performing the creative task which is in support of hypothesis 1

Result 7: Males are about 17% more productive outside the lab than in the lab while there is no statistical difference in the productivity of females by location.

Result 8: There is no statistical evidence to support the argument that non-procrastinators

Table 2.6: Regressions for Hypotheses 2-5

	Male	Procrastinate	Need Control	Productive on campus
Lab*In Group	−1.224* (0.690)	−1.217 (1.073)	−1.435* (0.823)	−1.294 (0.917)
Lab*Out Group	−0.566 (0.821)	−0.787 (0.618)	−0.535 (0.705)	−0.666 (0.649)
Male	1.416* (0.753)	1.098** (0.538)	1.073** (0.539)	1.052** (0.543)
Procrastinate	0.235 (0.613)	0.482 (0.918)	0.183 (0.625)	0.293 (0.628)
Need Control	0.228 (0.541)	0.220 (0.546)	0.679 (0.770)	0.241 (0.537)
Productive Campus	−1.046* (0.601)	−1.126* (0.600)	−1.087* (0.593)	−1.390* (0.834)
In Group refers to the corresponding column dummy variable when it is equal to 1 and Out Group refers to the corresponding column dummy variable when it is 0. Errors clustered around individual, Control variables not shown: gpa and round *** $p<0.01$, ** $p<0.05$, * $p<0.1$				

are affected by the environment and the statistical significance found in the t-test showing that procrastinators are more productive outside the lab goes away in the more careful regression analysis.

Result 9: Those who need personal control are about 20% more productive outside the lab while the environment has no statistical affect on the productivity of the group who do not desire more personal control.

Result 10: Subjects who claim to be more (less) productive on campus (off) campus do not seem to be affected by the location. Again, the significance of the t-test results go away once the regression analysis is ran.

As in the typing task, the environment affects a large number of the subjects when performing the creative task. The affect of being in the lab is reversed for this task though as it has a positive impact on the productivity of the subjects when subjects are performing the creative tasks.

2.3.2 Conclusion and Discussion

There exists contradictory claims on the productivity of teleworkers and the academic literature to date does not offer satisfactory answers. This paper adds to the current

knowledge by providing a baseline of the environmental effects on worker productivity in two distinct tasks; mundane and creative. These two categories mimic the work done in most industries wishing to implement a telecommuting policy. This study used a simple, sterile experiment with a piece-rate pay scheme to show that the environment does affect productivity in an intuitive manner. It was shown that the out of lab environment reduced productivity in the mundane task by 6% – 10% depending on the group examined. Conversely, the out of lab environment increased productivity in the creative task by 11% - 20%.

Caution should be exercised that these results not be over-generalized. The work setting here has some potentially important omissions. With these concerns in mind, the main results of this paper have practical implications for how workers are chosen to work away from the office. If the work to be performed resembles data entry, a productivity decrease may be likely if it is performed outside the office. If on the other hand the task is more creative in nature, it may be in a manager's best interest to allow their employees to work outside the office.

A reasonable question that arises from these results is what elements of the environment matter most in increasing worker productivity for the two tasks? In this study, the subjects in the lab were asked to perform the experiment at a time they agreed to, but set up by the experimenter. More flexibility in the workplace may be the key to the increase in productivity for creative tasks. Less structured environments are currently used by many companies, such as Google, which rely on creativity. Dickinson and McElroy (2009) show that when subjects are asked to perform p-beauty contest games at suboptimal times of the day according to their sleep schedule, lower levels of iterative reasoning results. There may be other important factors that lead to productivity increases. For instance, peer effects were shown to be important in low level tasks in field environments by Mas and Moretti (2009) and experimentally by Falk and Ichino (2006). These peer affects may have resulted in an increase in productivity of the subjects in the lab when performing data entry tasks since these subjects could hear others typing.

In addition to the direct implications of the study, there may be tangentially related issues. Hays (1999) estimated that there will be a loss in productivity when employees are allowed to use the internet for purposes other than work. The current study shows that the exclusion of additional distractions may or may not be important in the work setting

depending on the task performed.

There are many directions in which the baseline study outlined in this paper can be taken. One such direction will deal with telecommuting work which involves more team-oriented outcomes. These outcomes include more complex interpersonal interactions that make measuring individual contributions more difficult in the field environment. Another direction would be to vary the amount of risk being taken on by the manager and the employee. This could have implications on how contracts are formed for telecommuters. Once these elements are included, the results may be very different but it will be easier to point to these additions as causing the increase since this study lays the foundation for understanding these other factors in congruence with the work setting.

2.4 Appendix

CHAPTER 3

PUNISHMENT HISTORY AND SPILLOVER EFFECTS: A LABORATORY INVESTIGATION OF BEHAVIOR IN A SOCIAL DILEMMA

3.1 Introduction

Punishment is a tool commonly used to discourage bad behavior in favor of good behavior. Though it is unlikely that a punishment mechanism will possess such a narrow scope that it only alters the behavior of the one punished, very little research in economics has considered the broader impacts of observing punishment.[1] This implies that our current understanding of punishment effects may be incomplete. For instance, punishment could be more effective than currently thought if the observers of punishment subsequently reduce their own undesirable behavior. While less likely, the opposite is also possible where the gains in behavioral modification of the punished individual could be offset by an adverse reaction from observers of the punishment. It is the goal of this paper to strengthen our understanding of punishment mechanisms by using experimental methods to explore how the observation of others being punished might affect behavior.

Punishment is intended to discourage undesirable behavior or actions,[2] with effectiveness shown to depend on its frequency, intensity and immediacy (Anderson and Stafford 2003, Johnston 1972). Since Becker's seminal work in "Crime and Punishment: An Economic Approach" (1968), economists have become increasingly interested in the behavioral effects of punishment. The importance of punishment spillovers in organizations is apparent (Bandura 1971). Punishment attached to low effort conveys the information that adherence to a

[1] see Xiao and Houser 2010

[2] e.g. Andreoni, Harbaugh, Vesterlund 2003, Fehr and Fischbacher 2004, Ostrom, Walker and Gardner,1992, Xiao and Houser 2010

behavioral standard is socially beneficial and supports a collective goal. Punishment therefore provides information on the acceptability of a behavioral standard, as well as providing an incentive to adhere to it.[3] However, when free riding incentives are present, as in many organizational environments with public goods features, the individual decision maker still faces a behavioral dilemma even with punishment potential.[4] Our paper is novel in that it experimentally investigates whether observing punishment of someone else in a social dilemma setting can increase one's own socially beneficial behaviors (even when there is no chance that the observer will be punished).

Because of the difficulties in isolating the variables of interest in a field setting, we use laboratory methods to conduct our analysis of punishment spillovers. Our design uses a simple social dilemma environment (i.e., a public goods game) that produces a well-defined outcome variable. If subjects respond to observing punishment by contributing more towards public goods, this would imply vicarious punishment is real and that we currently underestimate the beneficial behavioral effects of punishment mechanisms. On the other hand, if observed punishment reduces cooperative behavior—the direction of punishment spillovers are contrary to the usual expectation—then researchers have overestimated the ability of punishment to reduce undesired behaviors in a broader sense

3.2 Literature

Outside economics, researchers have invoked social learning research to hypothesize that observed punishment leads to a decrease in undesirable behavior (see Arvey and Jones 1985, Trevino 1992 and references therein). In the psychology literature, there has been significant interest in the topic of vicarious punishment (see Malouff, Thorsteinsson, Schutte, and Rooke 2009 and references therein). Specifically, Malouff et al (2009) is a meta-study of psychology experiments examining the existence of vicarious punishment, or "whether the observer is, in effect, punished." They find that, in general, subjects react in the predicted direction which can be explained by learning in their setting. In addition to the learning story,

[3] Of course, the effectiveness of punishment depends greatly on the group members' regard for the established standard. If a boss institutes a particular rule that he sees as integral to the success of the firm, but that the employees find useless, punishment for breaking this rule will not improve behavior and in fact may have the opposite effect (Trevino 1992).

[4] Also, while severe punishment penalties may be effective at reducing undesirable behavior, such punishment may be used infrequently due to prohibitively high costs (Xiao and Houser 2010).

these studies also use nonstandard subject pools (most used child subjects which may not be applicable to our setting) and outcome measures that are not clearly defined or tasks that are not incentivized, which introduce potential confounds or a lack of saliency into the environment. In what follows, we will refer to a more general "spillover effect", noting that the term "vicarious punishment" implies the intended deterrent effect of punishment, while the general term is more flexible.

Experimental economics research has shown that punishment can be effective in sustaining cooperation in public goods settings (i.e., the decision setting we use in our experiments), and in some cases may be more effective than the use of positive reinforcement (See Dickinson 2001, Masclet, Noussair, Tucker and Villeval 2003, Noussair and Tucker 2005, Sutter, Haiger, and Kocher 2010). However, Anderson and Stafford (2003) find that past punishment has a negative effect on compliance. Nevertheless, these studies only examined the direct effect of punishment on the individual being punished. The question of punishment spillovers has not been adequately addressed, though some indirect effects have been identified.

Results from the laboratory have shown that third parties care about others' behavior even when they are strictly observers with no financial stake in the outcome (Fehr and Fischbächer 2004, Croson and Konow 2007). A separate strand of experimental economics literature has documented the impact punishment can have on adherence to social norms (Bendor and Mookherjee 1990, Ostrom, Walker, and Gardner1992, Fehr and Gächter 2000, Anderson and Putterman 2006, Carpenter, Mathews and Ong'Ong'a 2004, Xiao and Houser 2010). Thus, there is reason to believe that those observing punishment may change their own behavior when involved in a social dilemma where social norms play an important role.

In addition to examining the spillover effects of punishment, we also explore the role that social distance between punished agent and observer plays in the strength of the spillover effect. Trevino (1992) shows that reduced social distance increases social learning and motivation, but the direction of the net behavioral change is not clear. Reduced social distance between team members may enhance social learning of observed punishment such that the observer is more likely to avoid the prohibited behavior. However, other findings suggest that the observer may be more prone to respond with negative emotions, attitudes and behaviors (Brockner, Grover, Reed, DeWitt, and O'Malley 1987, Brockner 1990, and Brockner & Greenberg 1990). Dickinson and Villeval (2008) find that reduced social distance crowds out the effectiveness of negative sanctions (i.e., increased monitoring),

though they examine principal-agent relationships as opposed to peer relationships with exogenous punishment. Overall, existing research highlights the interest in and relevance of this topic, but we contribute to the literature with several design elements that allow a clean estimation of punishment spillover effects.

3.3 Model

Punishment is often implemented when self interested behavior negatively impacts others. Such is the case in a social dilemma, which led us to use a voluntary contributions mechanism (VCM) game in our experimental design. In a VCM game, a subject is given an endowment of tokens, E, which represents money or an endowment of ability. A subject's choice, x, is the amount contributed (money or effort) to the group and the subject keeps the remainder, $E - x$. The tokens are converted to money where the tokens kept return α and the tokens given to the group account return β to each of the N individuals in the group where $\alpha > \beta$ and $\alpha < N * \beta$. Given this standard structure, utility with no punishment, $U_{i,N}(x)$, can be specified as

$$(1) \qquad U_{i,N}(x) = \alpha(E - x_i) + \beta \sum_{j=1}^{N} x_j$$

where x_i is the amount contributed to the group account by subject i. The above assumes the standard typical money-maximizing agent. With these assumptions, complete free-riding is the predicted outcome since utility is maximized at the boundary where $x_i = 0$. Because of this outcome, punishment is seen as a way to increase contributions to the group. When punishment is added, the utility function, $U_{i,P}(x)$, is now

$$(2) \qquad U_{i,P}(x) = \alpha(E - x_i) + \beta \sum_{j=1}^{N} x_j - I(\overline{x}) * (\Pr * d)$$

where Pr is the probability of punishment of level d and $I(\overline{x})$ is an indicator function equal to 1 if the individual's contributions to the group are below a threshold level of $\overline{x}$ and zero otherwise. With this addition, contributions to the group will depend on the threshold, the probability of punishment and its severity. Prior studies looking at observance of punishment have focused on manipulating these variables and/or the information available about them. As an example, the psychology study by Schnake (1986) studied college students' reaction

to a confederate being punished for low output. The subjects in this study had no idea that punishment for low effort was possible. Once they observed punishment of someone else, they were able to update their prior (presumably incorrect) beliefs of $I(\overline{x})$, Pr and/or d. In other words, subjects were maximizing $U_{i,N}(x)$ before observance of punishment, and $U_{i,P}(x)$ after observing punishment. In a related economics study, Xiao and Hauser (2010) examine how the observance of endogenous punishment to fellow group members affects contributions. In this instance, subjects can be assumed to be maximizing $U_{i,P}(x)$ with uncertainty about $I(\overline{x})$, P and d. Since maximization of this utility depends on these variables, behavior can be expected to change as more information is gathered about the relevant variables. The effect can again be viewed in terms of updating prior beliefs of what a subject views the relevant punishment variables are. These are important studies in understanding how subjects update their prior beliefs.

Up to this point utility has been defined in terms of monetary payoffs. As decades of research have shown, utility is more general than that. Our study diverges from prior studies by examining the behavioral implications of observing punishment that has no direct impact on one's monetary payoff. Our interest lies in understanding how the observance of punishment in an independent social dilemma may affect one's own-group contributions (i.e., contributions in a separate and distinct social dilemma). More specifically, we examine a utility function of agent i where she only observes punishment and this observation will not affect her own monetary payoffs. Consider utility, $U_{i,C}(x)$, defined as

$$(3) \qquad U_{i,C}(x) = \alpha(E - x_i) + \beta\sum_{j=1}^{N} x_j + C(x_i, P|V)$$

where $C(\cdot)$ is a function accounting for the psychological cost (or benefit) of observing punishment.[5] This addition to the model allows us to think how an agent may increase her utility by either increasing or decreasing her contributions after the observance of punishment. In this function, P is an indicator equal to one if punishment is observed while V represents personal characteristics which map the agent's reaction to observed punishment into her contributions, x_i. Notice that $\frac{dC(\cdot)}{dP} = 0$ when $P = 0$. In essence, the personal characteristics, V, determine if the observance of punishment is a psychic cost or benefit

[5] See Ku and Salmon (2010) for a similar model.

which in turn will lead the agent to increase or decrease contributions. In other words, V determines the sign and magnitude of $\frac{d^2C(\cdot)}{dx_i dP}$ when punishment is observed. V will include, among other things, a history of punishment, one's natural tendency to be a "cooperator" or other relevant features that may change individual behavior after observance of punishment. One of our objectives is to uncover some of the specific factors that influence the sign and magnitude of $C(\cdot)$. Behaviorally, if a characteristic in V determines that the sign of $\frac{d^2C(\cdot)}{dx_i dP}$ is negative, this means that an increase contributions after observance of punishment will lead to an overall net decrease in utility and thus an agent should decrease contributions in order to increase utility.

Notice that in model (2) the personal experience of punishment affects contributions through money maximization while the observation of punishment in (3) does not affect monetary outcomes. As such, a strict money-maximizing agent will not change her behavior and will contribute zero to the group regardless of observing punishment or not. In other words, Model (3) reduced to Model (1).

Hypothesis 1: If agents are money-maximizers, behavior will not change when punishment is observed.

Above we assume that the punishment variables are fixed and that agents have full information on these variables and that they know they will not be punished. For the more general case of utility maximization shown in (3), observed punishment may increase or decrease contributions, depending on agent perceptions of the fairness of the punishment mechanism. This may occur for a variety of reasons. In line with Trevino's proposition 4a, an anti-social reaction can occur if the punishment is viewed as unfair.[6] Nevertheless, we have noted in Section 2 that the majority of the existing research on punishment in public goods games finds that it increases cooperative behaviors among those directly punished. Thus, our initial hypothesis regarding behavioral effects of observed punishment follows:

Hypothesis 2: The observance of punishment will lead a subject to increase her own contributions.

[6]This is similar to Frey's (1993) hypothesis that monitoring workers may "crowd out" the intrinsic motivation to perhaps the job task. While Hypothesis 2 refers to the observation of punishment, one might argue that this could reduce the intrinsic motivation of being socially cooperative.

For observed punishment to affect behavior, whether in the pro-social or anti-social direction, the agent must internalize the observed punishment as if it were her own. Our experimental design includes two features that may increase the saliency of the punishment. The first feature is a treatment where personal and direct experience with a punishment mechanism is given before subjects are strictly observers of punishment. In order to internalize the punishment as happening to them, agents must first know how they would react to being punished. Thus, it may be important for an agent to have experience with a punishment mechanism before they can internalize observed. This leads to our third Hypothesis.

Hypothesis 3: Direct experience (history) with a punishment mechanism will help subjects internalize the observation of punishment, thus strengthening the behavioral effects.

Notice that Hypothesis 3 is general and testable even if our data do not support Hypothesis 2. Should we find evidence of anti-social punishment effects in our environment, support for Hypothesis 3 would imply that observed punishment causes similar behavioral effects (thus strengthening the total behavioral effect of punishment).

The second design feature we include is a social distance manipulation between the one being punished and observer. Reduced social distance should lead to the observance of punishment being more personal and thus we expect that the magnitude of the reaction to be increased when social distance is decreased in line with Trevino's (1992) proposition 1c. This leads to our fourth Hypothesis.

Hypothesis 4: Decreasing social distance between two agents will help internalize the observation of punishment, thus strengthening the behavioral effects.

3.4 Experimental Design

The experiments were conducted in the xs/fs lab at Florida State University. Subjects were assigned computer terminals at random and used software programmed in z-Tree (Fischbacher, 2007). In what follows we refer to super-groups and VCM groups to avoid confusion. For example, if a session involved 18 subjects, two super-groups of 9 subjects are randomly formed and remain fixed for the entire experimental session. We will refer to these groups as super-group 1 and super-group 2. Subjects will play 3-person VCM games

with other subjects within their super-group, and we refer to these 3-person groups as the VCM groups. A super-group of 9 subjects allows us to randomly form new VCM groups every round from within one's super-group. Having two super-groups allowed us to randomly match each subject from super-group 1 with a subject from super-group 2 for the purposes of observed punishment treatments.

In each session, there were three games played by each VCM group. We will refer to the three as Game A, Game B, and Game C. Game A is a standard VCM game, which is used as a baseline and gave subjects experience playing the game and learning about the social norms of others. This is important since the theory dictates that we control for awareness of these norms. Subjects were asked to allocate 10 tokens into an "individual" or "group" account in whole token increments. Tokens allocated to the individual account yielded \$0.025 to that person alone and tokens allocated to the group account returned \$0.0125 to all three members of that subject's VCM group. Thus, a money-maximizing agent would prefer to put all 10 tokens in the individual account (earning \$.25) while the social optimum is for all subjects to put all 10 tokens in the group account (earning \$.375). The only real difference in our baseline compared with many other standard VCM games is that super-group 1 subjects always made decisions first, sequentially followed by the decisions of super-group 2. Since our main interest is examining how others react to seeing someone punished, a sequential move was needed. Game A does not allow for punishment, but the sequential move structure is needed to ensure that results in Games B and C are not simply a function of sequential decisions across super-groups 1 and 2.

Game B was similar to A except that a punishment mechanism was introduced. Punishment was only possible for VCM groups within super-group 1 in Game B. If a VCM group member contributed less than 5 tokens to the group account, there was a 50% chance of being punished by losing \$0.25 (equal to an expected punishment cost of \$0.125, or 5 tokens) of their period earnings. This still leaves the strategy structures weakly intact in that a risk-neutral money-maximizing agent would be indifferent between complete free riding and contributing 5 tokens to the group account where their expected payoff is \$.075 in each case. The word "punished" was specifically used in the experiment for the exogenous punishment mechanism.

The size and probability of punishment were structured so that we would have a reasonable number of subjects being punished, which is necessary to test our predictions.

In addition to the punishment mechanism for super-group 1, recall that each subject was matched with an "other-group counterpart" from the other super-group. These counterparts remained fixed throughout the experiment and never interacted in the VCM games. In each round of Game B the subjects in super-group 1 made a decision and outcomes (i.e., private payoffs, group payoffs and punishments) were determined. Thereafter, the partner from the unpunished super-group 2 is informed whether her other-group counterpart was punished or not and how often they had been punished previously and the costs of such punishment. Those in super-group 2 then made a decision in their respective Game B where they personally do not have the possibility of being punished. Because other-group counterparts never play each other, any behavioral response by the unpunished group can't be due to an attempt to increase the first person's contribution nor can it be reciprocal as a means to rewarding the first person.

Game C is similar to Game B except now subjects from both super-groups 1 and 2 are subject to the same punishment mechanism. Thus, subjects in super-group 2 can observe punishment and can be directly punished themselves in Game C. The sequence of decisions was the same. Subjects played all three games in a single session where Game A was always played first. To control for order effects across punishment treatments, half the experiments were ran in game order ABC and half in order ACB. Counterbalancing the treatment order for Games B and C is also necessary to generate the differential history needed to test hypothesis 3.

In addition to the above, the social distance of the other-group counterparts varied according to two social distance treatments: friends and strangers. In the friend treatment, social distance is reduced using the following methods. After subjects were seated, but before instructions for the game were distributed, subjects were given a list of general topics to discuss with the person next to them for a short get-to-know-you-session. This person was eventually the "other-group counterpart" in the experiment and they knew this. Subjects were told they had five minutes in which to talk to the person next to them. In the stranger treatment subjects were simply assigned an "other-group counterpart" (OGC) at random. Neither "friends" nor "strangers" were allowed to talk after the experiment began. Once the experiment began, the subjects kept the same "other-group counterpart" throughout the experiment. Recall that the person from super-group 2 observed the punishment outcome of their OGC in super-group 1. Table 3.1 gives a summary of the treatments and the number

of subjects in each treatment.[7]

Table 3.1: Number of subjects per treatment

	Chatted with other group counterpart	
Order of Game Play	Yes (Friends)	No (Strangers)
ABC	18	24
ACB	18	36

Game A was the baseline. In Game B, only first movers could be punished while in Game C both first and second movers could be punished.

Before each treatment, subjects were given written instructions that were read out loud by the experimenter and were followed by a short quiz to ensure subjects understood the instructions. Once all subjects completed the quiz, the first 10-period treatment (Game A) started. In the instructions, subjects were given information only about the current treatment but knew others would follow. The same procedure was carried out for all three 10-period treatments of the experiment where instructions were read out loud followed by a quiz. A total of 96 subjects participated in the experiment (recruitment used ORSEE software (Greiner 2004)). On average, subjects made $20.46 for about an hour of their time.

3.5 Results

Game A was meant to make sure subjects understood the computerized game interface, the social norms in this game and generated baseline predictions on initial levels of cooperative behavior. In Game A, subjects contributed an average of 2.3 tokens to the public account over ten periods. The decline typical of VCM games is evident as first period contributions averaged 3.0 while last period contributions averaged 1.9. Since our interest lies in vicarious punishment, the behavior of the second movers is most relevant. On average, second movers

[7]The first session, represented in the upper right quadrant of Table 1, had 24 subjects and the return on the public and private accounts were doubled compared to the other sessions. When this treatment is compared to the treatment with chat and the same game order, there is no statistical difference in second-mover's average contributions (t-test, p-value = .432). We also ran two 18 subject sessions of the no chat, ACB treatments since subjects in the first were initially handed the instructions with the doubled payoffs. The error was quickly caught and they were handed the correct instructions. The appendix has regressions ran with and without this session showing that the basic results do not change.

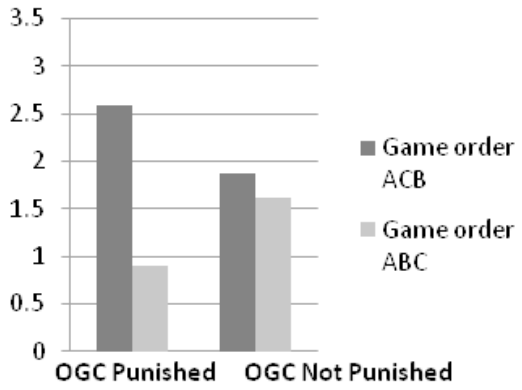


Figure 3.1: Average contributions, Game B, Punished, Game Order

contributed 2.4 tokens to the public account in Game A, with contributions declining over the 10 rounds.

Game B allows us to observe the behavior of the subjects when they saw their other-group-counterpart (OGC) was punished (or not), but they themselves could not be punished. In Game B, there were a total of 480 observations (48 second movers who played the game 10 times). The average amount contributed by second movers in Game B was 1.8. Of these 390 observations, second movers observed their OGC being punished 84 times. There was no statistical difference in contributions conditional on observing if a subject's OGC was punished or not (1.8 vs. 1.8). These summary statistics may mask underlying trends though.

In the left portion of Figure 3.1 we see that when second movers in game B already have experience playing game C, their average contributions of 2.6 are much higher if they see their OGC punished than average contributions of .9 without this experience. This difference is statistically significant (t-test, p=.01). In the right portion of Figure 3.1, it is shown that contributions when a subject's OGC was not punished are statistically similar for the two treatment orders (p>.10). Because the reaction to seeing one's OGC punished depends on the treatment order, this implies that experience with punishment is an important determinant of the vicarious punishment effect.

Figure 3.2 looks at the same breakdown with regards to the chat treatment. On the left

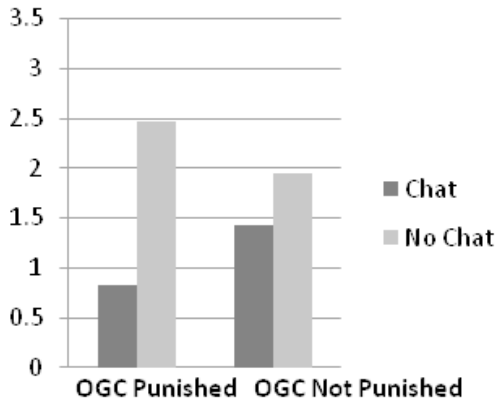


Figure 3.2: Average contributions, Game B, Punished, CHAT

hand side , it is shown that those who saw their OGC punished contributed significantly less if they chatted than if they did not chat (t-test, p=.01). On the right side of Figure 3.2, we see that there is a significant "chat" effect for those not observing OGC punishment as well (t-test, p=.05). Thus, chatting with someone seemed to lower overall contributions, even more so when subjects observed their OGC was punished.[8]

In addition to the above analysis, it is also useful to examine how observing punishment affects those subjects who are more inclined to be generous versus selfish (see Figures 3.3 and 3.4). Figure 3.3 looks at the contributions in Game B of those subjects who contributed more than the average person in the first period of Game A. While a significant difference is not observed in the first 5 periods, a statistically significant difference is found in the final rounds of Game B (t-test, p<.01). Thus, subjects who are initially more generous are contributing much less in the final 5 periods if they observe their OGC was punished. Figure 3.4 shows the opposite effect in the final rounds for below average contributors (t-test, p<.01).

Though the above analysis is suggestive, a more comprehensive econometric analysis controlling for multiple factors is needed to fully examine our data. We use a random effects model to account for unobserved individual effects and cluster on the individual level

[8]Though not the focus of the paper, something worth further exploration is why chat seems to lower contributions. Recall also that this result does not refer to chatting with individuals within one's own social dilemma group.

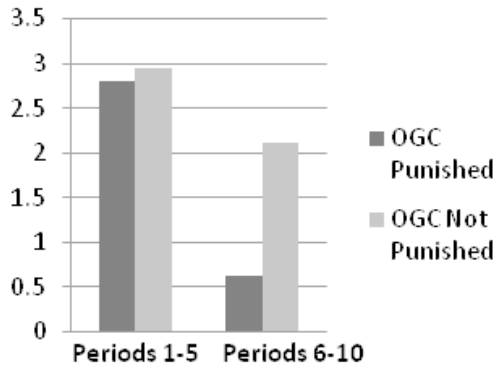


Figure 3.3: Average contributions in Game B, Above Average Contributors

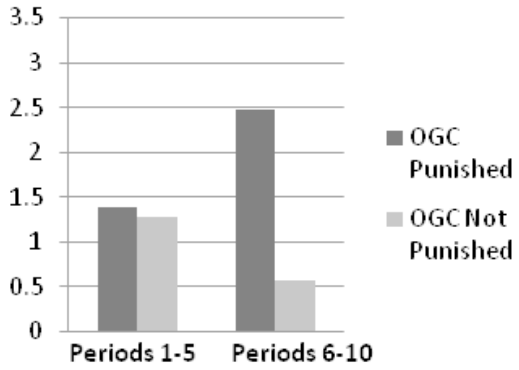


Figure 3.4: Average contributions in Game B, Below Average Contributors

to control for correlation of error terms in our panel data. The dependant variable for cooperation is "tokens contributed" to the public account by second movers. The main explanatory variable is OGC Punished, which equals one if they saw their partner was punished and zero otherwise. Recall that a money-maximizing agent has a weakly dominant strategy to contribute nothing to the public account and, specifically, second movers have identical monetary incentives to contribute in Game B compared to the baseline Game A because second movers do not face a punishment threat in Game B. Thus, Game B allows a more clean examination of the vicarious punishment effect compared to Game C data.

Model 1 results in Table 3.2 are from a basic regression on Game B data that includes only OGC Punished and a variable for the decision round, Period, as explanatory variables. The significant negative coefficient on Period implies declining contributions across rounds as is typical in traditional VCM games, but observance of one's OGC being punished (i.e., vicarious punishment) is insignificant. The above figures, however, imply that additional controls are needed in the analysis (i.e., models 2-5).

Model 2 (Table 3.2) includes additional dummy variables for Chat (=1 for reduced social distance treatments) Game ACB (=1 when subjects in Game B have already played Game C where they faced their own punishment threat) and a continuous variable for First Period Contributions. From Model 2, notice that there is no main (pure) effect of Chat on contribution levels. What is shown in Model 2 is that having already played game C significantly increases the contributions of second mover subjects in game B. Additionally, a positive correlation is seen between first period contributions and contributions in Game B.

Model 2, however, cannot discriminate between whether the previous history of Game C play increases Game B contributions because of past OGC punishment or because of past direct punishment (as both are possible in Game C). To explore whether Game ACB matters differentially when one observes counterpart punishment or not, or whether Chat and First Period Contributions interacts with OGC punishment, we add interaction terms to produce ourfinal Models 3, 4, and 5. Model 3 is estimated for data on all 10 periods of game B contributions, whereas Models 4 and 5 are estimates based on data from periods 1-5 and 6-10, respectively.[9]

From the estimates in Models 3-5, the general decline in contributions over time is robust (Period estimate), as is the pure effect of punishment treatment history reflected in the Game ACB variable. We now turn to Hypotheses 1-3. From these more complete models 3-5, it is clear that the vicarious punishment effect depends on the history of play. Observation of punishment leads to a decrease in contributions when second movers have no history of Game C play (coefficient estimates on OGC Punished in models 3 and 4), but it leads to increased contributions when second movers have a history of Game C play (coefficient estimates of Game ACB*OGC Punished in models 3 and 4). One can also see from Models 3, 4, and 5

[9]The fifth period was arbitrarily chosen because it is the halfway point in the Game. Our results are robust to changing this cutoff point between early and later rounds. We also control for gender effects in Models 2-5, though gender is statistically insignificant in all instances.

Table 3.2: Regression Analysis: Contributions in Game B

Variable	Model 1	Model 2	Model 3 (all periods)	Model 4 (periods 1-5)	Model 5 (periods 6-10)
OGC Punished	−.400 (.311)	−.359 (.306)	−.993* (.602)	−1.657*** (.556)	.484 (.781)
Period	−1.24*** (.034)	−.172*** (.040)	−.175*** (.040)	−.336*** (.115)	−.154* (.083)
Game ACB		2.284*** (.598)	2.065*** (.610)	3.223*** (1.219)	2.319*** (.748)
Chat		−.445 (.488)	−.377 (.485)	−.301 (.538)	−.440 (.525)
1st Period Contributions/Game ACB		.190** (.091)	.191** (.087)	.277*** (.096)	.116 (.098)
Game ACB/OGC Punished			1.464*** (.543)	2.490*** (.735)	.421 (.730)
Chat /OGC Punished			−.366 .497()	−.595 (.595)	−.572 (.765)
1st Period Contr/OGC Punished			.008 (.129)	.224* (.128)	−.441** (.198)

Notes: Standard errors are corrected for clustering at the subject level.
Three (***), two (**), and one (*) stars indicate statistical significance at the 1%, 5%, and 10% respectively.

that some of the key behavioral effects may be limited to the first half of the game (rounds 1-5), and also differ based on the propensity of a subject to contribute initially (e.g., see First Period Contr variable interaction term). Overall, we find:

Result 1: Agents are not simple money-maximizers. Observed punishment affects behavior.

We find support for Hypothesis 2. Independent of punishment history, we find that observed punishment decreases contributions to one's own VCM game, which is consistent with the idea that the punishment mechanism is viewed as unfair. Interestingly, in the last half of the game (rounds 1-5) this effect appears to be stronger for those subjects who initially were the most cooperative.[10] The impact of punishment history will be explored further below. Thus, we have:

[10] We find evidence that the adverse punishment spillover effect in game B (treatment order ABC) is less severe in the first 5 rounds for those subjects with higher first period (game A) cooperation. That is, in Model 4, we estimate a statistically significant positive coefficient on the interaction term First Period Contr.*OGC Punished. The pure effect of being initially cooperative is to be more cooperative following OGC punishment, though the magnitude of this effect is overwhelmed by the main effect of observed OGC punishment. Thus, absent the history of game C play, the decrease in contributions following OGC punishment is less severe in the initial rounds for the most initially cooperative subjects, though this interaction effect reverses in rounds 6-10 (Model 5).

Result 2: Observed punishment (controlling for punishment history) has the adverse (anti-social) effect of decreasing contributions.

As for Hypothesis 3, while history with punishment matters, it reverses the sign of the behavioral effect on observed punishment, which is not consistent with Hypothesis 3 but merits further analysis (see below).[11] The lack of significance on the key interaction term in model 5 indicates that this punishment-history result is concentrated in the first 5 periods of play in Game B. We therefore have:

Result 3: History or experience with a punishment mechanism significantly impacts the observed-punishment effect. However, it reverses rather than strengthens the effect.

In other words, Result 3 highlights that subjects with direct punishment history become more cooperative when observing punishment, whereas the no-history tendency is for observed punishment to decrease cooperative behavior in one's own social dilemma. This effect is estimated to occur only in the first half of the Game B data (rounds 1-5), so it appears that the history effect may be more short-term.

Finally, we examine Hypothesis 4 by referring to the Chat*OGC punished interaction term. It is clear from all model estimates that Chat does not have a significant main effect or interaction effect with observed punishment. Thus, the data do not support Hypothesis 4.

Result 4: Social distance does not affect contributions nor does it impact the behavioral effect of observed punishment.

3.5.1 Further Analysis using Game C data

Result 3 suggests that experience with punishment matters in how subjects react to seeing their OGC punished. Game C data can help us test which part of experience matters by comparing subjects who only observed to the punishment of others to subjects who had only direct punishment. Specifically, we examine second-movers who played game B before game C and compare their initial two rounds of contributions in Game C with the contributions

[11] Of course, here we refer to immediate experience with punishment institutions in the same experimental session, as opposed to one's experience with VCM game punishment from a previous experiment, although it would be of interest to explore the limits of any experience effect in future research.

of first-movers in the initial two rounds of Game C. Thus, we are comparing first mover and second movers who have played the same number of total periods, but the second movers' history in the 10 rounds of the prior game was only observed punishment history. For first movers, the previous treatment history involved a direct punishment institution.[12] From this comparison, we can discern if direct or observed punishment history has more impact on cooperative behavior. We arbitrarily focus on the first two rounds of the final game to avoid the buildup of new history effects on first- and second-mover contributions. Those with only direct punishment experience contributed an average of 4.3 tokens, while those who only observed punishment contributed a significantly higher 5.3 tokens (t = -1.89, p=.06).

Result 5: Experience with observed punishment has a stronger effect than experience with direct punishment in promoting cooperative behavior in Game C (everyone can potentially be punished).

Results highlighted previously in Figure 3.1 indicate that history with the direct punishment institution (game C) is a significant predictor of how second movers respond to observed punishment. That result did not distinguish second movers who actually experienced direct punishment from those who did not. We now consider the subset of second movers who played and were punished in game C before they played game B (n=16) to those who played game C but were never punished (n=11). We again focus on the initial two rounds of behavior in the final game before confounds build up. For the subset having played and been punished in game C, those who observed OGC punishment contributed 3.3 tokens on average compared to 1.2 tokens for those who did not observe OGC punishment (t-test, p=.06). For the subset of subjects having played Game C but never were punished, those who observed OGC punishment contributed 4.5 tokens on average, compared to 3.2 tokens for those who did not observe OGC punishment, which is a statistically insignificant difference (t-test, p = .53). Thus, while consistent with Figure 3.1 results, this shows more explicitly that direct punishment history plays a significant role in shaping cooperative behavior in the presence of observed punishment.

[12]Strictly speaking, the analysis would compare only those subjects experiencing direct punishment to those only having experienced observed punishment. This greatly reduces our sample size such that we would have low power to detect a significant effect, and such a comparison might also be criticized on the basis of sample selection regarding first movers who experienced direct punishment.

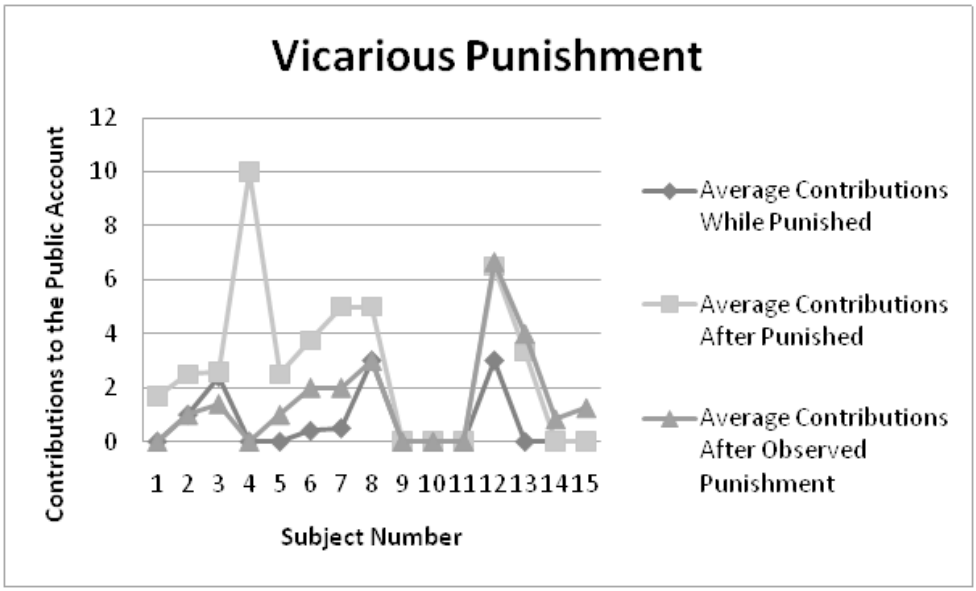


Figure 3.5: Contributions pre-punishment, post-punishment, and post-vicarious punishment (i.e., following observed OGC punishment)

Result 6: Experience with direct punishment increases the effectiveness of the punishment mechanism through observance of punishment.

The two preceding results relied on between-subjects analysis, either comparing first movers and second movers (Result 5) or different subsets of second movers (Result 6). A within subjects comparison offers the general advantage of control over individual differences present in between subject comparisons.[13] In order to more carefully examine if subjects respond to observed punishment similarly to if they had experienced punishment directly, we will first examine responses to direct punishment and then compare how these same subjects respond to observed OGC punishment. The extant literature defines "vicarious" punishment to mean that observed punishment deters undesirable (in this case, non-cooperative) behavior. In other words, direct punishment is typically assumed to have the intended deterrent effect, and so vicarious punishment has similar behavioral effects.

To pursue this within subjects analysis, we must identify the second movers who were

[13]For a given number of subjects, there is increased statistical power in a within-subjects test. The counterbalanced order of our games in the ABC and ACB session helps minimize the concern of ordering effects being the source of our test results.

directly punished at some point during Game C and who also observed OGC punishment in Game B. Out of the 48 second movers, 15 meet this criteria. Keep in mind that token contributions during the same period of direct punishment occur before the punishment is administered, and so we use this as a proxy for the subject's baseline contribution. We find that average token contributions for the period one receives direct punishment was .41, average contributions for the period after one is directly punished was 2.4, and average contributions of these same subjects after observing OGC punishment in Game B was 1.26 tokens. Thus, the average subject response to direct punishment is in the beneficial direction of increased contributions in the next round (t-test, $p < .01$), the response to observed OGC punishment is significant and in the same direction as the direct punishment effect (t-test, $p < .01$), but this "vicarious" punishment effect is not as large in magnitude as the direct punishment effect (t-test, $p = .04$). Figure 3.5 shows these results by subject. This final result, though based on simple comparison of means is in some sense more general than Table 3.2 results in that it includes game B and game C data. Table 3.2 used the more "clean" data from game B (where second movers could not also be directly punished) to highlight the importance of history on punishment spillovers, whereas this last result simply shows that the average punishment spillover effect is in the same beneficial direction as the direct punishment effect (i.e., vicarious punishment exists).

Result 7: Vicarious punishment exists, though the effect is not as strong as direct punishment.

3.6 Discussion

Punishment is typically modeled in a reinforcing manner where the effect of punishment reinforces desired behavior in a simple, predictable direction. We show that how punishment affects others is not so simple. The purpose of this study is to provide insight into the broader effectiveness of punishment mechanisms via spillover effects. We add to the prior research by identifying a pure behavioral punishment spillover effect. To do so, we have employed experimental methods to systematically examine what may influence the direction and magnitude of punishment spillover effects. Our design differences include a main treatment where subjects know with certainty that they will not be punished after

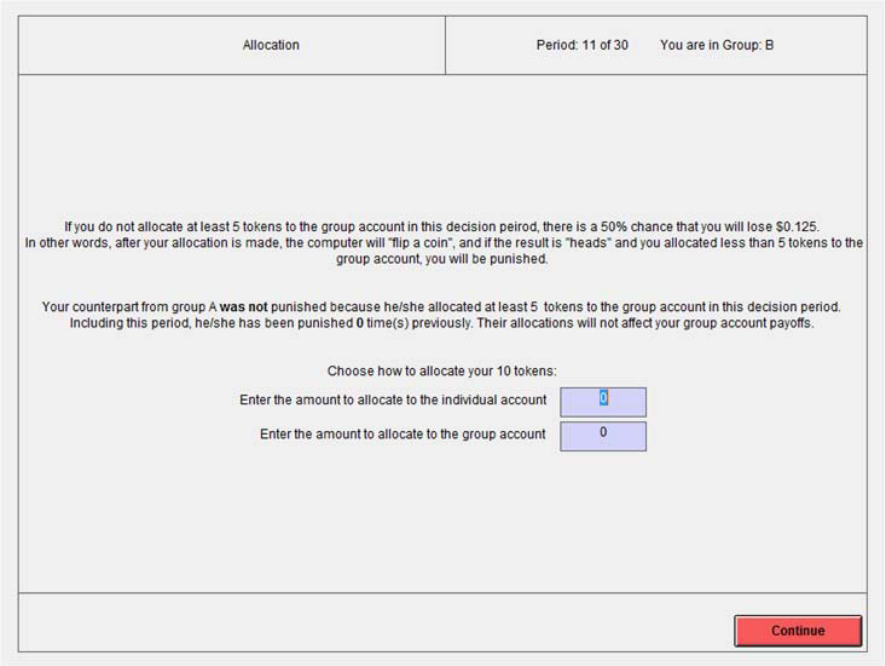
observance of the same and they also know the exact contributions which lead to external punishment. Understanding how direct punishment effects differ from punishment spillover effects is important for punishment mechanism design, as is a better understanding of when punishment has its intended deterrent effect or not. A main result that emerges from our data is that the immediate response (i.e., periods 1-5 of Game B) of subjects who observe punishment is an adverse response of contributing less to the provision of a public good, but this response is surprisingly reversed when the subject has previous experience with direct punishment mechanisms (i.e., Game C experience). Thus, one's previous history with punishment mechanisms appears to play a key role in determining the direction of the punishment spillover effect.

History of behavior with a direct punishment in place is not the same as actually being punished, so we further examined the data to clarify which component of experience seems to drive this result. The first aspect of experience that seems to matter is if a subject had actually been previously punished. The old adage, "walk a mile in my shoes," seems to ring true in this setting as those who had been punished previously themselves contributed more to the public good when they saw someone else was punished than those that did not observe punishment. In addition to experiencing punishment themselves, we found that a history of seeing someone else being punished, but no history of self punishment, led to a more cooperation behavior when faced with the possibility of one's own punishment. This history of observing others punished seemed to make current punishment more salient and resulted in more cooperative behavior than those subjects who only had a history of self punishment. We also found that the less cooperative a subject is initially, the more likely the subject will display vicarious punishment effects later in the game.

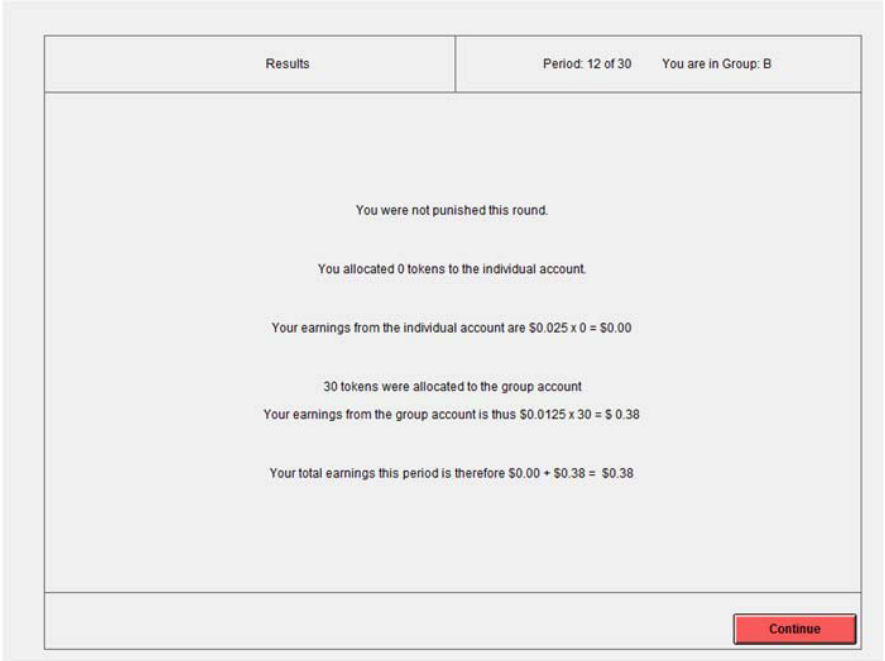
A main theme of this paper is that spillover effects of punishment seem more complicated than the effects of direct punishment. These effects should be taken into consideration going forward in both the design of punishment mechanisms and when organizations are considering implementing punishment as a reinforcing tool. Previous experience is of primary concern in understanding these vicarious punishment effects.

3.7 Appendix

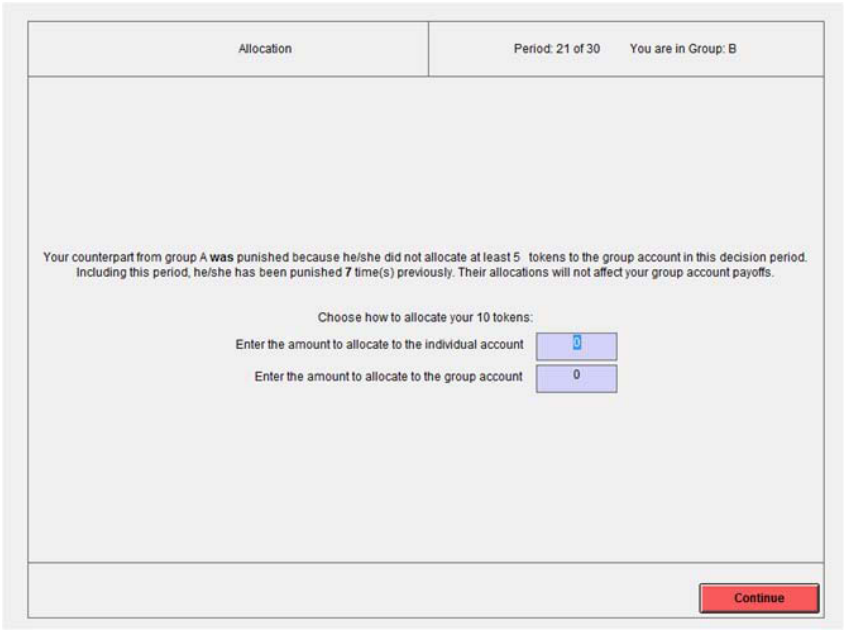
Screenshots and Robustness checks



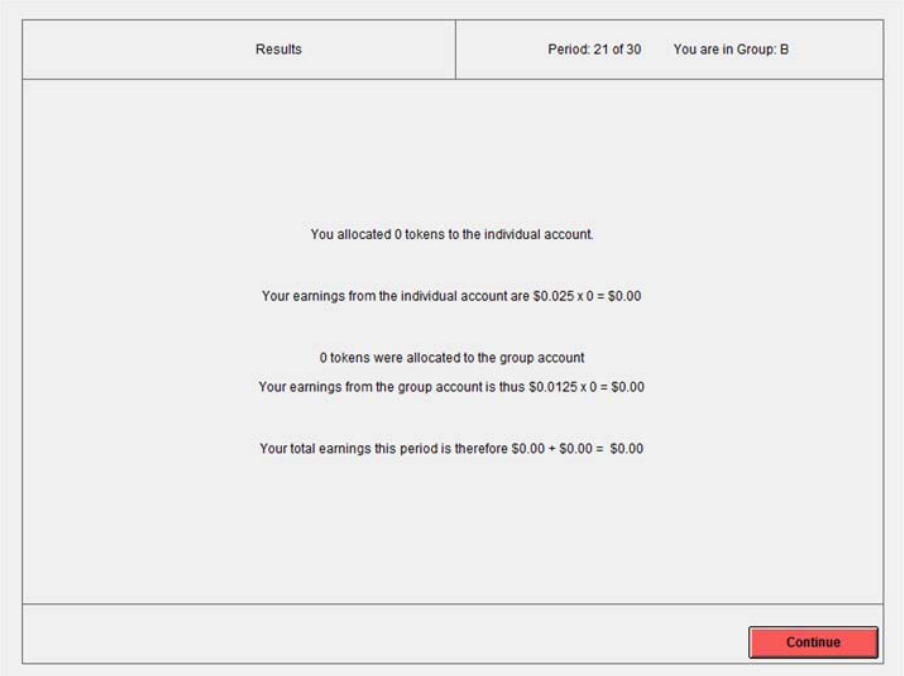
A decision screen of a subject in group 2 in Game C



The results screen of a subject in group 2 for Game C



A decision screen of a subject in group 2 in Game B



The results screen of a subject in group 2 for Game B

Table 3.3: Main regression re-ran to check for robustness of results

Variable	Model 3 (original)	Model 3a (exclude session 4)	Model 3b (include a dummy for session 4)
OGC Punished	−.993* (.602)	−1.275** (.619)	−1.003* (.781)
Period	−.175*** (.040)	−.167*** (.045)	−.175*** (.040)
Game ACB	2.065*** (.610)	1.914*** (.760)	1.958*** (.749)
Chat	−.377 (.485)	−.346 (.645)	−.271 (.630)
1st Period Contributions	.191** (.087)	.186* (.112)	.185** (.099)
Game ACB/OGC Punished	1.464*** (.543)	1.396*** (.528)	1.477*** (.547)
Chat /OGC Punished	−.366 .497()	−.155 (.532)	−.371 (.497)
1st Period Contr/OGC Punished	.008 (.129)	.073 (.119)	−.011 (.130)

Notes: Standard errors are corrected for clustering at the subject level
Three (***), two (**), and one (*) stars indicate statistical significance at 1%, 5%, and 10% respectively
In model3b, the dummy variable for session 4 has a p-value of.726

CHAPTER 4

THE EFFECTS OF SOCIAL DISTANCE ON WORK EFFORT: AN EXPERIMENTAL STUDY

4.1 Introduction

> "The typical worker operates in a setting where efforts are exerted in the hope of a promotion, salary revision, or bonus, which are typically at the discretion of superiors."
>
> Prendergast (1999)

Previous research has shown that in a simple setting, rewards are in fact an effective mechanism to elicit higher work effort from employees. However, this effectiveness is likely altered when the workers who are competing for the rewards vary in their social ties to their manager. To illustrate why social distance may matter, suppose a manager has a reward she must give to one of two employees; one who is socially close to her (or part of the in-group) and one who is not (or part of the out-group). If both employees produce equal outputs, the manager would likely give the reward to the employee who is socially closer to her.[1] More generally, there must be some threshold level of productivity for each worker where the manager is indifferent to giving the reward to the in- or the out-group member where this threshold will be lower for the in-group member than the out-group member. As a result, an out-group member will have to work harder to garner the same rewards as an in-group member. It stands to reason that in this instance, work effort may be much different than when social ties are not considered. The question remains how these workers internalize the advantage or disadvantage. If workers see social ties as a substitute for work

[1] The social ties may make management feel more comfortable with certain workers when trust is needed for things such as implicit contracts (Knack and Keefer, 1997).

effort, the out-group employee will contribute more than the in-group employee. If employees internalize the inequity in a purely strategic manner, the reverse will be true. This paper will use experimental methods to explore this issue focusing on how workers compete, via work effort, for a reward when social distance is present.

Social distance can be a natural part of a corporation where the manager has closer social ties with some of the employees because of their compatibilities, hobbies, similarities outside of work or relationships beyond the work setting. With this added element, the work effort of the employees competing for these work-place rewards may be distorted which could prove costly to the firm. This distortion could arise if an in-group member chooses to work less when group membership is present because they assume their social ties will make up for their lack of work effort in gaining the favor of the manager and in turn securing the reward over the out-group member. In a worst case scenario for the firm, an out-group member could view their lack of group membership as a disadvantage to their prospect of gaining a reward when pitted against an in-group member and will decrease their work effort as well. Since both employees work less, this result is counter to the purpose of the reward and will result in a decline of overall productivity. In addition to the loss in productivity, the firm also loses the cost of the reward given and the cost of lost time by the manager who evaluated the employees' performance. This grim scenario isn't the only plausible outcome as the opposite could also occur. An out-group member could attempt to compensate for their lack of group membership by working harder and an in-group member could respond to this by also contributing more work effort. It remains unclear however which effects will dominate and what the end result will be. It is important to understand how these in- and out-groups affect employee work effort so that workable solutions can be formulated to improve the efficacy of the rewards and in turn, increase the profitability of corporations.

Since the reward structures used in organizations vary and the resulting response of the worker to these reward structures may also vary, rewards mimicking bonuses (can be given to multiple employees) and promotions (can be given to only one employee) will be taken into account. These two types of incentives are widely used as evidenced in the opening quote from Prendergast (1999), and pointed out by Baker, Jensen and Murphy (1988). The Baker et al. study argues that a company will use a bonus system when there are few promotable opportunities. For instance, a CEO is already at the top of the ladder and thus not promotable. They also classify small firms with fewer hierarchical levels and firms in

declining industries as more likely to use bonuses over promotions.

In order to more closely examine the effect social ties have on employee competition, it must first be established that employees believe they are rewarded, at least partially, on social distance from their superior. As a specific example, social distance is a concern for current and potential telecommuters. These employees fear that once they are out of the office, their employers will forget about them when rewards are given out (Kurland and Cooper, 2002). The popular view seems to be in line with the Kurland and Cooper study as evidenced in a *Network World* article which confirms this fear by stating that most executives believe office workers will be promoted before telecommuters.[2]The view that social distance is a determining factor for preferential treatment is held by office-based workers as well as seen by the advice given in a recent Yahoo Hotjobs article titled *How to get a raise*. The author asserts "If you stay cloistered in your cubicle, you'll probably be disappointed when raises are announced–no matter how hard you work."[3]

Academic literature provides strong evidence to back up the accepted view that social distance plays an important role in rewards decisions. Westphal and Stern (2006) show that social ties are important for employees to gain board positions where these social ties are substitutes for ability or background. Their study suggests that an out-group worker must exert more effort to build interpersonal relations in order to receive the preferred position. Kristof-Brown (2000) finds similar results for appointments into management positions. As pointed out in Schulze et al. (2001), this effect is generalizable to employees not in management roles and shows that favoritism results in a decline in the returns on assets and market-to-book ratios. Bandiera et al. (2009) further show that managers in a fruit picking setting show favoritism towards those socially closer to them which generates higher potential payoffs for socially close workers but at a cost to the firm.

This evidence suggests that a reward that is meant to encourage higher work effort may actually have the opposite effect. The primary purpose of the current research proposal is to explore how social distance in the workplace affects employee behavior when they are competing for rewards. This is worth understanding because the work effort of employees directly affects a company's bottom line. Experimental methods will be used to explore this research agenda. Using an experimental methodology is useful for the examination of

[2]http://www.networkworld.com/news/2007/011707-telecommute-career.html
[3]http://hotjobs.yahoo.com/career-articles-how_to_get_a_raise-1164

these issues because of the inability in many field settings to observe work effort (even if productivity is observed), the employee's utility functions or the value of the reward to the workers.

The main result of the paper shows that females are much more sensitive to in- and out-group classifications than males where out-group females contribute much less effort to their manager than their in-group counterparts. Surprisingly, in-group males contribute the same amount of effort when they are competing for the reward with an out-group member as when they are competing with a fellow in-group member. The rest of the paper is organized as follows: Section 2 goes into further detail on the current literature, section 3 highlights a theoretical model which generates testable predictions for the experiment, section 4 goes over the experimental design, section 5 presents the results and section 6 concludes with a discussion of the results.

4.2 Literature

There are three main lines of literature which are directly related to this study. The first deals with social distance. Psychologists have devoted decades to the study of social or group identity. Social identity is derived in some way from identifying with a group. This identification will reduce the social distance among the group members.[4] In economics, Chen and Li (2009) look at a series of games to determine the role group identity plays in how subjects viewed others in allocation decisions. Groups are formed based on painting preferences by the subjects or by random assignment. They also use treatments to enhance social identity in the group and have the subjects play 24 two-person games to separate out the effects of group identity. Their results show a very strong in-group bias for items such as charity, envy, punishment and social concerns.

Brandts and Sola (2010) introduce social distance into a trust game (Berg, Dickhaut and McCabe, 1995). In their game, employers (Player A) pass the tokens to the employees (Players B and C) who then simultaneously decide how many to pass back. The payoff for player A is the amount they keep plus the sum of tokens passed back to them while the payoff for players B and C is the amount they decided to keep. In their setup, player A gives 10 tokens to Player B and Player C in either 6-4 or 4-6 ratios. These amounts are then multiplied by an efficiency factor of M_B or M_C. The efficiency factor is varied to mimic

[4] Refer to Chen and Li (2009) for a great review of this line of literature

worker ability heterogeneity. They have three treatments for efficiency. The first is the standard where $M_B = M_C = 3$.The second is that $M_B = 2.5$ and $M_C = 3.75$. The third is $M_B = 3.5$ and $M_C = 2.25$. They add social distance by recruiting friends where players A and B know each other personally and player C knows they know each other but does not know players A or B. They use six treatments, three with the different efficiency factors and no social distance and three with the different efficiency factors with social distance.

Their results indicate that A players give more to their friends and player B's reciprocate by giving more back regardless of the efficiency factor used. Player C's don't react negatively to this discrimination by giving less back dependent on the efficiency factor used. They claim that "The result suggest that personal relations could indeed be efficiency-enhancing in situations where agents' decisions are not purely distributive but also productive."

A similar study was conducted by Fiedler, Haruvy and Li (2010). Their study included a virtual world where game play occurs through a computer program called Second Life. In this experiment subjects were allowed to chat, via the program, with one person and then chose if they wanted to be paired with this person for a trust game or someone whom they didn't chat with. The person they didn't chat with had a higher multiplier and thus had the potential to earn more money overall. They find that subjects prefer to be matched with someone they chatted with (in-group) and they sent them more money and received more in return. This reduction of social distance has a profound effect even when the social ties are weaker than in the Brandts and Sola (2010) paper.

Though the current study was built around understanding how social distance affects work effort in the workplace, it is similar to previous studies of "unfair" tournaments (O'Keeffe, Viscusi and Zeckhouser (1984); Bull, Schotter and Weigelt (1987); Schotter and Weigelt (1992); Harbring and Irlenbusch (2003); Orrison, Schotter and Weigelt (2004)) and provides a nice compliment to many of these studies. One of the major focuses of previous studies of unfair tournaments was to understand how discrimination affected work effort. Experimentally, unfair tournaments were used where an "advantage" was induced for some workers since it is difficult to test things such as racial or gender discrimination in the lab and even more so in the field. Since the focus of the present study is to look at social distance between the worker and the manager, a real person will decide the reward allocations and thus the discrimination will be endogenously determined. This addition will allow the present research to have broader implications for understanding discriminatory behavior

since it provides a nice avenue to determine if/how participants internalize the previously induced discrimination. In this manner, the prior studies on advantaged and disadvantaged workers provide a test bed for this study.

The pioneering work of Lazear and Rosen (1981) demonstrated that if workers are risk-neutral, a reward scheme awarding workers based on relative work effort is equivalent to a properly designed piece-rate scheme. Bull, Schotter and Weigelt (1987) experimentally test this using a design where a tournament pay scheme is parameterized to yield the same equilibrium effort levels as a piece rate pay scheme. Part of the parameterization involves adding noise onto the worker's effort choice. In their design, two workers were competing for a prize, where effort was costly and the reward was M or m where $M > m$. The reward M was automatically given to the person with the highest observed effort level while m was given to the person with the lowest. They find that on average the predictions of Lazear and Rosen hold, but the variance of work effort is much higher in the tournament pay scheme than the piece-rate one. They also introduce asymmetry in the workers by having different cost structures where one worker has higher costs of contributing effort than another worker. The "advantaged" worker contributed about the level of effort predicted, but surprisingly they found that the "disadvantaged" worker contributed much higher levels of effort than predicted.

To further study the results in the asymmetric case, Schotter and Weigelt (1992) conduct another experiment where costs were again asymmetric for some treatments and include an additive constant in other treatments to signify how much a worker is being favored in the tournament. Thus, in this case, a player wins the big prize M if their observed effort level plus the constant is greater than the observed effort level of the other worker in the tournament, or if $y_i + k > y_j$, and win the little prize m otherwise. In this setup, a positive k represents that worker i is favored. Their main interest lies in seeing how different institutions similar to affirmative action or equal opportunity laws affect work effort. They find that both cost disadvantages and favoritism disadvantages leads to lower work effort by the disadvantaged. Their main takeaway is that affirmative action increases overall work effort.

The third line of literature focuses on how competitive outcomes differ by gender (Gneezy, Niederle and Rustichini, 2003; Niederle and Vesterlund, 2007, 2010; Sutter and Rützler, 2010; Wozniak, Harbaugh and Mayr, 2010; Gupta, Poulsen and Villeval, 2011). The basic finding of the Gneezy, Niederle and Rustichini study was that as the competitiveness of

the environment increases, males increase their effort more than females in a real effort experiment. Niederle and Versterlund (2007) further show that men select into competitive pay schemes twice as often as women. Sutter and Rützler find that this competitive drive of males is present as early as three years old.

The current paper will compliment the current literature in several important avenues. First, there is no concrete use of post-effort rewards with the presence of varying social distance. This paper does so by using two treatments meant to mimic a bonus or a promotion. Of the papers that do study post-effort rewards, non-induced advantages or disadvantages are rarely used. Because of this, experimental contests are usually ran in non-mediated environments. The research question necessitates the use of mediated contests and will build our understanding of behavior in this setting.

4.3 Model

In this section, a simple model of employee competition will be highlighted where some employees are socially closer to the manager than others. Predictions derived from this framework will be tested using experiments. The questions previously addressed in the literature take on two basic forms. The first looks at how employees respond after they are given an incentive. In these settings, social distance has been shown to evoke more reciprocal behavior (Chen and Li, 2009) which either benefits the firm (Brandts and Sola, 2010) or harms the firm (Bandiera et al. 2009). In these papers studying how group identity affects behavior, it is assumed that the manager makes the first move or is the bottleneck and the employee must respond. Though these studies look at social distance, they do not examine how social distance affects workers competing for the prize.

The second form looks to see how an employee reacts to an incentive in the future. In this strand of the literature (Bull, Schotter and Weigelt 1987, Schotter and Weigelt 1992, Andreoni et al. 2003, Brandts and Cooper 2006 etc.), the aspect of social distance is not explored though these studies do examine heterogeneity of various forms typically by inducing varying cost structures.

The seminal study of Lazear and Rosen (1981) provide one of the earliest studies of promotion tournaments. Orrison, Schotter and Weigelt (2004) typify these studies using advantaged and disadvantaged workers primarily testing the theory of O'Keeffe, Viscusi and Zeckhauser (1984). In this line of research, the main tool used is altering observed vs. chosen

effort where observed effort will rarely be equal to chosen effort because of a random noise term added to chosen effort. The winner(s) of the tournament is automatically chosen to be the worker(s) with the highest observed work effort who receives M while the loser(s) receive m where $M > m > 0$. In their study, they look at how having advantaged workers affects work effort of the advantaged and the disadvantaged. The advantaged worker is differentiated by adding a positive constant to their chosen work effort.

Cason, Masters and Sheremeta (2010) experimentally examine the entry decisions of workers in a winner-take-all tournament contest and a proportional contest. In their setting, workers are performing a real effort task where subjects were asked to add sets of numbers and awards were automatically awarded. In the winner take all contest, the worker with the highest set of correctly added numbers won the entire prize. In the proportional contest, the subjects were awarded a proportion of a fixed prize based on their effort relative to everyone else's effort in their group

Based on the reward structure used in corporations, the following model will consider how social distance affects work effort when the reward is similar to a promotion or when it is similar to a bonus. Promotions can be thought of as winner take all (WTA) contests while bonuses can be thought of as Proportional contests. The models rely on two primary assumptions; (1) how the worker internalizes the advantage or disadvantage and (2) their beliefs about what the person they are competing with will do.

4.3.1 Risk-Neutral Workers

In this simple setup, we will assume that social distance is fixed and agents are risk neutral. When agents are risk-neutral, there will be no predicted difference in effort between the two contests so the following will apply to both contests. This is because a risk neutral worker views a proportion of the prize the same as the expected value of the prize. This assumption will be relaxed in the next section. It will also be assumed that a worker is endowed with an initial amount of time and can divide their effort between a productive task and an outside option. Thus, a worker's payoff is a function of their value of the prize and how much time they devote to their outside option. In this simple setup, it will be assumed there are only two workers competing for the prize; one who is socially closer to the manager (an in-group employee) while the other is not (an out-group employee). With the addition of social distance, a manager will discount the work effort of the out-group worker by a factor

of $\delta[0,1)$ or the manager views $e_i = \delta e_o$ where e_i represents the effort of the in-group worker and e_o the effort of the out-group worker. With a non-linear cost function, the workers have expected payoffs of:

$$(1) \qquad \pi_i = I + \pi(e_i, \delta e_o)M - \tfrac{e_i^2}{c}$$
$$\pi_o = I + [1 - \pi(e_i, \delta e_o)]M - \tfrac{e_o^2}{c}$$

Where I is total effort available, M is the value of the prize, c is a constant cost parameter and $\pi(\cdot)$ is the probability the worker will win this prize. Notice that $\pi(\cdot)$ is determined by the manager. When the manager is making their decision, the interpretation of observed effort will likely be "noisy." To account for this, a random variable will be added to the productivity of each employee. This will be the productivity that a manager will use when making their decisions. This falls in line with the explanation of a random measurement error brought up by O'Keeffe, Viscusi, and Zeckhauser (1984). The main point of the error is that a manager may not distinguish between workers when their efforts are fairly close together. We will further make a simplifying assumption that the noise is a random iid term that is uniformly distributed with mean zero around $[-a, a]$. Thus, player i wins the prize M if their effort plus the noise term is greater than the discounted effort plus the noise term of the outside worker, or $e_i + \varepsilon_i > \delta e_o + \varepsilon_o$ or, $\pi(e_i, \delta e_o)$ is equal to $\Pr(\varepsilon_o - \varepsilon_i > e_i - \delta e_o) = \Pr(Z < e_i - \delta e_o) = F(e_i - \delta e_o)$ where $Z = \varepsilon_o - \varepsilon_i$. Under these conditions, the probability a worker wins the prize is reduced to finding the density of the sum of the two random variables (ε_i *and* ε_o). This is carried out in the Appendix, and we find that $\pi(e_i, \delta e_o) = \frac{1}{2} + \frac{e_i - \delta e_o}{2a} - \frac{(e_i - \delta e_o)^2}{8a^2}$.

In the above, notice that the distribution of Z determines the structure of $F(Z)$. Given these assumptions, a PSNE exists. Note that there is no PSNE if there are no random shocks. This is because without the random term, an increase in effort will lead to an increase in the probability of winning the prize with certainty. If this were the case, the in-group worker will always be able to best the out-group worker and any effort the out-group worker chooses, the in-group worker will choose effort slightly above this. Through iterative reasoning, this would eventually lead the out-group worker to contribute nothing and the in-group worker to contribute slightly above that amount. But if this were the case, then the out-group worker would contribute slightly more than the in-group worker and the process continues cyclically. Thus, the random component guarantees that if a worker increases their effort

above equilibrium effort levels, they will not win the prize with 100% certainty. If a worker is using a strict money maximizing strategy, they will maximize the above expected payoffs and set marginal benefits equal to marginal costs, or:

$$(2) \qquad \begin{aligned} \frac{\partial \pi}{\partial e_i} M &= \frac{2e_i}{c} \\ -\frac{\partial \pi}{\partial e_o} M &= \frac{2e_o}{c} \end{aligned}$$

In the Appendix it is shown that $e_i\delta = e_o$ which results in the equilibrium effort levels:

$$(3) \qquad e_i^* = \frac{2aMc}{8a^2+Mc(1-\delta^2)}$$

For the in-group worker and

$$(4) \qquad e_o^* = \delta * \frac{2aMc}{8a^2+Mc(1-\delta^2)}$$

for the out-group worker.

From (3) and (4), we see that if the employees are risk-neutral, the in-group worker will contribute more effort than the out-group worker.[5] Notice that the two efforts are equal except one is discounted by δ. This leads to the first proposition.

Proposition 1 *The in-group employee will contribute more than the out-group employee and the out-group employee will contribute less than the in-group employee.*

What is also apparent from (3) and (4) is that both workers will contribute more as δ increases or as social distance decreases. This is easy to see from the derivatives of equilibrium effort levels w.r.t. δ; $\frac{de_i^*}{d\delta} = \frac{4a\delta(Mc)^2}{(8a^2+Mc(1-\delta^2))^2} > 0$ and $\frac{de_o^*}{d\delta} = \frac{(8a^2+Mc(1-\delta^2))2aMc+4a\delta(Mc)^2}{(8a^2+Mc(1-\delta^2))^2} > 0$.This leads to the second proposition:

Proposition 2 *Overall work effort will decline as social distance increases.*

For comparison purposes, it is useful to point out that if social distance did not exist ($\delta = 1$), the equilibrium effort levels reduce to:

$$(5) \qquad e_k^* = \frac{Mc}{4a}$$

[5] The above analysis follows closely the results obtained by Bull et al. (1987)

This result holds if both workers are from the in-group or both workers are from the out-group. This gives proposition 3.

Proposition 3 *Work effort will be highest when both workers share the same social connections with the manager.*

4.3.2 Risk Averse Workers

With risk neutral workers, there will be no difference in the predictions about work effort between a WTA contest and a proportional contest. It is commonly understood though that risk neutrality is often too strict an assumption and will be relaxed in this section. In this setting, that means that the workers must account for their own risk aversion and that of their co-workers which has been shown to be not analytically solvable for the more realistic heterogeneous risk parameters (see Skaperdas and Gan, 1995; Cornes and Hartley, 2003). For the purposes of this exposition, it will suffice to show the comparative statics with homogeneous constant relative risk aversion (CRRA) when one type of contest is compared to the other. While this will give the comparative static prediction indicating how individuals respond to different levels of risk aversion, heterogeneous risk preferences will eventually be econometrically controlled for. With this in mind, we will assume a standard CRRA utility function of the form $u(y) = y^{\alpha}$ where y is wealth and $\alpha \in (0, \infty)$ is the risk aversion parameter where $\alpha < 1$ represents a risk-averse person. For the proportional contest, the utility for the in-group worker is now:

$$(7) \qquad U_i(e_i, e_o; \delta) = \pi(e_i, \delta e_o)(I + M - \tfrac{e_i^2}{c})^{\alpha} + [1 - \pi(e_i, \delta e_o)](I - \tfrac{e_i^2}{c})^{\alpha}$$

For the winner take all contest, the in-group worker has utility:

$$(8) \qquad U_i(e_i, e_o; \delta) = (I + \pi(e_i, \delta e_o)M - \tfrac{e_i^2}{c})^{\alpha}$$

In the above, and in the experimental design, $I \geq \frac{e_k^2}{c}$ for all feasible e_k. To understand how effort will vary in the two contests, we must find the optimal effort in each reward scenario and compare efforts from the two. It will be sufficient to show the comparison of the in-group worker as the result of the out-group worker has the same comparative statics. The optimal effort in (8) is the same as (3), or:

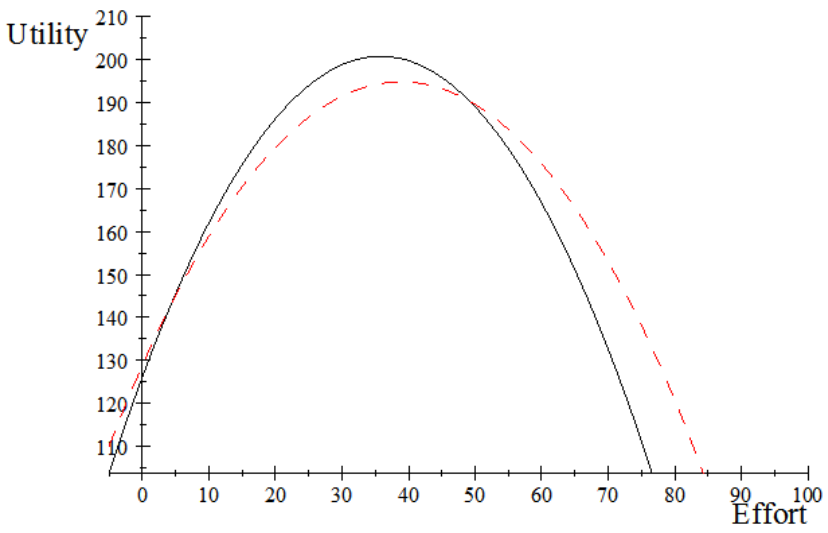


Figure 4.1: Comparative statics of optimal effort with risk-aversion

$$e_i^{WTA} = \frac{\partial \pi}{\partial e_i} * \frac{MC}{2}$$

The optimal effort in (7) is somewhat more complex:

$$\frac{\partial \pi}{\partial e_i}(I + M - \frac{e_i^2}{c})^{\alpha} + \frac{2e_i\pi\alpha}{c}(I + M - \frac{e_i^2}{c})^{\alpha-1} + \frac{2e_i\alpha}{c}(I - \frac{e_i^2}{c})^{\alpha-1}$$
$$= \frac{\partial \pi}{\partial e_i}(I - \frac{e_i^2}{c})^{\alpha} + \frac{2e_i\pi\alpha}{c}(I - \frac{e_i^2}{c})^{\alpha-1} = 0$$

The simplest examination of the comparative statics is done through fixing the parameter values and graphing the utilities as seen in Figure 4.1. This figure shows how the utility functions change as effort increases. The solid line represents utility in the proportional contest while the dashed line represents the utility in the WTA contest. From this graph, notice that optimal work effort in the WTA is greater than that in the proportional contest.

Proposition 4 *With the addition of risk-aversion, work effort in the WTA contest is greater than work effort in the proportional contest.*

4.4 Experimental Design

Since the main point of the paper is to determine if subjects act as theory predicts when social distance is present, subjects played three main games which are meant to separate out other confounds. In order to test the above predictions, 108 subjects were recruited to one of 9 sessions at The Ohio State University using ORSEE (Greiner 2004). The experiment lasted about an hour where the subjects made an average of $22.32. Out of the 108 subjects; 72 were assigned to play the role of an employee, 42 of whom were assigned to the proportional contest.

Once in the lab, subjects were asked to fill out a consent form. After all consent forms were collected, instructions were handed out for the first part.[6] The subjects were not told how many parts were in the experiment and only knew that they were recruited for 1 $\frac{1}{2}$ hours. In the first part, subjects were asked to choose which painting they preferred out five pairs that were shown at the front of the room. The paintings were done by either Paul Klee or Wassily Kandinsky.[7]This methodology has been used to generate a salient group identity in psychology for some time (Tajfel, Billig, and Claude, 1971) and more recently in Economics (Chen and Li 2009). Subjects were next divided into two groups, where the first group (group A) was comprised of 2/3 of them who preferred Klee more often than the other 1/3 while the rest were in the second group (group B).[8] The experimental participants next had the opportunity to meet and talk with others in their group where everyone in group A met at the front of the room and everyone in group B met in the back. They were given topics to discuss and were encouraged to get to know the others in their group. This aspect of the design was used to strengthen the group identity and more closely mimics the environment of interest.[9] After 10 minutes, everyone was called back to their seats to begin the paying portion of the experiment.

Within group A, 1/2 of the subjects were assigned the manager role, while the other 1/2 were in the role of an employee; everyone in group B was an employee.[10] Subjects then

[6]Instructions are available in the appendix.

[7]The paintings by Klee were Ancient Sound, Abstract on Black; Red Balloon; Carnival in the Mountains; Paukenspieler; Angelus Novus and the ones by Kandinsky were Transverse Line; Fugue; Yellow-Red-Blue; Black Spot I; Orange.

[8]Ties were broken randomly

[9]Buchan, Johnson and Croson (2006) found this kind of "personal communication" increased other regarding preferences.

[10]Neutral wording was used in the experiment where the managers were told they were a type A player

played the following three games in groups of 3; one manager and two employees where the matchings were randomly made and at no point did any subject know specifically who they were matched with. In all the games, each subject's group affiliation was common knowledge. Before each game, instructions were handed out and read out loud and screenshots were shown at the front of the room to ensure that every subject knew what each type would see so that all information was common knowledge. The experiment was conducted using z-Tree (Fischbacher, 2007). The final game is meant to mimic employee competition for a prize when social distance is present while the first two are needed to identify confounding effects and test the propositions.

To test the above predictions, three main pieces of information will be needed which will be provided by the following games. In all three games, the payoff to the employees will be equal to the amount of the prize given to them by the manager plus their initial endowment minus their cost of effort or:

$$m + I - \frac{e_{i,o}^2}{c}$$

Where $m \in [0, M]$ is the amount given to the employee by the manager. Though not the interest of the paper, the payoff to the manager will just be the amount both employees contribute to them multiplied by 2 to mimic a productive activity. Because of the complexity of the employee payoffs, subjects in this role were given an on-screen calculator which told them specific payoffs for their decisions before they made their decisions final. The below games will be played in two main treatments; where the prize, M, is non-divisible (winner take all contest), or when it is divisible (proportional contest). In all three games, the matchings of employees and managers was randomly made at the start of each period. The data is used from 4 periods of the game, where there is one period of Game 1 & 2 and two periods of Game 3.[11] The parameter values chosen for the experiment are represented to the subjects as tokens and are $I = 100$, $M = 300$ and $c = 100$. The subjects were paid on an exchange rate of 1 token = \$0.01.

and the employees a type B.

[11] There were another two periods of game three which were played but the software reported incorrect values for these periods so the data won't be used. It was verified that the two periods of data used are not different.

4.4.1 Game 1

In this first game, the employees and managers moved simultaneously and were in a group comprised of one manager (who is always from group A), one employee from group A and one employee from group B. Notice when the matchings are made in this manner, the employee from group A is from the same group as the manager who together form the in-group while the person from group B forms the out-group. Both the employees and the manager started out with an endowment of money. The endowment that the manager had can be viewed as the amount of the prize they must give and the amount the employee starts with can be seen as how much effort they can potentially give to productive activities. The employees must choose how much of their endowment to keep and how much to give to the manager who will either be in their group or not. In order to mimic a productive activity, their contributions to the manager will be doubled. The manager's decision was to decide how much of their endowment to give to someone in their own group and how much to give to someone in the other group with no option to keep any for herself. Neither the employees nor the manager were able to observe the allocation of the person(s) they were matched with when they made their allocation decisions. This will give an estimate of in-group bias (δ in the model) which will be used in game 3. Because of the simultaneous nature of the moves of the manager and the employees, there will be no competition amongst employees for the prize in this game.

4.4.2 Game 2

Propositions 2 and 3 rely on knowing how employees compete for the reward in the absence of social distance. Game 2 was designed to address these hypotheses. This game was similar to Game 1 except the moves were sequential where employees moved first and after seeing the employees' decisions, the manager decided their allocation choices. To make certain social distance is the same for the employees, both were from the same group. So, both employees were either from the group A or group B. The theory does not distinguish between which group the employees belong to as long as they both belong to the same group. This game will serve as a baseline for competition when social distance is the same for both employees.

4.4.3 Game 3

This game is similar to Game 2 except the employees were from different groups and were shown the in-group bias of the manager which was determined from the manager's decision in Game 1. Propositions 1 and 2 rely on employees knowing the bias thus it is important to make sure employees knew the advantage or disadvantage they face. This game mimics employees competing for a reward when social distance is present. This game was repeated twice where the matchings were randomized each time.

4.4.4 Additional Controls

In order to get an idea of what the employees beliefs were about what the other employee contributed, a belief-elicitation mechanism was implemented after the second iteration of game 3. Subjects did not know this task was coming when they played game 3 and was only played by employees. Subjects were paid using a quadratic scoring rule (Palfrey and Wang, 2009) based on how close their guess was to the actual contributions of the other employee matched to the same manager as them. In addition to the above games, Proposition 4 requires that a subject's risk preferences be controlled for. To accomplish this, a traditional Holt-Laury risk assessment test (Holt and Laury 2002) was given to the subjects.

4.5 Results

Though not the focus of the current study, the behavior of the manager will be examined first. It is important to analyze this for two basic reasons. First, since mediated contests are rarely used to study competition, a manager's decision in this setting is not well known. More importantly though, it is pertinent to examine managerial decisions since testing the above propositions relies on understanding if managers show a bias to in-group workers and if so, the size of this bias. The summary statistics for managers are given in Table 4.1. There are a couple things that stand out. First, on average, managers give 18 percentage points more to in-group workers than out-group workers in Game 1. This advantage represents δ in the above equations. Notice also that even though managers receive only 53% of their income from in-group workers in Game 3, they give them 62% of the total prize. Controlling for the extra 6% given them by the in-group workers, the managers decision in Game 3 is consistent with the advantage seen in Game 1 of 18 percentage points. This means that the

advantage given to the employees in Game 3 wasn't completely earned.

Table 4.1: Managers' Decisions and Payoffs

	% Given by Manager				
	In-Group	Out-Group	Statistic test (for Difference)	Sequential Move	% of Manager Income (from In-Group)
Game 1	59%	41%	$t-test, Wilcoxon$ $p<.01$, $p<.01$	No	52%
Game 2	50%	50%	N/A	Yes	51%
Game 3	62%	38%	$t-test, Wilcoxon$ $p<.01$, $p<.01$	Yes	53%
Note: By design, in Game 2 the manager had to give exactly 50% to in-group and 50% to out-group.					

Now that the manager's in-group bias has been established, we will turn our attention to employee behavior and Proposition 1. Proposition 1 states in-group workers (IGW) will contribute more than out-group workers (OGW) and OGW will contribute less than IGW. Table 4.2 gives the summary statistics for employee contributions while Table 4.3 gives results of regressions of Game 3.[12]

We can see in table 4.2 that the difference between in- and out-group employees is not found to be statistically significant. What becomes immediately evident from looking at Game 3 results is that contributions are much different for males than females. In this study, 45 of the subjects assigned to play the role of an employee were male. Previous studies have shown gender is important in competitive outcomes (e.g. Gneezy, Niederle and Rustichini, 2003; Niederle and Vesterlund, 2007, 2010; Sutter and Rützler, 2010; Wozniak, Harbaugh and Mayr, 2010; Gupta, Poulsen and Villeval, 2011). The basic finding of these studies that women compete less than men is also true of the current study as males contributed 69% of their endowment to the manager while females contributed 50% (differences are confirmed statistically; t-test, $p < .01$, Wicoxon, $p < .01$). To control for gender, Table 4.3 presents regression analysis of Game 3. The dependent variable is the number of tokens out of 100 each employee gave to their manager. Model 1 includes dummy variables for In-Group and Female. It is shown that once gender is controlled for, in-group workers contributed more

[12]For simplicity, Table 3 only presents the results of a standard OLS regression. Note that because of the use of only two periods of data, not much is gained from using panel data methods such as a random effects model or clustering. Nonetheless, for robustness an additional table is included in the Appendix where all of the Models are re-ran with these features.

than out-group workers. Looking back at Table 2 though, we can see this is a specification error and that the results are likely driven by the female out-group worker.

Additionally, Table 4.2 highlights that contributions by females vary based on group membership while there is no variation for males. Because of this, the predictions of Proposition 1 will be evaluated separately for males and for females.

Result 1: In-group Females contributed more tokens to their manager than out-group females.

Table 4.2: Employees' Decisions

	% Given by Employee to Manager			
	In-Group	Out-Group	Statistic test (for Difference)	Sequential Move
Game 1	35%	32%	$t-test, Wilcoxon$ p=.59, p=.81	No
Game 2	65%	63%	$t-test, Wilcoxon$ p=.79, p=.94	Yes
Game 3	66%	58%	$t-test, Wilcoxon$ p=.19, p=.20	Yes
Game 3 WTA	74%	63%	$t-test, Wilcoxon$ p=.33, p=.24	Yes
Game 3 Proportional	61%	54%	$t-test, Wilcoxon$ p=.38, p=.48	Yes
Game 3 Male	70%	69%	$t-test, Wilcoxon$ p=.84, p=.57	Yes
Game 3 Female	61%	36%	$t-test, Wilcoxon$ p=.01, p<.01	Yes

Table 4.2 shows that the difference in contributions of in-group females is confirmed higher than out-group females (61% vs. 36%). Again, a more careful analysis controlling for other relevant variables is in order. Model 2 adds an interaction between In-Group and Female to Model 1 while Model 3 adds the dummy variable for WTA. From the most complete Model 3, it is evident that female IGW contribute much more than female OGW.

Result 2: In-group males did not contribute significantly more tokens to their manager than out-group males.

From Table 4.2 it is shown that there is no statistical difference between the contributions of male IGW and male OGW. Once again, a more careful analysis is carried out in Table

4.3 and results using t-tests and Wilcoxon tests will be taken in concert with the regression results in Table 4.3. The effect of being a male IGW is captured in the variable for "In-Group" in Models 2 and 3. It is shown in these two models that being in the in-group is neither economically important for males nor statistically significant in any of the 3 Models.

Table 4.3: Regression analysis of the amount the employee gives to the manager

Variable	Model 1	Model 2	Model 3	Model 4
In-Group	10.22** (4.68)	1.58 (5.81)	0.90 (5.68)	0.46 (5.65)
Female	−20.69*** (4.83)	−32.79*** (6.74)	−34.39*** (6.74)	−33.42*** (6.72)
Female*In-Group		23.16** (9.51)	25.12*** (9.31)	27.39*** (9.35)
WTA			12.77*** (4.56)	12.87*** (4.70)
Holt-Laury				−1.94* (1.04)
Advantage from the Manager				−0.01 (0.04)
Constant	64.72*** (3.67)	68.75*** (3.97)	63.96*** (4.24)	77.14*** (9.76)

Notes: Standard errors are corrected for clustering at the subject level
Three (***), two (**), and one (*) star indicate statistical significance
at the 1%, 5%, and 10% respectively

The Nash equilibria found in the preceding section requires players are best responding based on correct beliefs about what the other player will do. To this end, the results from the belief elicitation task can be utilized to get a better idea of the underlying causes driving behavior. After decisions were made in the final round of Game 3, employees were asked to make an incentivized guess as to how many tokens they thought the person they were paired with in that period contributed. Combined with the allocation decisions, we will be able to tell if subjects are contributing more than, less than or equal to what they think their counterparts contributed.

Result 3: In-group males, out-group males and in-group females contributed significantly more than what they believed a co-worker contributed while it could not be confirmed that out-group females contributed less than what they thought their in-group counterparts contributed.

In the final period of Game 3, in-group females contributed 68 tokens to the manager and they thought their out-group counterpart contributed 44. This difference is confirmed statistically (t-test, p = .03; Wilcoxon, p = .07). Males, regardless of group, also contributed more than they believed their co-worker did. In-group males contributed 69 tokens while they thought their counterparts contributed 49 (t-test, p = .05; Wilcoxon, p = .02) while male OGW contributed 73 and thought the IGW contributed 45 (t-test, p = .05; Wilcoxon, p = .02). In the same period, out-group females contributed 34 while they thought their in-group counterpart contributed 44, though this difference is not confirmed statistically (t-test, p = .26; Wilcoxon, p = .31).

We will now explore the predictions of Proposition 2. Proposition 2 states that as this advantage increases, overall work effort will decline. To test this, Model 4 in Table 4.3 includes a continuous variable, "Advantage from the Manager", which accounts for how many of the 300 tokens the manager gave to someone from their own group in Game 1.

Result 4: Employee contributions to the manager do not vary with the strength of the advantage (disadvantage) given to the in-group worker (out-group worker).

The variable measuring the advantage from the manager varies anywhere from 0 to 300 with the average being 178, a standard error of 10.5 and a 95% confidence interval ranging from 156 to 199. With this much variation, any effect coming from an increase in the advantage (or disadvantage) should be able to be identified in the econometric model. As seen in Model 4, the coefficient for this variable fails to reach statistical significance.[13]

Result 5: There is no difference in employee contributions to the manager when employees are competing for the reward with someone from their own group or with someone from the other group.

Table 4.4 compares the amount given by employees in Game 2 and in Game 3. This table shows that there is only one instance, OGW, where the difference between contributions in Game 2 and Game 3 is shown to be statistically significant, and this is only found using the Wilcoxon test but the result does not hold when using a t-test.

[13]It can be argued that subjects view the advantage (or disadvantage) more broadly, or with some noise. For instance, it could be the case that they view 160 and 170 similarly. To test this other methods of classifying this "advantage" were used where the variable was categorized into broader categories and none were found to affect contributions.

Table 4.4: Employees' Decisions in Game 2 compared to Game 3

	% Given by Employee		
	Game 2	Game 3	Statistic test (for Difference)
Total	64%	62%	$t-test, Wilcoxon$ p=.59, p=.35
In-Group	64%	66%	$t-test, Wilcoxon$ p=.68, p=.51
Out-Group	63%	58%	$t-test, Wilcoxon$ p=.17, p=.04
Male	70%	69%	$t-test, Wilcoxon$ p=.88, p=.75
Female	53%	50%	$t-test, Wilcoxon$ p=.49, p=.23

This leads to the final untested Proposition. Proposition 4 states that because of risk aversion, work effort is greater in the WTA contest than the Proportional contest.

Result 6: The contributions in the WTA contest are much greater than those in the Proportional contest.

We can see from Model 3 that the sign of the coefficient for "WTA" is both positive and significant meaning contributions from the employees to the manager is greater in the WTA contest than in the Proportional contest. For completeness, model 4 includes an additional variable, "HL", which captures the results of a Holt-Laury risk-aversion test. It is evident from Model 4 that the WTA variable is still positive and highly significant.[14]

4.6 Discussion

The aim of this paper was to determine if people view the in-group (or out-group) bias between themselves and their manager as theory predicts and if not, what effect this will have on work effort when the employees are competing for a reward given out by the manager. It was found that females behave as the theory predicts, while males do not. More specifically, females who were in the in-group contributed significantly more effort than females in the out-group while there was no difference in the contributions of males. In fact, there is no

[14] When breaking the data down by gender, males are found to contribute more in the WTA contest than in the Proportional contest (t-test, p = .05, Wilcoxon, p = .02) while there is no difference for females (t-test, p = .53, Wilcoxon, p = .70).

statistical difference in the contributions of female in-group employees and male employees (t-test, p = .24; Wilcoxon, p = .13). There are several implications from these findings.

First, these results may give additional insight into the gender pay gap. A large unaccounted for differential still exists which is currently attributed to discrimination (Blau and Kahn, 2000). If females view themselves as part of the out-group and workers are paid, at least partially, based on their perceived effort, we should expect female workers to make less than male workers if the results of the experiment hold. This can by no means be the entire explanation of the difference in the gender pay gap, but may serve to explain some of the unexplained differences.

Second, there is a large body of literature which show that females contribute less in competitive environments than males (e.g. Gneezy, Niederle and Rustichini, 2003; Niederle and Vesterlund, 2007, 2010; Sutter and Rützler, 2010; Wozniak, Harbaugh and Mayr, 2010; Gupta, Poulsen and Villeval, 2011). The findings of the current study show that if employees are included in the managements group, this effect may go away, or be lessened to a large extent for females. This should be an encouraging finding for the recent interest in group identity (Chen and Li 2009) and should increase the importance of such works as the Eckel and Grossman (2005) study which carefully dissects how to create team identity to reduce social distance.

4.7 Appendix A

4.7.1 Proof of e_i^* (3) and e_o^* (4)

With a non-linear cost function, the workers have expected payoffs of:

$$(1) \qquad \pi_i = I + \pi(e_i, \delta e_o)M - \tfrac{e_i^2}{c}$$
$$\pi_o = I + [1 - \pi(e_i, \delta e_o)]M - \tfrac{e_o^2}{c}$$

Where I is total effort available, M is the value of the prize, c is a constant cost parameter and $\pi(\cdot)$ is the probability the worker will win this prize. If a worker is using a strict money maximizing strategy, they will maximize the above expected payoffs and set marginal benefits equal to marginal costs, or:

$$(2) \qquad \tfrac{\partial \pi}{\partial e_i} M = \tfrac{2e_i}{c}$$
$$-\tfrac{\partial \pi}{\partial e_o} M = \tfrac{2e_o}{c}$$

Actual decisional work effort will take the form:

$$y_k = e_k + \varepsilon_k$$

For $k = i, 0$ and where ε_k is a random iid shock that is uniformly distributed with mean zero around $[-a, a]$. Thus, the player wins the prize M if $y_i > y_o$ which means that the probability an agent wins, $\pi(e_i, e_j)$ is equal to $\Pr(\varepsilon_o - \varepsilon_i > e_i - \delta e_o) = \Pr(Z < e_i - \delta e_o) = F(e_i - \delta e_o)$ where $Z = \varepsilon_o - \varepsilon_i$. To solve for the PSNE, we must figure out the probability a worker wins the prize which requires finding the density of the sum of the two random variables. Notice that the pdf of Z is just the triangular distribution ranging from $-2a$ to $2a$ with a mean at 0 and a maximum at $\frac{1}{2a}$. First, notice that

$$(3) \qquad f_{\varepsilon_i}(\varepsilon_i) = f_{\varepsilon_o}(\varepsilon_o) = \begin{cases} \frac{1}{2a} & if -a \leq \varepsilon \leq a \\ 0 & otherwise \end{cases}$$

And the density function for the sum is

$$(4) \qquad f_z(z) = \int_{-\infty}^{\infty} f_{\varepsilon_o}(z + \varepsilon_i) f_{\varepsilon_i}(\varepsilon_i) d\varepsilon_i$$

Because of (3), (4) becomes:

$$(4) \qquad f_z(z) = \tfrac{1}{2a} \int_{-a}^{a} f_{\varepsilon_o}(z + \varepsilon_i) d\varepsilon_i$$

Note that the integrand is 0 unless $-a \leq z + \varepsilon_i \leq a$ $(or\ if\ z - a \leq \varepsilon_i \leq z)$. This sets up two different densities for z. The first is when $-2a \leq z \leq 0$. In this case,

$$f_z(z) = \tfrac{1}{2a} \int_{-a}^{z} d\varepsilon_i = \tfrac{1}{2a}(F(z) - F(-a))$$

where $\int_{-a}^{z} d\varepsilon_i$ is the CDF of the uniform which is $\frac{x+a}{2a}$ in this scenario. Thus, the problem is reduced to

$$\tfrac{1}{2a}(\tfrac{z+a}{2a} - \tfrac{-a+a}{2a}) = \tfrac{1}{2a} + \tfrac{z}{4a^2}$$

Similarly, when $0 \leq z \leq 2a$,

$$f_z(z) = \tfrac{1}{2a} - \tfrac{z}{4a^2}$$

Notice, the convolution of two uniform distribution functions is just the triangular distribution with known CDF:

$$F(z) = \begin{cases} 0, & if \quad z \leq -2a \\ \frac{1}{2} + \frac{z}{2a} + \frac{z^2}{8a^2}, & if \quad -2a \leq z \leq 0 \\ \frac{1}{2} + \frac{z}{2a} - \frac{z^2}{8a^2}, & if \quad 0 \leq z \leq -2a \\ 1, & if \quad z \geq 2a \end{cases}$$

From this, the PSNE can be outlined. First, note that $\frac{\partial \pi}{\partial e_i} = \frac{\partial F(e_i - \delta e_o)}{\partial e_i}$ and $\frac{\partial \pi}{\partial e_o} = \frac{\partial F(e_i - \delta e_o)}{\partial e_o} = -\frac{\partial F(e_i - \delta e_o)\delta}{\partial e_i}$. Combining this with (2), we get that $e_i = \frac{e_0}{\delta}$. Using this, we know that in equilibrium $0 \leq z \leq 2a$ which means that $\frac{\partial \pi}{\partial e_i} = \frac{1}{2a} - \frac{e_i - \delta e_o}{4a^2} = \frac{1}{2a} - \frac{e_i - \delta^2 e_i}{4a^2}$ which, combined with (2) leads to the optimal effort levels.

$$e_i^* = \frac{2aMc}{8a^2 + Mc(1-\delta^2)}$$

For the in-group worker and

$$e_o^* = \delta * \frac{2aMc}{8a^2 + Mc(1-\delta^2)}$$

for the out-group worker.

4.8 Appendix B: Robustness checks

Table 4.5: Random effects model with clustering at the subject level.

Variable	Model 1	Model 2	Model 3	Model 4
In-Group	10.22* (6.)	1.58 (7.78)	0.90 (7.40)	0.46 (7.48)
Female	−20.69*** (6.21)	−32.79*** (8.60)	−34.39*** (8.02)	−33.39*** (8.45)
Female*In-Group		23.16* (12.04)	25.12** (11.54)	27.35** (11.74)
WTA			12.77** (5.85)	13.06** (5.70)
Holt-Laury				−1.94* (1.17)
Advantage from the Manager				−0.02 (0.02)
Constant	64.72*** (4.41)	68.75*** (4.70)	63.96*** (5.32)	78.10*** (10.82)

Notes: Standard errors are corrected for clustering at the subject level
Three (***), two (**), and one (*) star indicate statistical significance
at the 1%, 5%, and 10% respectively

CHAPTER 5

CONCLUDING REMARKS

The goal of this dissertation was to develop a deeper understanding of how three specific non-monetary factors affect the work effort of employees. The classical assumption that employees care only about monetary incentives has only recently been abandoned. The essays in this dissertation provide further evidence that this abandonment is justified in some cases.

Chapter 2 provides evidence that the productivity of males may change more than that of females based on the structure of the environment. More specifically, males are more productive in a structured environment when performing mundane tasks and are more productive in an unstructured environment when performing creative tasks. The productivity of females was found to be unchanged by the environment. This leads to the conclusion that if the work to be performed is primarily creative in nature or will be done by females, a manager should not be worried about a loss in productivity when the work is to be performed outside the office. One should be careful though in how far this conclusion can be taken. Many office environments are more complex than the simple experiment used. An avenue of future research is to examine how the structure of the environment may affect work effort when employees are in teams. In many settings where telecommuting is a possibility, employees perform tasks in a team where the total effort is a sum of all the individual efforts. There are both benefits and additional complications that arise when the team members are working remotely. The most widely cited benefit is the ability of firms to attract higher quality applicants because of the lack of geographic constraints. A closer inspection of how to overcome the limitations is needed though to provide a more complete picture of how the environment may affect productivity.

Chapter 3 uncovered another non-incentivized behavioral pattern resulting from how

people react to observing punishment of someone else. The conclusion reached was that experience in a punishment mechanism matters in implementing more successful deterrent to unwanted behavior in a work-team environment. This result also raises more questions for future research. More specifically, can the same result be obtained from examining how others respond to observing someone else being rewarded? Additionally, it would be useful to determine if the behavior in the work-team setting transfers to other games based on the similarity of these games. These additional questions need to be answered to generate a satisfactory answer to how behavior is altered from observing a reinforcing action.

Chapter 4 explored one final non-pecuniary motive believed to alter employee work effort. As in chapter 2, gender was again found to be the determining factor, though this time it was females who had the diverging results. It was found that in a setting where social distance is present between an employee and a manager, the socially closer female contributes much more effort than the socially distant female while there was no difference in male behavior based on social distance between a worker and a manager. This study serves as a useful introduction to the topic, though there were many simplifying assumptions that can be relaxed to get a better idea of the complete effect social distance in the workplace has. The first probably avenue of future research will examine how varying transparency conditions of the reward affects worker effort. In many office settings, the amount of the reward that the workers are competing for is uncertain either because the worker does not have full information from the manager or the worker has some uncertainty of the value of the reward to themselves and/or their co-worker. Chapter 4 only considers the case when workers are all equally productive. This serves as a nice baseline, but another realistic assumption that can be added to this setting is when workers vary in their ability. This added element could be especially important to understand how the social distance interacts with worker ability in a promotion tournament since the goal of these tournaments is to promote the most able workers (Gibbons and Waldman, 1999).

APPENDIX

Experimental Instructions - Telecommuting

Email to subjects:

Thank you for signing up for today's social science experiment. This experiment is internet based and thus you will be able to login to participate in the experiment from any location with an internet capable computer. Note: you will not come to the lab at any specified time but will complete this experiment outside the lab where experiments are typically held. If you do not wish to participate in this experiment please reply to this email and we will remove you from the experiment. You will have 24 hours to complete the experiment. The experiment software will accept logins from 4:00 pm on 4-21-2009 (today) to 4:00 pm on 4-22-2009 (tomorrow). To begin the experiment, you must go to http://xsfs.fsu.edu/experiment_t and login with the username and password provided in this email. Once logged in, you will be presented with a consent form which allows you to consent to participate in the experiment or refuse. After you have given your consent, you can click on the link for the pre-experiment questionnaire which is a 5 question questionnaire. After you complete the questionnaire, you will then be presented with the instructions for part 1 of the experiment. After reading these instructions, you can participate in part 1 which consists of five 5-minute rounds. Following the completion of part 1, you will be presented with the instructions for part 2 and participate in part 2. Part 2 consists of five 3-minute rounds. Upon completion of part 2, a 10 question questionnaire will close out the experiment. Note: you do not have to complete the entire experiment all at once. Once you log on to the website and start a round, only that round must be completed. If you are logged off because of inactivity, you can go to the link again and log in at a later time to finish any other rounds if you wish. Because of the structure of the tasks in today's session, your payments will be available within one week of completion of the experiment. We will email you when your payment is ready to pick up and then you will come by Bellamy 039, the office next to the usual lab

used for conducting experiments, to receive your payment.

Username: xxxxxxx

Password: xxxxxxx

Instructions for Part 1

This is an experiment on the economics of decision making. You will have the chance to earn money, in addition to your participation fee, based on your decisions in this experiment. This is an individual experiment and you should complete all sections on your own. You should be able to complete all sections without any external assistance of any kind.

In this part of the experiment, you will be able to choose between two tasks. You can split your time among the tasks however you choose. Meaning you can spend all of your time on task I and none on task II, all of your time on task II and none on task I, or some combination of task I and task II or neither. Both of these tasks are available by clicking on the tab labeled "Task I" or "Task II" in each of the first five (5) rounds. Each round in this experiment is 5 minutes long. Once you finish a round, the next round will not begin until you click on the link to begin it.

Once you begin Part 1, there will be two tabs; one for task I and one for task II along with a timer to let you know how long you have remaining in the round. For task I, a string of random characters is displayed and your task is to type the string of characters, exactly as they are displayed, in the box provided and hit the enter key (or click the submit button). This string consists of letters and numbers. Note, the letters are case sensitive and in order to get credit for a correctly typed string, you must match the string exactly as it appears including upper and lower case letters. Every time you hit the enter key (or click the submit button), a different random string will appear and you can go through the task again. In each round of the experiment, the default payment for each string typed correctly is $.02. A running total of how many you have typed correctly and your total earnings will be displayed beside the timer. An example for how the payoffs will be determined is given below.

To choose task II, click on the tab labeled "task II". In task II, you will be playing a game against the computer. The game is commonly known as tic-tac-toe. There are nine spaces in which to either put an X or an O. The X represents player one's choice, while the O represents player two's choice. Player one and player two take turns putting either an X

or an O in a non-occupied space. The three outcomes of the game are win, lose or draw. The game is won when one of the players has three X's or O's in a row, lost when the other player has three X's or O's in a row and is a draw when neither player does. The X's and O's can be in a row vertically, horizontally or diagonally. For this game you will be player one and the computer will be player two. Each time you win you will get paid $.01. A running total of the results of the game are displayed to the right.

Let's go through an example. In a round, you chose either task I or task II for five minutes. Let's say you correctly typed 100 strings of random characters in the five minutes. In addition, lets' say you won 10 games of tic-tac-toe. In this instance, you will get $.02 for each correctly typed entry, or $2.00, and $.01 for each game of tic-tac-toe won, or $.10 , thus your total payment for this round will be $2.10. If you had similar totals for all rounds, your cumulative earnings for part 1 would be $10.20. You will see your earnings, along with how many strings you attempted to type and how many you typed correctly, updated as you type.

Summary: There are five, 5-minute rounds which you can complete. During the rounds, you can choose task I or task II in any combination. After you complete part 1, you will be allowed to read the instructions for part 2 and complete part 2. You are now ready to begin play. To play, click on the box to acknowledge you understand the instructions and then click on the "next" button.

Instructions for part 2

We remind you that this is an individual experiment and that you should complete all sections on your own. You should be able to complete all sections without any external assistance of any kind.

In this part of the experiment, you will be able to choose between two tasks labeled task I and task II. You can split your time among the tasks however you choose. Meaning you can spend all of your time on task I and none on task II, all of your time on task II and none on task I, or some combination of task I and task II or neither. These tasks are available in each of the last five (5) rounds. Each round in this experiment is 2 minutes long. Once you finish a round, the next round will not begin until you click on the link to begin it.

For task I, a description of an object will appear and your task will be to come up with as many ideas as you can of uses for the object other than the one for which it was

originally designed or intended. You will enter each idea in the space provided and hit the enter key (or "submit" button) to record it. Once you hit the "submit" button, your answer will be recorded and show up below at which point you can type another use. As an example, suppose the object that appeared was "a tin can-container used to store food". The first phrase describes the object and the second phrase describes its intended use. Some unintended uses, those which you would get credit for, for which it was not intended could be a "flower pot", a "holder for my dreams", "attach a string between two cans to make telephones", "cut the can and make a pinwheel" etc. Intended uses, those which you would not get credit for, would involve things like "storing carrots", "container for evaporated milk" etc. Additionally, each use must be unique for Part 2. This means that you cannot use the same use for multiple rounds. If for instance you used "a flower pot" for the first object, you will not get credit for it if you use it for any of the other objects. You will also not get credit for uses that are the same. In the example above, if you had put "a daisy pot" and "a petunia pot" this would get counted as one use since both are flower pots. For this task, you will receive $.05 for each use you come up with in which the object was not intended for and an additional $.05 for each use that no one else in the group participating today comes up with. So, for each use there is the potential to make $.10. There will be approximately 10 other people participating in the experiment on the same day as you. An example for how the payoffs will be determined is given below. The validity and uniqueness of your responses can not be judged immediately and so you will not see immediate feedback on your earnings. Your responses will be judged by two independent parties to determine whether or not they satisfy the requirementsYou will be notified by email when the process is completed and your payoff has been determined.

Task II is the same as in part 1. Remember, the task was tic-tac-toe played against the computer. The payment for winning this game is still $.01.

Let's go through an example. In a round, you chose either task I or task II on each screen for three minutes. Let's say you typed 20 uses of the object which satisfy the above criteria in three minutes and earned $.05 for each or $1.00. Also, you came up with 5 uses which no one else participating in the same time period and earned an additional $.05 for each or $.25. In addition, let's say you won 6 games of tic-tac-toe. In this instance, you will get $.01 for each game of tic-tac-toe won or $.06. Your total payment for this round will be $1.31. If you had similar totals for all rounds, your cumulative earnings for part 2 would be $6.55.

Note that earnings from task I will not show in cumulative earnings.

Summary: There are five, 3-minute rounds which you can complete. After you complete part 2, you will be allowed to complete a brief post-experiment questionnaire and receive instructions on payments. You are now ready to begin. To start Part 2 of the experiment, click on the "next" button.

5.0.1 Experiment Instructions- Punishment Chat Game Order ACB

This is an experiment on the economics of decision making. In addition to your participation fee, you will have the chance to earn money based on your decisions in this experiment. It is extremely important that you put away all materials including external reading material and turn off your cell phones and any other electronic devices. If you have a question, please raise your hand and I or one of my assistants will come by and answer your question privately.

You will be randomly and anonymously assigned to be in either group A or group B. You will remain in this group for the entire experiment. Each person in group A will be matched with another person from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B.

You will have 5 minutes to chat with your counterpart from Group A or B prior to the beginning of the decision experiment. Below we list some suggested questions to give you ideas of things to ask your counterpart. The number on the note card in the upper left corner of your desk will be the same for you and your counterpart from the other group whom you are matched with. Remember, use this time to get to know the other person. At this time you may locate and chat with your counterpart from the opposite group.

1. If you could be any superhero and have super powers, which one would you like to have and why?

2. If you could transport yourself anywhere instantly, where would you go and why?

3. What is one item that you really should throw away, but probably never will?

4. What is your major?

5. What year are you in school?
6. What is your favorite musical act?
7. If they made a movie of your life, what would it be about and which actor would you want to play you?
8. What's your favorite cartoon character, and why?
9. What's the ideal dream job for you?
10. What thought or message would you want to put in a fortune cookie?
11. If you had to give up a favorite food, which would be the most difficult to give up?
12. What is one food you'd never want to taste again?
13. If you won a lottery ticket and had a million dollars, what would you do with it?
14. You've been given access to a time machine. Where and when would you travel to?
15. Mount Rushmore honors four U.S. presidents: Washington, Jefferson, Lincoln, and Roosevelt. If you could add any person to Mount Rushmore, who would you add and why?
16. In your opinion, which animal is the best (or most beautiful) and why?
17. Growing up, what were your favorite toys to play with as a child?

Today's experiment will last for 30 periods which are divided into 3 parts of 10 periods. The following instructions are for periods 1 – 10. Prior to the start of period 11, additional instructions will be given.

At the start of each decision period, you will face a decision and will be matched with two people from your group (so, if you are in group A, you are matched with two other group A individuals from the decision task, even though you will still have an assigned counterpart in group B). You will be randomly re-matched with a different pair of people for each decision. You will never be told who you are matched with in your group.

In each period, you will be given 10 tokens. Your task is to decide how many tokens to allocate to a group account and how many to allocate to an individual account. You

can allocate anywhere from 0 to 10 tokens to each account, but the total allocated to both must sum to 10. (Negative allocations or fractional allocations are not allowed). Each token allocated to the individual account will generate a $0.025 payoff to you and you alone. Each token allocated to the group account, however, will generate $.0125 to you and $.0125 to each of the other two members of your group. Similarly, when other members of your group allocate their tokens into their individual accounts, it generates a payoff to that person and no one else. But, for each token another member of your group allocates to the group account, this generates $.0125 for each member of the group, including you. So, your total earnings in each period are equal to $.025 times the number of tokens you allocate to your individual account plus $.0125 times the total number of tokens you and the other members of your group place in the group account.

Let's go through some examples. Suppose you allocated 5 tokens to the individual account, 5 tokens to the group account and the total in the group account was 12 tokens (implying that the other two members of your group allocated a total of 7 tokens to the group account). In this example, your payoff would be $.025 times the 5 tokens you allocated to your individual account plus $.0125 times the 12 total tokens in the group account, for a total payoff of $.025*5 + $.0125*12 = $.28

If, on the other hand, you allocated 10 tokens to the individual account, 0 tokens to the group account and the total in the group account was 12 tokens, your payoff would be $.025 times the 10 tokens you allocated to your individual account plus $.0125 cents times the 12 tokens in the group account, for a total payoff of $.025*10 + $.0125*12 = $.40

As a final example, suppose you allocated 0 tokens to your individual account and the total tokens in the group account is 25. In this case, your earnings would be just the $.0125 times the 25 tokens in the group account (since you allocated no tokens to your individual account), for a payoff of $.0125*25 = $.32.

The members of group A will make their decisions first and then the members of group B will make their decisions. After all of the members of group B have made their decisions (i.e., allocations), a screen will be displayed showing the results of the period and your payoff for that period. No one else will see this results screen or how much your earnings are for the period.

Summary: You will need to decide how many of your 10 tokens to allocate to your group and individual accounts in each decision period. Your total payoffs in each period are equal

to $.025 times the number of tokens you allocate to your individual account plus $.0125 times the total number of tokens you and the other group members allocate to the group account. The members of this group will be re-randomized after every decision period.

These instructions are for periods 11-20. Additional instructions will be given prior to the start of period 21.

The task is similar in this part except both members of group A and group B have a 50% chance of being punished $.125 if they do not allocate at least 5 tokens to the group account. For instance, if you allocated 6 tokens to the individual account and 4 tokens to the group account, there is a 50% chance you will lose $0.125. This chance can be thought of like flipping a coin. If you allocated less than 5 tokens and the computer flips a coin and it is heads, you lose $.125, if it is tails, then nothing is subtracted from your payoff. On the other hand, if you allocate 5 or more tokens to the group account, there is no possibility of being punished. (note: whether punished or not, you would still earn $.025 for each token in you individual account plus $.0125 times the total number of tokens in the group account. Punishment, if it occurs, would simply subtract $.125 from your payoff for that decision period).

Again, at the start of each period, you will be matched with two people from your group for the task. In addition to this, each person in group A will be matched with the same person from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B. You keep the same counterpart from the other group for decision periods 11-20. Each decision period will be the same in that group A subjects will make their allocations first. Following group A allocations, group B will be shown if their counterpart in group A was punished or not (but not what their counterpart's exact allocation choice was) and how many times they have been punished. After all of the members of group B have made their decisions, a results screen will be displayed showing the results of the decision period. This is repeated for 10 periods. The only thing that is changed for this part is that members of both group A and group B face the possibility of getting punished if placing less than 5 tokens in the group account and subjects in group B will observe if their group A counterpart was punished or not and how many times they have been punished.

These instructions are for periods 21-30.

The task and payoffs in this set of decision periods is similar to before except that if you are in group A there is a 50% chance you will be punished $.125 if you do not allocate at

least 5 tokens to the group account. As a reminder, if you are in group A and you allocated 6 tokens to the individual account and 4 tokens to the group account, there is a 50% chance you will lose \$0.125. This chance can be thought of like flipping a coin. If you allocated less than 5 tokens and the computer flips a coin and it is heads, you lose \$.125, if it is tails, then nothing is subtracted from your payoff. On the other hand, if you allocate 5 or more tokens to the group account, there is no possibility of being punished. (note: whether punished or not, you would still earn \$.025 for each token in you individual account plus \$.0125 times the total number of tokens in the group account. Punishment, if it occurs, would simply subtract \$.125 from your payoff for that decision period). If you are in group B, you will not be punished, no matter what your allocation of tokens is.

Again, at the start of each period, you will be randomly matched with two people from your group for the decision task. In addition to this, each person in group A will be matched with someone from group B as his/her "other-group counterpart". So, if you are in group A, you will have a counterpart in group B. You keep the same counterpart from the other group that you had previously for decision periods 21-30. Each decision period will be the same in that group A subjects will make their allocations first. Following group A allocations, group B will be shown if their counterpart in group A was punished or not (but not what their counterpart's exact allocation choice was) and how many times they have been punished. After seeing this information, group B subjects will make their allocation decisions. After all of the members of group B have made their decisions, a results screen will be displayed showing the results of the decision period. This will be repeated for 10 periods. The only thing that is different in these periods compared to periods 11-20 is that members of group A face the possibility of getting punished if placing less than 5 tokens in the group account and their counterparts from group B will observe if their group A counterpart was punished or not and how many times they have been punished before they make their own allocation decisions (and group B subjects will not face any possibility of being punished, regardless of their allocation decision).

5.0.2 Instructions-Competition with Social Distance-Proportional Contest

This is an experiment on the economics of decision making. In addition to your participation fee, you will have the chance to earn money based on your decisions and

the decisions of others. It is extremely important that you put away all materials including external reading material and turn off your cell phones and any other electronic devices. If you have a question, please raise your hand and I will come by and answer it privately.

In today's first task you will be shown five pairs of paintings and for each pair you will be asked to indicate which you like better. Each set of paintings will be shown on the projection screen at the front of the room and in each pair one will be labeled A with the other B. On your computer screen, you will choose either A or B, where your choice corresponds to the painting you like the best. There is no right or wrong answer here, so choose which one you like best.

[To be handed out after everyone has made their selections]

The paintings previously shown were either done by two different painters named Paul Klee and Wassily Kandinsky. You will now be divided into two groups where Group 1 comprises two-thirds of you who chose Klee more often than the other one-third (Note: ties are broken randomly). Thus, you will be grouped with others whose preferences for the paintings match your own. So, if you are in Group 1, you will be matched with others who prefer paintings by Klee over Kandinsky more often than those in Group 2 and if you are in Group 2 you are grouped with others who prefer Kandinsky over Klee more often than those in Group 1. On your computer screen, you will notice that it shows which group you belong to. You will remain in the same group for the duration of the experiment. Thus, if you are now in Group 1, you will always remain in Group 1 and similarly for those of you in Group 2. In a moment, you will get together with others in your group to get to know them. Below are some suggested topics to discuss, but feel free to talk about other things but use this time to get to know others in your group. You will have 10 minutes to talk, and after this time, I will call you back to your seats. At this time, if you are in Group 1, please meet with your group members at the front of the room and if you are from Group 2, please meet at the back of the room.

Suggested discussion topics:

1. If you could be any superhero and have super powers, which one would you like to have and why?

2. If you could transport yourself anywhere instantly, where would you go and why?

3. What is one item that you really should throw away, but probably never will?
4. What is your favorite musical act?
5. What's your favorite cartoon character, and why?
6. What's the ideal dream job for you?
7. What thought or message would you want to put in a fortune cookie?
8. If you had to give up a favorite food, which would be the most difficult to give up?
9. What is one food you'd never want to taste again?
10. If you won a lottery ticket and had a million dollars, what would you do with it?
11. You've been given access to a time machine. Where and when would you travel to?
12. In your opinion, which animal is the best (or most beautiful) and why?

[To be handed out after 10 min.]

For the remainder of the experiment, please do not talk with those around you. If you have a question, please raise your hand and I will come answer it. Remember, your group remains fixed for the entire experiment. If you are in Group 1, you and the others in Group 1 prefer Klee paintings more than those in Group 2 and if you are in Group 2 you and the others in Group 2 prefer Kandinsky paintings more than those in Group 1. Your group is also comprised of the same people you just chatted with. Additional to your group assignment, you will also be assigned to either be an A player or a B player. You will remain in this role for the entirety of the experiment. If you look on your computer screen, you will notice which role you have been assigned. If you are in Group 1, your role has been randomly determined by the computer. If you are in Group 2, you are automatically a B player.

There are multiple tasks in today's experiment. The instructions below describe period 1.

If you are a B player, you have been matched with an A player. You will begin with 100 tokens and your task is to decide how to split the 100 tokens with the A player you are matched with. Tokens that you choose to keep will generate 1 cent to you. Each token

you choose to pass to the A player will be worth 2 cents to them and $(x - \frac{x^2}{100})$ cents to you where x is the amount passed to the A player. As an example, if you allocated 50 tokens to the A player and kept 50, the A player you are matched with will receive 100 cents and you will receive $50 * 1 + (50 - \frac{50^2}{100}) = 75$ cents. There is a "Test" button that you can press if you would like to see the outcomes of any potential choices. Once you have made your final allocation decisions, you must hit the "Continue" button. In addition to this payment, you will also receive some amount allocated to you from an A player through a decision they will be making that will be described in a moment. You will not see how much was allocated to you by this person when you make your decision and they will not see how much you allocated to them when they make their decision. The only information you will be shown is if the person you are matched with is in the same group as you or not. Please look at the overhead for an example of the screen that B players will see.

If you are an A player, you have been paired with two B players, one from your own group and one from the other group. Your task is to decide how to allocate 300 cents between the two B players. You may allocate the tokens in any proportion you wish, but the total amount allocated must equal 300 and you do not have the option of keeping any for yourself. Every token you allocate to a B player will not cost you anything. When making your allocation decisions, you will be told which group the B players are from but not what their allocation decisions were. Once you enter the amounts you wish to allocate to the B players, you must hit the "Continue" button to move on. Your payment for this round is based on the decisions of B players, but you will not observe their decisions when you make your allocation choices and they will not observe your decisions when they make their choices. Please look at the overhead for an example of the screen that A players will see.

Summary: Your prior membership in Group 1 or 2 remains from the previous exercise and you have now been assigned to be either an A player or a B player. You will remain in the same roles and groups for the rest of the experiment. If you are a B player, your task is to decide how much of your endowment to allocate to an A player and how much to keep. If you are an A player, your task is to decide how to allocate 300 cents between someone in your group (Group 1) and the other group (Group 2) with no option to keep any for yourself. You will do this only once. Note: all A players are from Group 1 and the B players are split evenly into Group 1 and Group 2. After everyone has made their allocation decisions, the first period will be concluded and we will move on to the next task. Are there any questions?

[To be handed out after period 1]

Remember, you will remain in the same group and role for the entire experiment. If you are in Group 1, you and the others in your group prefer Klee paintings more than those in Group 2 and if you are in Group 2 you and the others in your group prefer Kandinsky paintings more than those in Group 1. Your group is also comprised of the same people you just chatted with.

The following instructions apply for period 2. At the beginning of period 2, the computer will randomly assign two B players to be matched with one A player. The two B players will both be from the same group. Thus, if you are in Group 1, the B player matched to the same A player as you is also from Group 1 and similarly for Group 2. The tasks in this period are similar to those from the last period except the A players will now observe the decisions of the B players before they make their decisions about allocating their endowment between the two B players.

As before, if you are a B player, you must decide how many of your 100 tokens to allocate to the A player you are matched with and how many to keep. There is another B player also matched to the same A player as you who will be simultaneously making the same decision as you about how much of their endowment to allocate to the A player. Tokens that you choose to keep will generate 1 cent to you. Each token you choose to pass to the A player will be worth 2 cents to them and $(x - \frac{x^2}{100})$ cents to you where x is the amount passed to the A player. The only information B players will be given when they make their allocation decisions is if the A player is in the same group as them or not. Please look at the overhead for an example of the screen that B players will see.

After both B players have made their choice the A player will observe the allocation decisions of both B players and which group they are from before they decide how many of the 300 tokens to give to each B player. So unlike in the previous period when the A player was making their allocation decision knowing only the group membership of the B players, the A player will now be dividing up the 300 cents with full knowledge of the decisions made by the B players regarding how much of their endowments the B players each decided to pass to an A player. Please look at the overhead for an example of the screen that A players will see.

Summary: For this part of the experiment, two B players will again be matched with one A player. This time though, both B players will be from the same group and will

make their decisions before the A players. A players will see the allocation decisions of the two B players they are matched with before they make their decisions. After everyone has made their allocation decisions, the second period will be concluded. This task will only be completed once. Again, all A players are from Group 1 and the B players are split evenly into Group 1 and Group 2. After everyone has made their allocation decisions, we will move on to the next task. Are there any questions?

[To be handed out after period 2]

The following instructions apply for periods 3-5. Remember, if you are in Group 1, you and the others in your group prefer Klee paintings more than those in Group 2 and if you are in Group 2 you and the others in your group prefer Kandinsky paintings more than those in Group 1. Your group is also comprised of the same people you chatted with in the beginning. At the beginning of period 3, the computer will again randomly assign two B players to be paired with one A player. The two B players now will be from different groups. Thus, if you are from Group 1, the other B player matched to the same A player as you will be from Group 2. Otherwise the decisions will be made as before. Type B players will make an allocation decision between themselves and the A player with whom they are matched with and then after observing how much the two B players send to them, the A player will choose how to divide up 300 cents. In addition to this, the B players will be able to observe the first period allocation decisions of the A player they are matched with. Specifically, the B players will be able to see how many tokens the A player they are matched with previously allocated to someone in Group 1 and how many they allocated to someone in Group 2. Remember, this is the period where the A players could not observe what the B players did when they made their decisions. After B players make their allocation decisions, A players observe these results before they make their allocation decisions. Please look at the overhead for an example of the screens that A and B players will see.

This will be repeated for 3 periods. After the initial period, B players will observe what the other B player chose in the previous period but not see the decisions of the A players. Please look at the overhead for an example of the screen that B players will see.

The A or B player(s) you are matched with will remain fixed for the 3 periods.

Summary: For this part of the experiment, two B players will again be matched with one A player. This time though, the B players will be from different groups. They will still make their decisions before the A players. In addition to this, the B players will observe

the first period allocation decisions of the A players and they will observe what the other B player contributed the prior period. A players will see the allocation decisions of the two B players they are matched with and which group each B player belongs to before they make their decisions. This is repeated for 3 periods. Again, all A players are from Group 1 and the B players are split evenly into Group 1 and Group 2. Are there any questions?

[To be handed out after period 6]

For period 6, if you are a B player, you are now paired with a different A player. If you are an A player, you are now paired with two different B players. Otherwise, all rules from the previous part are the same.

The following instructions are for period 7. If you are a B player, you and another B player from a different group were matched with an A player last period. Your task is to try and guess how many tokens the other B player contributed the last period. Earnings in this period are tied to how close your guess is. If you correctly guess what they contributed, you will get an additional 300 cents. If you do not guess correctly, your earnings will be determined by the following formula:

$E = 300 - \frac{3}{100} * (Actual\ Contributions - Your\ Guess)^2$

If for instance, the other B player contributed 30 tokens and you guessed 40. This would mean that you would receive $300 - \frac{3}{100} * (30 - 40)^2 = 297$ cents. If you instead guessed 50, you would receive $300 - \frac{3}{100} * (30 - 50)^2 = 288$ cents. Thus, notice that the farther you are away from the actual allocation, the amount you will receive decreases quickly.

If you are an A player, you will do nothing for this period. Are there any questions?

5.0.3 Use of Human Subjects in Research - Approval Memorandum

Office of the Vice President For Research

Human Subjects Committee

Tallahassee, Florida 32306-2742

(850) 644-8673 • FAX (850) 644-4392

APPROVAL MEMORANDUM

Date: 11/20/2008

To: Edwin Dutcher

Address: 2180

Dept.: ECONOMICS

From: Thomas L. Jacobson, Chair

Re: Use of Human Subjects in Research

Does Telecommuting Affect Productivity?

The application that you submitted to this office in regard to the use of human subjects in the proposal referenced above have been reviewed by the Secretary, the Chair, and two members of the Human Subjects Committee. Your project is determined to be Expedited per 45 CFR § 46.110(7) and has been approved by an expedited review process.

The Human Subjects Committee has not evaluated your proposal for scientific merit, except to weigh the risk to the human participants and the aspects of the proposal related to potential risk and benefit. This approval does not replace any departmental or other approvals, which may be required.

If you submitted a proposed consent form with your application, the approved stamped consent form is attached to this approval notice. Only the stamped version of the consent form may be used in recruiting research subjects.

If the project has not been completed by 11/19/2009 you must request a renewal of approval for continuation of the project. As a courtesy, a renewal notice will be sent to you prior to your expiration date; however, it is your responsibility as the Principal Investigator to timely request renewal of your approval from the Committee.

You are advised that any change in protocol for this project must be reviewed and approved by the Committee prior to implementation of the proposed change in the protocol. A protocol change/amendment form is required to be submitted for approval by the Committee. In addition, federal regulations require that the Principal Investigator promptly report, in writing any unanticipated problems or adverse events involving risks to research subjects or others.

By copy of this memorandum, the Chair of your department and/or your major professor is reminded that he/she is responsible for being informed concerning research projects involving human subjects in the department, and should review protocols as often as needed to insure that the project is being conducted in compliance with our institution and with DHHS regulations.

This institution has an Assurance on file with the Office for Human Research Protection. The Assurance Number is IRB00000446.

Cc: Timothy Salmon, Advisor

HSC No. 2008.1895

5.0.4 Use of Human Subjects in Research - Approval Memorandum

Office of the Vice President For Research

Human Subjects Committee

Tallahassee, Florida 32306-2742

(850) 644-8673 • FAX (850) 644-4392

APPROVAL MEMORANDUM (for change in research protocol)

Date: 3/25/2009

To: Edwin Dutcher

Address: 2180

Dept.: ECONOMICS

From: Thomas L. Jacobson, Chair

Re: Use of Human Subjects in Research (Approval for Change in Protocol)

Project entitled: Does Telecommuting Affect Productivity?

The form that you submitted to this office in regard to the requested change/amendment to your research protocol for the above-referenced project has been reviewed and approved.

Please be reminded that if the project has not been completed by 11/19/2009, you must request renewed approval for continuation of the project.

By copy of this memorandum, the chairman of your department and/or your major professor is reminded that he/she is responsible for being informed concerning research projects involving human subjects in the department, and should review protocols as often as needed to insure that the project is being conducted in compliance with our institution and with DHHS regulations.

This institution has an Assurance on file with the Office for Human Research Protection. The Assurance Number is IRB00000446.

Cc: Timothy Salmon, Advisor

HSC No. 2009.2531

5.0.5 Use of Human Subjects in Research - Re-Approval Memorandum

Office of the Vice President For Research

Human Subjects Committee

Tallahassee, Florida 32306-2742

(850) 644-8673 • FAX (850) 644-4392

RE-APPROVAL MEMORANDUM

Date: 10/1/2009

To: Edwin Dutcher

Address: 2180

Dept.: ECONOMICS

From: Thomas L. Jacobson, Chair

Re: Re-approval of Use of Human subjects in Research

Does Telecommuting Affect Productivity?

Your request to continue the research project listed above involving human subjects has been approved by the Human Subjects Committee. If your project has not been completed by 9/30/2010, you are must request renewed approval by the Committee.

If you submitted a proposed consent form with your renewal request, the approved stamped consent form is attached to this re-approval notice. Only the stamped version of the consent form may be used in recruiting of research subjects. You are reminded that any change in protocol for this project must be reviewed and approved by the Committee prior to implementation of the proposed change in the protocol. A protocol change/amendment form is required to be submitted for approval by the Committee. In addition, federal regulations require that the Principal Investigator promptly report in writing, any unanticipated problems or adverse events involving risks to research subjects or others.

By copy of this memorandum, the Chair of your department and/or your major professor are reminded of their responsibility for being informed concerning research projects involving human subjects in their department. They are advised to review the protocols as often as necessary to insure that the project is being conducted in compliance with our institution and with DHHS regulations.

Cc: Timothy Salmon, Advisor

HSC No. 2009.3205

5.0.6 Use of Human Subjects in Research - Approval Memorandum 2

Office of the Vice President For Research

Human Subjects Committee

Tallahassee, Florida 32306-2742

(850) 644-8673 • FAX (850) 644-4392

APPROVAL MEMORANDUM

Date: 4/21/2010

To: Cortney Rodet

Address: 2180

Dept.: ECONOMICS

From: Thomas L. Jacobson, Chair

Re: Use of Human Subjects in Research

Negative Reinforcement Spillovers

The application that you submitted to this office in regard to the use of human subjects in the proposal referenced above have been reviewed by the Secretary, the Chair, and two members of the Human Subjects Committee. Your project is determined to be Expedited per 45 CFR § 46.110(7) and has been approved by an expedited review process.

The Human Subjects Committee has not evaluated your proposal for scientific merit, except to weigh the risk to the human participants and the aspects of the proposal related to potential risk and benefit. This approval does not replace any departmental or other approvals, which may be required.

If you submitted a proposed consent form with your application, the approved stamped consent form is attached to this approval notice. Only the stamped version of the consent form may be used in recruiting research subjects.

If the project has not been completed by 4/19/2011 you must request a renewal of approval for continuation of the project. As a courtesy, a renewal notice will be sent to you prior to your expiration date; however, it is your responsibility as the Principal Investigator to timely request renewal of your approval from the Committee.

You are advised that any change in protocol for this project must be reviewed and approved by the Committee prior to implementation of the proposed change in the protocol. A protocol change/amendment form is required to be submitted for approval by the Committee. In addition, federal regulations require that the Principal Investigator promptly report, in writing any unanticipated problems or adverse events involving risks to research subjects or others.

By copy of this memorandum, the Chair of your department and/or your major professor is reminded that he/she is responsible for being informed concerning research projects involving human subjects in the department, and should review protocols as often as needed to insure that the project is being conducted in compliance with our institution and with DHHS regulations.

This institution has an Assurance on file with the Office for Human Research Protection. The Assurance Number is IRB00000446.

Cc: Bruce Benson, Chair

HSC No. 2009.3730

5.0.7 Use of Human Subjects in Research - Approval Memorandum 3.a

Office of the Vice President For Research

Human Subjects Committee

Tallahassee, Florida 32306-2742

(850) 644-8673 • FAX (850) 644-4392

APPROVAL MEMORANDUM

Date: 11/2/2009

To: Edwin Dutcher

Address: 2180

Dept.: ECONOMICS

From: Thomas L. Jacobson, Chair

Re: Use of Human Subjects in Research

Out of sight, out of mind?

The application that you submitted to this office in regard to the use of human subjects in the proposal referenced above have been reviewed by the Secretary, the Chair, and two

members of the Human Subjects Committee. Your project is determined to be Expedited per 45 CFR § 46.110(7) and has been approved by an expedited review process.

The Human Subjects Committee has not evaluated your proposal for scientific merit, except to weigh the risk to the human participants and the aspects of the proposal related to potential risk and benefit. This approval does not replace any departmental or other approvals, which may be required.

If you submitted a proposed consent form with your application, the approved stamped consent form is attached to this approval notice. Only the stamped version of the consent form may be used in recruiting research subjects.

If the project has not been completed by 11/1/2010 you must request a renewal of approval for continuation of the project. As a courtesy, a renewal notice will be sent to you prior to your expiration date; however, it is your responsibility as the Principal Investigator to timely request renewal of your approval from the Committee.

You are advised that any change in protocol for this project must be reviewed and approved by the Committee prior to implementation of the proposed change in the protocol. A protocol change/amendment form is required to be submitted for approval by the Committee. In addition, federal regulations require that the Principal Investigator promptly report, in writing any unanticipated problems or adverse events involving risks to research subjects or others.

By copy of this memorandum, the Chair of your department and/or your major professor is reminded that he/she is responsible for being informed concerning research projects involving human subjects in the department, and should review protocols as often as needed to insure that the project is being conducted in compliance with our institution and with DHHS regulations.

This institution has an Assurance on file with the Office for Human Research Protection. The Assurance Number is IRB00000446.

Cc: Timothy Salmon, Advisor

HSC No. 2009.3529

5.0.8 Use of Human Subjects in Research - Approval Memorandum 3.b

Office of the Vice President For Research

Human Subjects Committee

Tallahassee, Florida 32306-2742

(850) 644-8673 • FAX (850) 644-4392

RE-APPROVAL MEMORANDUM

Date: 10/22/2010

To: Edwin Dutcher

Address: 2180

Dept.: ECONOMICS

From: Thomas L. Jacobson, Chair

Re: Re-approval of Use of Human subjects in Research

Out of sight, out of mind?

Your request to continue the research project listed above involving human subjects has been approved by the Human Subjects Committee. If your project has not been completed by 10/20/2011, you must request a renewal of approval for continuation of the project. As a courtesy, a renewal notice will be sent to you prior to your expiration date; however, it is your responsibility as the Principal Investigator to timely request renewal of your approval from the committee.

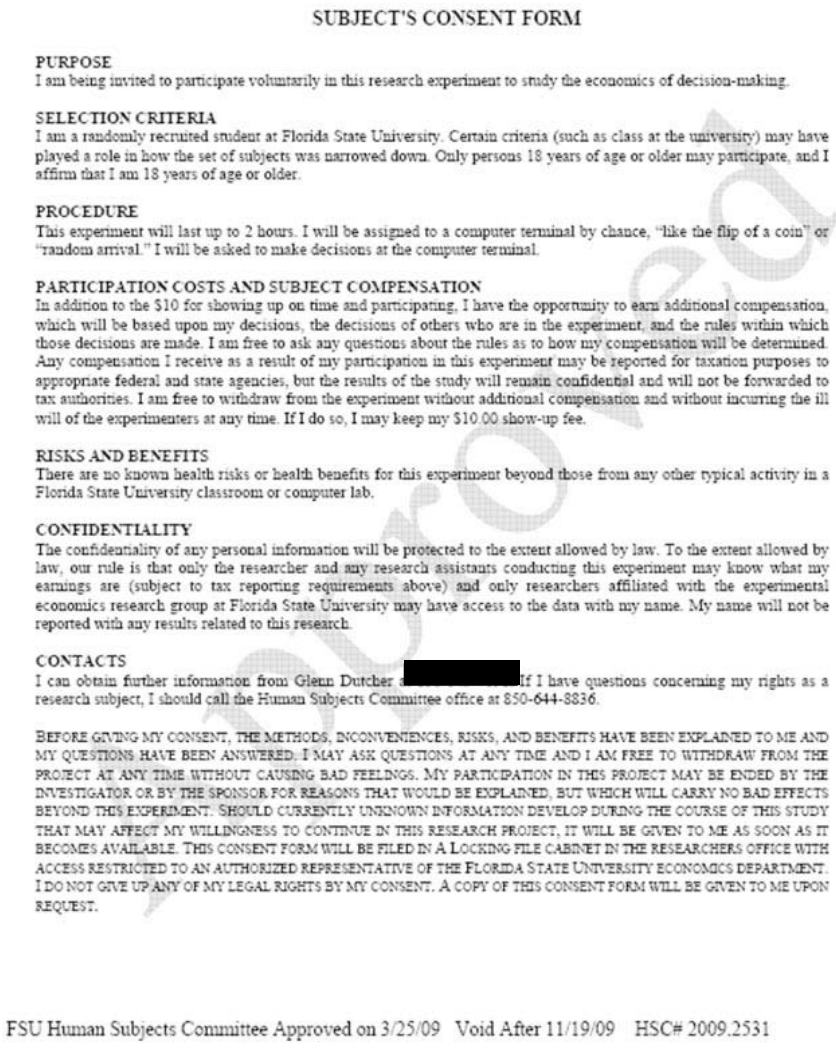
If you submitted a proposed consent form with your renewal request, the approved stamped consent form is attached to this re-approval notice. Only the stamped version of the consent form may be used in recruiting of research subjects. You are reminded that any change in protocol for this project must be reviewed and approved by the Committee prior to implementation of the proposed change in the protocol. A protocol change/amendment form is required to be submitted for approval by the Committee. In addition, federal regulations require that the Principal Investigator promptly report in writing, any unanticipated problems or adverse events involving risks to research subjects or others.

By copy of this memorandum, the Chair of your department and/or your major professor are reminded of their responsibility for being informed concerning research projects involving human subjects in their department. They are advised to review the protocols as often as necessary to insure that the project is being conducted in compliance with our institution and with DHHS regulations.

Cc: Timothy Salmon, Advisor

HSC No. 2010.5177

5.0.9 Subjects Consent Forms



SUBJECT'S CONSENT FORM

PURPOSE
I am being invited to participate voluntarily in this research experiment to study the economics of decision-making.

SELECTION CRITERIA
I am a randomly recruited student at Florida State University. Certain criteria (such as class at the university) may have played a role in how the set of subjects was narrowed down. Only persons 18 years of age or older may participate, and I affirm that I am 18 years of age or older.

PROCEDURE
This experiment will last up to 2 hours. I will be assigned to a computer terminal by chance, "like the flip of a coin" or "random arrival." I will be asked to make decisions at the computer terminal.

PARTICIPATION COSTS AND SUBJECT COMPENSATION
In addition to the $10 for showing up on time and participating, I have the opportunity to earn additional compensation, which will be based upon my decisions, the decisions of others who are in the experiment, and the rules within which those decisions are made. I am free to ask any questions about the rules as to how my compensation will be determined. Any compensation I receive as a result of my participation in this experiment may be reported for taxation purposes to appropriate federal and state agencies, but the results of the study will remain confidential and will not be forwarded to tax authorities. I am free to withdraw from the experiment without additional compensation and without incurring the ill will of the experimenters at any time. If I do so, I may keep my $10.00 show-up fee.

RISKS AND BENEFITS
There are no known health risks or health benefits for this experiment beyond those from any other typical activity in a Florida State University classroom or computer lab.

CONFIDENTIALITY
The confidentiality of any personal information will be protected to the extent allowed by law. To the extent allowed by law, our rule is that only the researcher and any research assistants conducting this experiment may know what my earnings are (subject to tax reporting requirements above) and only researchers affiliated with the experimental economics research group at Florida State University may have access to the data with my name. My name will not be reported with any results related to this research.

CONTACTS
I can obtain further information from Glenn Dutcher [redacted] If I have questions concerning my rights as a research subject, I should call the Human Subjects Committee office at 850-644-8836.

BEFORE GIVING MY CONSENT, THE METHODS, INCONVENIENCES, RISKS, AND BENEFITS HAVE BEEN EXPLAINED TO ME AND MY QUESTIONS HAVE BEEN ANSWERED. I MAY ASK QUESTIONS AT ANY TIME AND I AM FREE TO WITHDRAW FROM THE PROJECT AT ANY TIME WITHOUT CAUSING BAD FEELINGS. MY PARTICIPATION IN THIS PROJECT MAY BE ENDED BY THE INVESTIGATOR OR BY THE SPONSOR FOR REASONS THAT WOULD BE EXPLAINED, BUT WHICH WILL CARRY NO BAD EFFECTS BEYOND THIS EXPERIMENT. SHOULD CURRENTLY UNKNOWN INFORMATION DEVELOP DURING THE COURSE OF THIS STUDY THAT MAY AFFECT MY WILLINGNESS TO CONTINUE IN THIS RESEARCH PROJECT, IT WILL BE GIVEN TO ME AS SOON AS IT BECOMES AVAILABLE. THIS CONSENT FORM WILL BE FILED IN A LOCKING FILE CABINET IN THE RESEARCHERS OFFICE WITH ACCESS RESTRICTED TO AN AUTHORIZED REPRESENTATIVE OF THE FLORIDA STATE UNIVERSITY ECONOMICS DEPARTMENT. I DO NOT GIVE UP ANY OF MY LEGAL RIGHTS BY MY CONSENT. A COPY OF THIS CONSENT FORM WILL BE GIVEN TO ME UPON REQUEST.

FSU Human Subjects Committee Approved on 3/25/09 Void After 11/19/09 HSC# 2009.2531

Figure 5.1: Subject Consent - Telecommuting

SUBJECT'S CONSENT FORM

PURPOSE
I am being invited to participate voluntarily in this research experiment to study the economics of decision-making.

SELECTION CRITERIA
I am a randomly recruited undergraduate student at Florida State University. Only persons 18 years of age or older may participate, and I affirm that I am 18 years of age or older.

PROCEDURE
This experiment will last up to 2 hours. I will be assigned to a computer terminal by chance, "like the flip of a coin" or "random arrival." I will be playing a series of game with other participants.

PARTICIPATION COSTS AND SUBJECT COMPENSATION
In addition to the $10 for showing up on time and participating, I have the opportunity to earn additional compensation, which will be based upon my decisions, the decisions of others who are in the experiment, and the rules within which those decisions are made. I am free to ask any questions about the rules as to how my compensation will be determined. **Any compensation I receive as a result of my participation in this experiment may be reported for taxation purposes to appropriate federal and state agencies, but the results of the study will remain confidential and will not be forwarded to tax authorities.** I am free to withdraw from the experiment without additional compensation and without incurring the ill will of the experimenters at any time. If I do so, I may keep my $10 show-up fee. I understand that I may be asked to leave the experiment, following a verbal warning, if I engage in disruptive behavior such as talking with other subjects during the experiment or using electronic devices such as a cell phone or pager. If I am asked to leave the session, I will be paid all earnings up to the time I leave and will bear no further penalties.

RISKS AND BENEFITS
There are no known health risks or health benefits for this experiment beyond those from any other typical activity in a Florida State University classroom or computer lab.

CONFIDENTIALITY
The confidentiality of any personal information will be protected to the extent allowed by law. To the extent allowed by law, our rule is that only the researcher and any research assistants conducting this experiment may know what my earnings are (subject to tax reporting requirements above) and only researchers affiliated with the experimental economics research group at Florida State University may have access to the data with my name. My name will not be reported with any results related to this research.

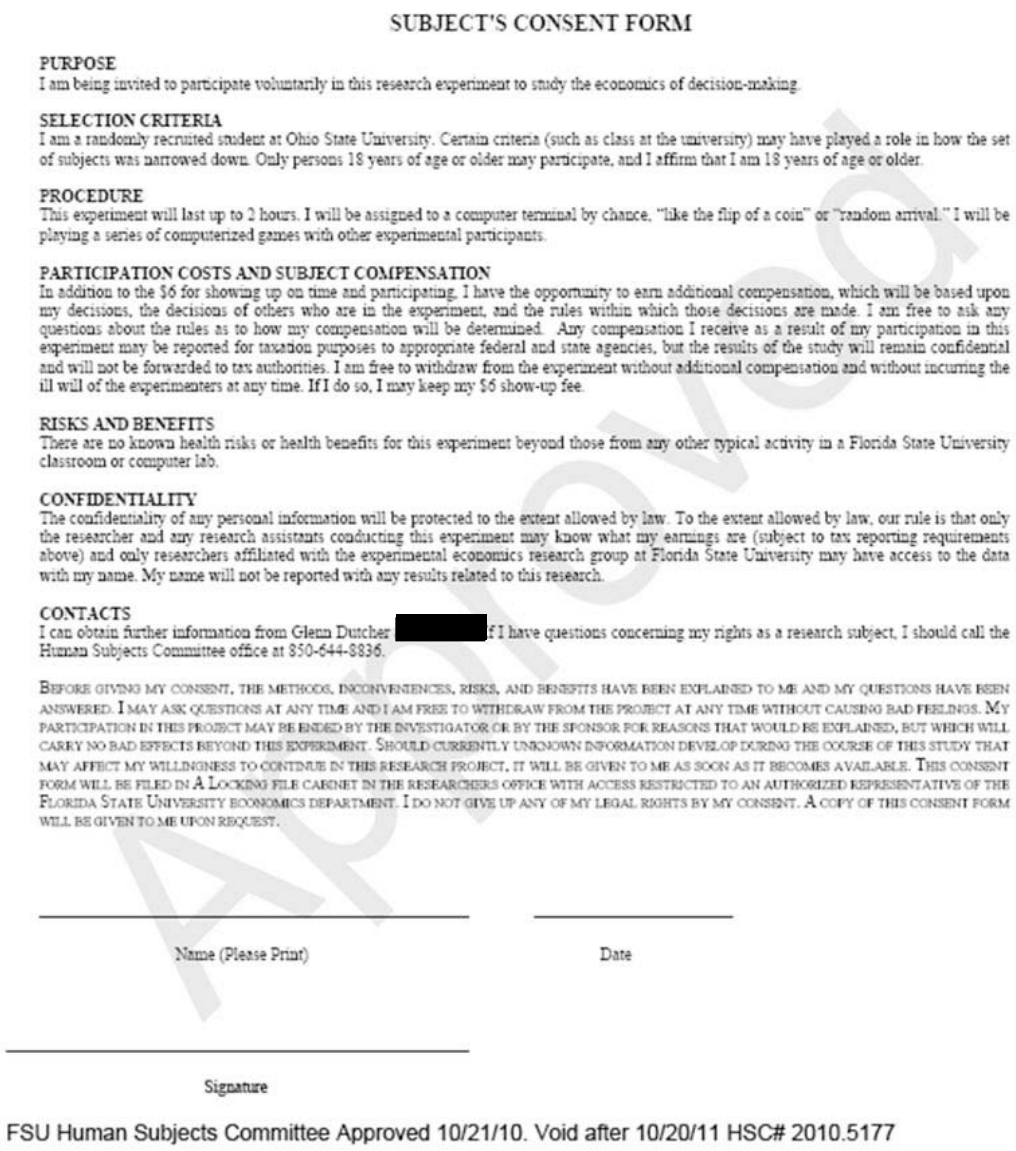
CONTACTS
I can obtain further information from Glenn Dutcher or Cortney Rodet at [redacted] or Prof. Tim Salmon at 850-644-7207. If I have questions concerning my rights as a research subject, I should call the Human Subjects Committee office at 850-644-8836.

BEFORE GIVING MY CONSENT, THE METHODS, INCONVENIENCES, RISKS, AND BENEFITS HAVE BEEN EXPLAINED TO ME AND MY QUESTIONS HAVE BEEN ANSWERED. I MAY ASK QUESTIONS AT ANY TIME AND I AM FREE TO WITHDRAW FROM THE PROJECT AT ANY TIME WITHOUT CAUSING BAD FEELINGS. MY PARTICIPATION IN THIS PROJECT MAY BE ENDED BY THE INVESTIGATOR OR BY THE SPONSOR FOR REASONS THAT WOULD BE EXPLAINED, BUT WHICH WILL CARRY NO BAD EFFECTS BEYOND THIS EXPERIMENT. SHOULD NEW INFORMATION BECOME AVAILABLE DURING THE COURSE OF THIS STUDY ABOUT RISKS OR BENEFITS THAT MIGHT AFFECT MY WILLINGNESS TO CONTINUE IN THIS RESEARCH PROJECT, IT WILL BE GIVEN TO ME AS SOON AS POSSIBLE. THIS CONSENT FORM WILL BE FILED IN A LOCKING FILE CABINET IN THE RESEARCHERS OFFICE WITH ACCESS RESTRICTED TO AN AUTHORIZED REPRESENTATIVE OF THE FLORIDA STATE UNIVERSITY ECONOMICS DEPARTMENT. I DO NOT GIVE UP ANY OF MY LEGAL RIGHTS BY MY CONSENT. A COPY OF THIS CONSENT FORM WILL BE GIVEN TO ME UPON REQUEST.

_____ _____

FSU Human Subjects Committee Approved 4/20/10. Void after 4/19/11 HSC# 2009.3730

Figure 5.2: Subject Consent - Punishment



SUBJECT'S CONSENT FORM

PURPOSE
I am being invited to participate voluntarily in this research experiment to study the economics of decision-making.

SELECTION CRITERIA
I am a randomly recruited student at Ohio State University. Certain criteria (such as class at the university) may have played a role in how the set of subjects was narrowed down. Only persons 18 years of age or older may participate, and I affirm that I am 18 years of age or older.

PROCEDURE
This experiment will last up to 2 hours. I will be assigned to a computer terminal by chance, "like the flip of a coin" or "random arrival." I will be playing a series of computerized games with other experimental participants.

PARTICIPATION COSTS AND SUBJECT COMPENSATION
In addition to the $6 for showing up on time and participating, I have the opportunity to earn additional compensation, which will be based upon my decisions, the decisions of others who are in the experiment, and the rules within which those decisions are made. I am free to ask any questions about the rules as to how my compensation will be determined. Any compensation I receive as a result of my participation in this experiment may be reported for taxation purposes to appropriate federal and state agencies, but the results of the study will remain confidential and will not be forwarded to tax authorities. I am free to withdraw from the experiment without additional compensation and without incurring the ill will of the experimenters at any time. If I do so, I may keep my $6 show-up fee.

RISKS AND BENEFITS
There are no known health risks or health benefits for this experiment beyond those from any other typical activity in a Florida State University classroom or computer lab.

CONFIDENTIALITY
The confidentiality of any personal information will be protected to the extent allowed by law. To the extent allowed by law, our rule is that only the researcher and any research assistants conducting this experiment may know what my earnings are (subject to tax reporting requirements above) and only researchers affiliated with the experimental economics research group at Florida State University may have access to the data with my name. My name will not be reported with any results related to this research.

CONTACTS
I can obtain further information from Glenn Dutcher [redacted] If I have questions concerning my rights as a research subject, I should call the Human Subjects Committee office at 850-644-8836.

BEFORE GIVING MY CONSENT, THE METHODS, INCONVENIENCES, RISKS, AND BENEFITS HAVE BEEN EXPLAINED TO ME AND MY QUESTIONS HAVE BEEN ANSWERED. I MAY ASK QUESTIONS AT ANY TIME AND I AM FREE TO WITHDRAW FROM THE PROJECT AT ANY TIME WITHOUT CAUSING BAD FEELINGS. MY PARTICIPATION IN THIS PROJECT MAY BE ENDED BY THE INVESTIGATOR OR BY THE SPONSOR FOR REASONS THAT WOULD BE EXPLAINED, BUT WHICH WILL CARRY NO BAD EFFECTS BEYOND THIS EXPERIMENT. SHOULD CURRENTLY UNKNOWN INFORMATION DEVELOP DURING THE COURSE OF THIS STUDY THAT MAY AFFECT MY WILLINGNESS TO CONTINUE IN THIS RESEARCH PROJECT, IT WILL BE GIVEN TO ME AS SOON AS IT BECOMES AVAILABLE. THIS CONSENT FORM WILL BE FILED IN A LOCKING FILE CABINET IN THE RESEARCHERS OFFICE WITH ACCESS RESTRICTED TO AN AUTHORIZED REPRESENTATIVE OF THE FLORIDA STATE UNIVERSITY ECONOMICS DEPARTMENT. I DO NOT GIVE UP ANY OF MY LEGAL RIGHTS BY MY CONSENT. A COPY OF THIS CONSENT FORM WILL BE GIVEN TO ME UPON REQUEST.

Name (Please Print) Date

Signature

FSU Human Subjects Committee Approved 10/21/10. Void after 10/20/11 HSC# 2010.5177

Figure 5.3: Subject Consent - Social Distance

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BIOGRAPHICAL SKETCH

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I was born in La Junta, Colorado, though my family eventually settled in Perryton, Texas where we were surrounded by a supportive extended family. I found immediate success in high school athletics and made it through four years of college on a track and cross country scholarship. After meeting and marrying my wife in Missouri, I tried jobs both in IT and real estate. Finding that these careers did not suit me well, I went back to school and finished my undergraduate degree in Economics from the University of Central Missouri. I then had the good fortune of attending graduate school and studying under some of the most respected names in the field of experimental economics at Florida State University. I currently have a joint paper with David Cooper which has been accepted for publication in Experimental Economics and revisions requested on another paper at the Journal of Economic Behavior and Organization. I have accepted a two year position at the University of Innsbruck.