EXPERIMENTS IN ALTRUISM AND CHARITABLE GIVING

A Dissertation

by

HALEY ANNE HARWELL

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Chair of Committee,	Catherine Eckel
Committee Members,	Alex Brown
	Timothy Gronberg
	Jane Sell
Head of Department,	Timothy Gronberg

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ABSTRACT

This dissertation introduces three sections on giving, employing a variety of methods in different environments to investigate giving. The first section, utilizes a simple incentivized game to assess differences in risk-sharing norms across communities. In the game, subjects decide whether to share resources with anonymous group members who have lost a lottery where everyone in the group has the same potential for a positive or negative outcome. To gauge the impact of formal institutions on informal risk sharing, subjects make a second sharing decision; but this time they have the opportunity to purchase simple insurance, which guarantees a positive outcome. I found that insurance crowds out informal risk sharing; the amount people share decreases significantly when self-insurance is offered, no matter whether the decision maker chooses to insure.

The second section, studies the effects of successful fundraising campaigns on individuals' philanthropic behavior. We investigate the source of funds that are raised in successful campaigns. Using a controlled lab experiment, we ask whether new funds are raised, or if individuals merely redirect funds from contributions to other organizations following a successful campaign. The results show that a successful campaign increases funds raised by the charity. However, the increase in giving to the target charity comes entirely at the expense of the other charities. This provides strong evidence for a 'crowding-out' effect for targeted campaigns.

My third and final section, investigates cooperation, giving, and the effect of punishment in two simple games, the trust game and the public goods game. Notably, the paper examines the puzzle presented by past experimental results of punishment effects in

the games. Punishment increases contributions in the public good game, while decreasing cooperation in the trust game. To test the effect of punishment on various sets of game parameters, subjects play modified versions of the two games. By observing the differences in contributions with and without punishment, I find design features are not related to the different effects of punishment.

DEDICATION

To my adviser, friend and mentor Catherine Eckel:

You have been an inspiration and offered more support than I could ever give you credit for. This would not have been possible without you. Thank you for all your support.

To my parents, Anthony and Patricia Harwell:

I hope this achievement acknowledges all the love and support you have given me throughout my life. Please know I could never express my gratitude for never giving up on me, or my dreams. You have never lost your faith in me, and it is my never-ending quest to make you proud.

To my nephews and nieces:

Please take this as evidence; never give up on your dreams. You can do anything you put your mind to.

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NOMENCLATURE

SO98	Selten and Ockenfels 1998
FT	Feeding Texas
TCE	Texas Campaign for the Environment
OK	Operation Kindness
CBEES	Center for Behavioral and Experimental Economic Sciences
ERL	Economic Research Laboratory
TG	Trust Game
PGG	Public Goods Game

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1. GENERAL INTRODUCTION

Focusing on altruism and giving, this dissertation introduces three studies investigating pro-social behavior. Previous research has established the importance of preferences at the individual and group level; the work I present here expands this body by investigating these factors in the realm of pro-social behavior.

In the first essay (Section 2), we use data collected in the field to examine risk-sharing norms across three different communities. We elicit norms of risk sharing within communities by examining how subjects share resources with members of their community. By employing a game that avoids the issues of adverse selection or moral hazard, we abstract from many factors including social ties, proximity, neediness, or the expectation of future reciprocity, which may affect risk-sharing. In doing so, we could investigate differences in risk-sharing norms and their causes across three distinct communities.

The experiment utilizes the solidarity game, developed by Selten and Ockenfels (1998), where subjects have an opportunity to share resources with a disadvantaged member of their three-person group. In each round of the game, subjects have a two-thirds chance of earning a positive amount of money and a one-third chance of earning zero. Each subject is a member of a group with two other subjects whose identities are unknown and is asked to choose whether to transfer funds to members who lost the lottery. Our primary treatment introduces the availability of a market alternative for insurance; purchasing the market insurance gives the participants a guaranteed positive outcome. Subjects then repeat the procedure outlined above with the addition of the insurance

option. By comparing the transfers with and without insurance, we can investigate how informal risk sharing responds to the presence of formal insurance; we conclude that formal institutions crowds out informal risk sharing.

The inclusion of three distinct communities affords us the opportunity to observe if community characteristics such as access to formal credit, access to formal insurance, or risk exposure impact giving in this setting. Using the mentioned community characteristics, we identify communal norms that could influence informal risk sharing behavior. All communities exhibit the norm of sharing with disadvantaged group members, but community income strongly influences the amount of the transfer. Supporting this finding, we find that sharing risk is strongest in the lowest-income community. When insurance is available, we see a significant decrease in informal transfers to less fortunate group members. The reduction in sharing comes from both those who purchase insurance and by those who do not. Surprisingly, the effect is caused by the availability of insurance regardless of pickup; the effect is lowest in the poorest community with the strongest norm of sharing. This experiment presented an investigation of the effect of formal institutions on informal pro-social behavior. My next project (Section 3), examines pro-social behavior in the realm of formal institutions.

Charitable organizations use creative means to raise funds from donors, and campaigns in the United States helped raise much of the 358 billion dollars in charitable giving in 2013 (McKitrick, 2014). Recently, there has been a considerable number of fundraising campaigns conducted on social media sites. Livestrong, Susan G. Komen for

the Cure, Unicef, and the recent ALS Ice Bucket Challenge have illustrated the power of successful campaigns.

Considerable research investigates the factors that affect contributions, on both the intensive and extensive margins. However, little is known about the impact the campaign of one organization has on giving to other organizations. A successful campaign may attract new donors and additional spending by existing donors; but if donors have a fundraising budget, then an increase in giving to one cause will be fully offset by a decrease in giving to other causes. Thus, targeted campaigns may ultimately crowd out giving to the untargeted philanthropic organization.

To test our hypothesis, we conduct a "real donation" lab experiment (Eckel and Grossman 1996) examining whether a successful campaign crowds out giving to other charities. In each session, subjects are given an endowment and the opportunity to donate to three different Texas charities. Subjects then see a campaign for one of the three charities. The campaign we ran in the lab consisted of a short video describing the organization's mission and activities, and urged subjects to give to the targeted charity. Following the campaign, subjects again repeated their donation decision.

By comparing the subjects' allocations before and after the campaign, we measure the effect of the campaign on giving, to the target of the campaign as well as the other two charities. In all cases, the campaign succeeded in increasing giving to the target organization, indicating that the campaigns were effective. However, in all cases, total giving (the sum of donations to all three charities) remained unchanged; put simply, the increase in giving to the target charity came entirely at the expense of the other charities.

This provides strong evidence for a 'crowding-out' effect of a targeted campaign. We find that subjects do not increase their total charitable expenditures after receiving the video treatment. Instead, they reallocate funds from other charities to the targeted charity. This indicates campaigns affect the intensive margin and donors consider charities substitutes for each other.

In response to this finding, we design an additional treatment where we prime general charitable behavior. This was done to test the effects of campaigns without presenting a trade-off in charities. Using the same design as before, we prime a general charitable cause; this charitable prime does not produce an increase in money sent to the three charities. These results strengthen our prior evidence that subjects have a set budget of spending with regard to charitable contributions. This paper investigated how a positive influence can affect pro-social behavior. My last project investigates the opposite through testing the effects of potential negative outcomes on pro-social behavior.

In the final experiment (Section 4), I observe the effect of punishment on giving and cooperation. Previous work in this area has found mixed results; in one environment, the Public Goods game, punishment is effective at increasing pro-social behavior. However, in a second atmosphere, the Trust Game, punishment leads to less pro-social behavior. Through a survey of previous literature, I isolate parameters that I believe may explain the confounded results. I create hybrid versions of the original games to test my hypothesis.

Using the Frankenstein games, I explore if the structural differences are the explanation for the different effect of punishment. In particular, I consider whether the

game is repeated, and if the game is simultaneous or sequential. Using a 2x2 design, I run four treatments varying the number of rounds (one-shot vs. repeated) and order of play (simultaneous vs. sequential.). Using a combination of these factors, we observe the impact that punishment has on each game. By measuring the differences in the contributions with and without punishment, I can isolate the relationship between punishment and separate features of the game.

My results indicate that punishment is effective in increasing contributions in the public goods game, as long as the game is repeated more than once. On the other hand, in the trust game there is not a significant increase in cooperation or giving under any setting. Even though we see vast differences in the frequency of punishment in both games across settings, we find little evidence that the game structure explains the different effects of punishment.

Together, my three essays investigate pro-social behavior with respect to altruism and giving in different experimental settings. Through testing the effects of norms, punishment, and campaigning, these essays paint a picture that pro-social behavior is sensitive to the environment, other actors, and cultural norms.

2. USING EXPERIMENTAL GAMES TO UNDERSTAND RISK SHARING BEHAVIOR IN THREE COMMUNITIES

2.1 Introduction

"The impersonal hand of government can never replace the helping hand of a neighbor."

—Hubert H. Humphrey

Low-income individuals lack access to formal institutions that provide safety nets for unforeseen events. Without institutions like insurance, individuals must use informal sources of support to overcome the effects of sudden unforeseen shocks. One such mechanism that is used is sharing monetary resources with individuals in a community is solidarity. Solidarity is defined as "a willingness to help people in need who are similar to oneself but victims of outside influences such as unforeseen illness, natural catastrophes, etc." (Selten and Ockenfels 1998, hearafter SO98). Solidarity can be defined as a type of informal, indirect reciprocity; taking care of others who have ended up in a bad financial situation by chance. This informal mechanism mirrors what many scholars have identified as informal risk sharing. Informal risk sharing institutions develop when there is a lack of formal market sources of credit and insurance. Risk sharing arrangements exist when individuals informally share risk with others, spreading potential losses among members of an informal group. Risk sharing occurs in many situations including poor harvest, health emergency, lost employment, funeral expenditures, and other unexpected expenses. When group members transfer, funds or goods it helps reduce individual exposure to shocks.

This informal institution has been shown to insure individuals against income shocks in developing countries (Fafchamps and Lund 2003). Risk sharing allows households to insure even though formal institutions do not exist.

Empirical research supports that individuals in developing countries who are susceptible to income shocks participate in informal risk sharing groups to help smooth consumption when unexpected shocks exist (Townsend 1995, Cox and Jimenez 1998). This mitigates losses when formal market solutions are unavailable. Informal risk sharing has been observed in the rural Philippines and findings indicate households receive help through friends and relatives by "massive transfers of funds in the form of both gifts and loans" (Fafchamps and Lund 2003, p. 283). Individuals with low income are frequently left without recourse to formal credit, and must rely on members of their network for assistance (De Weerdt and Dercon, 2006).

Many of the same problems found in less-developed countries also arise in less-developed or disadvantaged neighborhoods in developed countries, but this phenomenon has not been studied in that context. If individuals have limited access to formal institutions, it is likely they rely on the informal mechanisms to smooth income fluctuations when unfortunate events occur. Risk sharing is difficult to study with observational data because most such monetary transactions take place off the record. The transfers occur through family or community connections, making it difficult to collect or obtain this data. However, researchers have utilized games that proxy sharing to measure the norms within a community or group: This is our approach. The games offer another advantage, in that they can be manipulated to test specific hypotheses about the response

of risk sharing to changes in the context or environment. In particular, the effects of a formal market alternative like insurance. If an individual can self-insure, it is likely that this may have a substantial impact on informal sharing arrangements. Social science literature has shown that market alternatives can crowd-out intrinsic pro-social behavior (Landmann, Vollan and Frölich 2012, Bowles 2008).

We implement an artefactual field experiment in three unique communities in a developed country. Subjects participate in a game that proxies risk sharing. In this game, there are three member anonymous groups and everyone has the same chance of a positive or negative outcome. Group members can share resources if they experienced a positive outcome, with group members who received a negative outcome purely by chance. The transfers the subjects make to group members give us a baseline measure for risk sharing. In the treatment, we allow individuals to self-insure, guaranteeing them a positive outcome. After subjects, make the decision to purchase insurance we then elicit the same transfers as before. This measure allows us to observe the crowding out that a formal market alternative has on the informal risk sharing. In this study we elicit norms of informal sharing within communities by examining how subjects share resources with anonymous others in their community. By removing the social connection, we abstract from many factors that might affect risk sharing. These include importance of family or friendship ties, proximity, neediness and the expectation of future reciprocity. Utilizing a game that rules out the problems of adverse selection or moral hazard that are typically associated with sharing groups allows us to observe risk sharing at the most basic level.

The experiment produces several interesting results. First, risk sharing occurs in developed countries for the same reasons as in developing countries. We find that communities with little access to formal institutions share risk more often than communities who have access to formal institutions. This is similar to what research in developing countries has shown. Individuals use the informal risk sharing as an alternative to formal insurance and credit. Second, the availability of insurance crowds out informal risk sharing, but the level of crowding out depends on community norms. When strong community norms for risk sharing exist, the formal institution has less impact on reducing sharing in the informal institution.

2.2 Background and Prior Research

Selten and Ockenfels (SO98) develop an experimental game designed to identify the willingness of people to help others in their group who are in a worse situation purely by chance. This type of sharing is consistent with shocks that individuals may face, such as crime or job-loss, that negatively influence income. Solidarity refers to the ties that bind individuals together in a community. This can be measured by a willingness to share resources with others in the same community in a simple game.

The "solidarity game" is an incentivized game that involves groups of three subjects. Each subject has a 2/3 chance of receiving 10DM, and a 1/3 chance of 0DM. Each person makes two contingent decisions, indicating the amount they are willing to send in a situation where there is one loser and two winners in their group, or in a situation where there are two losers and one winner. The motivation behind the transfers is based on individual preferences as well as closeness of group members (Barr and Genicot 2008.)

SO98 use this design to determine “types” of giving strategies. The types of strategies include individuals who have a fixed budget for giving no matter the number of losers, individuals who have a set gift they will send to a loser no matter the number, individuals who never transfer any money to losers, or an intermediate type. Bolle et. 2011 explore motives for giving in the solidarity game. The authors find motives can be determined for three quarters of their sample. Works has also explored solidarity behaviorbol with context to different cultural impacts such as East and West Germany through time (Ockenfels and Weimann 1999, Brosig-Koch et al. 2011.)

Many adaptations of the basic solidarity games have been studied. For example, Büchner, (2007) look at gift transfers before or after finding out if the person is lucky. Trhal and Radermacher (2009) observe self- inflicted neediness on sharing behavior in the solidarity game.They find no difference in gifts in these two environments. Other authors have designed different games to look at informal risk sharing in groups of two in a laboratory setting. (Charness and Genicot, 2009) The authors’ find risk aversion plays a role in risk sharing, by making a person ultimately smooth consumption over time in a student population. Extensive research has been done in field settings regarding risk sharing. The field studies have taken place throughout the world in less developed areas. For example, Attanasio, et al. 2012 shows that individuals assortatively risk pool, and confirms that network connections and proximity have influence on an individual’s willingness to share resources. The closer proximity within a network, the more risk attitude influences groups.

Reciprocal- self-insurance does not happen on a community level, but many times

more within the family and friend's level. (Fafchamps and Lund 2003) Research has been done in similar style using a loosely translated solidarity game. The authors offer insurance in the rural Philippines. They find:

> "The fact that the crowding-out effect can completely offset the protection offered by the insurance hinges on the incomplete take-up. If everybody was insured nobody would be left with a catastrophic outcome even in the complete absence of solidarity transfers. Yet, while around half of all participants opt for insurance if they have the choice, there is a substantial part remaining uninsured. Those uninsured now face a much higher risk of being left alone with a bad outcome than in the scenario when nobody can be insured." (Landmann, Vollan and Frölich 2012, p.5)

The SO98 solidarity design has been explored in a community field experiment in an urban setting in a developed country by my co-authors (de Oliveira, Eckel and Croson, 2014). The authors find evidence of voluntary, informal risk sharing in an urban poor neighborhood. The authors also find significantly less egotistical players and more fixed gift type strategies than in student samples. This indicates subjects are exhibiting reciprocity towards less fortunate group members. We build upon this study by comparing the baseline game and include a treatment offering a formal market for insurance. We expand this study across the original community and two additional Texas communities. Using the SO98 structure and adopting the simple visual field implementation used in de Oliveira, Eckel and Croson (2014) we identify informal sharing behavior when there is no formal market alternative, and then examine behavior after introducing an efficient market alternative of insurance. Using SO98 design we feel the transfers indicate a proxy of informal sharing and can help gage the willingness individuals have to help less fortunate group members. By offering adaptations, we can explore the situation of how formal

market alternatives such as insurance can change the solidarity behavior. By examining the transfers, we can gauge the degree of solidarity toward worse-off individuals in the group whose disadvantage occurs by chance.

Much of the literature states that less developed countries share risk in lieu of the unavailable market alternatives. However, very little research has identified if this practice exist in developed countries. It is important to understand if this informal risk sharing exists in developed countries, especially poor communities in these countries. Ultimately, the design will allow us to examine if solidarity is affected by ones income. In addition, by offering a treatment with a formal market alternative we can examine the impact that insurance has on informal institutions. By examining the market alternative, we can identify individual differences in insurance purchases. We expect to see insurance decisions effected by individuals risk preferences. Someone who is risk averse should be more likely to purchase insurance. Likewise, individuals living in high-risk areas (high crime, natural disaster prone, etc.) might consider insurance differently. We also expect the insurance to crowd out sharing to people who did not choose to self-insure. Examining three varying communities allows us insight into people's behaviors about informal sharing and individual preferences or community norms that motivate sharing.

2.3 Experimental Design

Our research involves a visual representation of SO98's solidarity game as well as a simple variation on the basic game. Our design includes higher stakes (for a non-student population) as well concrete randomization procedures. We fabricate this structure so subjects can clearly understand the game and decisions. In the treatment, we expand the

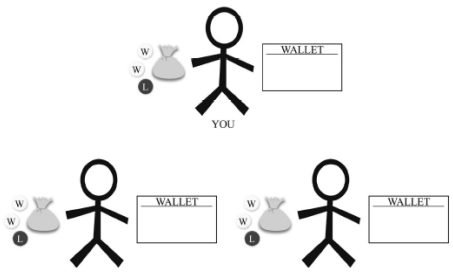
basic game to examine the availability of a formal market alternative. This market alternative is in the form of self-insurance.

In all three communities, this game is one of multiple experimental games played. The experimental games were always run in the same order, with no feedback between tasks. Matching for the Solidarity game and treatment took place at the end of the sessions only if this game was selected for payment. One game was chosen at random for payment, so neither the subjects nor the experimenter knew which activity would be paid until the end of the session.

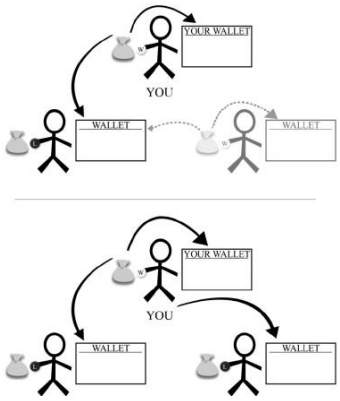
The Baseline Treatment, subjects play the SO98 solidarity game with increased stakes. The subjects have a 2/3 chance of winning $75 and a 1/3 chance of $0. This results in the expected value of the baseline decision is $70 (2/3 chance of $75 plus the $20 show-up payment). Subjects were explained the game and all possible outcomes (No losers, 1 Loser, 2 Losers, and 3 Losers) using the instruction sheet, and they indicated their transfers on the decision sheet (see Figure 1.) Each subject had an independent draw from a bag of three chips to indicate if they were winners or losers. The bag contained two "W" chips (indicating winner $75) and 1 "L" chip (indicating loser $0). We used very concrete randomization methods to insure the subjects were confident in the probabilities of the outcomes. Figure 1 shows two situations: The top part mimics the situation when the subject and one other group member both drew "W" chips, and there was only one loser in the group. The bottom half of the figure indicates the choice when the subject is the only group member to have the positive outcome (pull the "W" chip from the bag).

Figure 1: Instructional Page and Decision Sheet

Instructional Page



Subject decision sheet



Figures are the actual instruction and decision sheets subjects saw in the experiment

The baseline treatment allows us to observe the most basic solidarity in absence of market alternatives. We consider positive transfers indication of willingness to help group members and proxy this as informal sharing. Subjects indicated the amount they would like to transfer if they pull a "W" chip from the bag. They made decisions for both cases, one loser in the group and two losers in the group. There were no restrictions on transfers other than all transfers and the amounts placed in the wallet had to equal $75.

The Insurance Treatment introduces the possibility of insuring against the loss. The insurance itself is efficient, in that the price is less than the expected loss. In the treatment subjects can elect to pay a fee of $20 (equal to show-up fee) to insure that they will receive the $75.00 with certainty. The insurance increases the expected value of the game from $50 to $55 ($75 minus the $20 payment). If subjects choose not to insure (not to pay the fee), then they play the baseline game described above. The procedures were the same as in the baseline game unless the subject bought insurance therefore replacing

the one L chip in the bag with another W chip guaranteeing them to be winners. All subjects are still able to send money to the "losers" in the group. Although, it is common knowledge the only way an individual would be unlucky (earning $0) is if they did not purchase the insurance. Examining the solidarity of group members when everyone has an opportunity to insure allows us to estimate the potential impact on sharing when a market option is introduced.

We introduce this treatment in order to: a) observe which individuals purchase insurance, and b) if informal sharing is affected by the possibility that individuals can insure – i.e., we ask whether the availability of private insurance will reduce the willingness of others in the group to bail out its members in case of a loss. The transfers subjects make provide insight into the reasons that sharing is employed, and the change in community sharing that is likely to occur in the response to a market-based insurance option. The informal solidarity is a substitute for market-based insurance, so when insurance is introduced it is likely to reduce solidarity within a community. Allowing individual who insure to make transfers allows us the opportunity to identify even if even the most generous supporters of others may react to the presence of insurance by reducing their willingness to support those who do not purchase it.

$$EV_B = \left[\left(\frac{2}{3}\right)\$75 + \left(\frac{1}{3}\right)\$0\right] + \$20_{show-up\ fee} = \$70 \qquad (1)$$

$$EV_T = \left[\left(\frac{3}{3}\right)\$75\right] - \$20_{fee} + \$20_{show-up\ fee} = \$75 \qquad (2)$$

By examining the expected payoffs in both the baseline game (1) and the treatment (2), it is clear that $EV_B < EV_T$. Considering the expected value of the treatment is more than that of the baseline, rational subjects should always pay the fee to purchase the insurance. This allows an increase earnings of $5 dollars. Due to the design of this insurance, we expect that the take-up rates of insurance should be relatively high.

2.4 Sample and Data

The communities differ with regard to income, race, education, and employment therefore the baseline and treatment offer insight on community norms on informal sharing as well as a formal market alternative. This experiment was conducted as part of the field protocol in two different studies covering three distinct communities. Integrating the data from these three communities, we test the relationship between communities and behavior in the baseline game and treatment. These communities differ from the usual undergraduate students, in that they are older, less educated, tend to have children, and are married. We describe each study in turn. The field studies recruit participants using similar protocols, as follows.

2.4.1 Brownwood and Port Lavaca

This study was designed to assess natural disaster preparedness in two Texas towns. During this study, data was collected for three consecutive years, beginning in 2009. The cities are: Port Lavaca, TX, located on the gulf coast, chosen for its exposure to risk of hurricanes and tropical storms and Brownwood, TX, located in the interior, which faces exposure to risk of flooding and tornados. Over the three-year period, we

recruited 224 individuals in Port Lavaca and 208 people in Brownwood. Subjects were recruited door-to-door, based on a random sample of residential tax parcels in select neighborhoods in the two towns. The solidarity game was conducted in the third year (2011), with 71 subjects in Port Lavaca and 98 in Brownwood. In Port Lavaca the two samples (total sample and participated in 3rd year) were not statistically different with respect to gender (t=1.776), age (t=0.966), marital status (t=0.1793), employment (t=0.1103), education (t=0.4498) and income (t=0.8266). In Brownwood the two samples (total sample and participated in 3rd year) were not statistically different with respect to gender (t=1.082), race (t=1.313), age (t=0.426), marital status (t=0.9597), employment (t=0.6733) and income (t=0.217). However the third year sample did vary from the total sample in Port Lavaca with respect to race (Race_Sample: μ=3.365 SD=0.765 Race_Exper: μ=3.685 SD=1.112 t(286)=2.639 p=0.0088). In Brownwood the samples varied with respect to education (Education_Sample: μ=3.9902 SD=1.066 Education_Exper: μ=4.327 SD=1.419 t(300)=2.298 p=0.0223)

The sample in Port Lavaca is 71% female with an overall mean age of 49. In this coastal community, 12.7% of the subjects have a college degree, and 22.5% did not graduate high school. Half of the subjects (49%) were married and approximately 39% had full time employment. Port Lavaca subjects have a median income of 20K-30K yearly. This median falls slightly lower than the city's median income, which is approximately 33K year. In Brownwood, the sample has 63% females with a total mean age of 48.9. In Brownwood 27.6% of our subject, pool has a college education and 40.8% report some college. Half of the subjects were married and had full time jobs. This community is the

wealthiest of the three, with the median income ranging from 30K to 40K. The solidarity game was run in the final sessions in 2011 along with risk, time preference, and strategic ignorance. Many of the subjects had participated previously in dictator and ultimatum games as well. The subjects were paid for one task chosen at random and earned on average $75 including a $20 show-up fee.

2.4.2 South Dallas

The next study is South Dallas Neighborhood study, designed to analyze the impact of a large public investment project on a low-income, predominantly African-American neighborhood. The sample was recruited based on information obtained from a geographically weighted sample selected to represent households at varying distances from three new light rail transit stations in the neighborhood (Leonard et. Al 2011.) Because the goal of the larger study was to examine the impact of light rail investment on child behavior outcomes, families with children were preferentially selected into the study. Community-based field researchers made initial contact at the participants' home and invited them to come to a community-based field research station to complete the data collection. Survey data was collected from 496 participants. Of these, 198 randomly selected participants participated in economic experiments. We conducted t-tests to compare mean characteristics of the sample that participated in the economic experiments versus the sample that completed only survey and obesity measures.[1] The two samples

[1] The final study is based on a sample of 169 participants with the complete set of variables necessary for the analysis that follows. The other data points were independent variables (13 participants) or the dependent variable (14 participants).

were not statistically different with respect to gender (t =1.4749), race (t=0.1077), marital status (t = 0.0969), or employment (t =1.4536). There was significant statistical difference from the sample population and the participants in the experiments concerning age, education and income. (Age_Sample: μ=48.43 SD=14.675 Age_Exper: μ=43.484 SD=13.190; t(674)=4.064 p=0.0001; Education_Sample: μ=3.4347 SD=1.434 Education_Exper: μ=4.042 SD=0.7497; t(631)=5.5115 p=0.0001; Income Sample: μ=2.118 SD=1.5778 Income_Exper: μ=1.2062 SD=1.7242; t(675)=6.6175 p=0.0001) The sample is described in Table A.1 in the appendix.

South Dallas is a community that includes Fair Park in Dallas. In our sample, 96% were African American. Our subject pool consisted of 61% women and the mean age was 44 years old. Only 18% of the subject pool was married and 54.4% of the sample reported they were single; 10.4% of our sample was employed full time. The proportion of the sample that graduate high school is 42%, with 8.8% college graduates and 22.9% reporting dropping out of high school. This community has the lowest income of the three. Ninety-five individuals in South Dallas (49%) report a household income of less than $10,000.

The experiments were conducted in November 2009 and February 2010. Among the measures collected in that study is a series of experimental games. For the present study, we include 181 observations for individuals who took part in this protocol. Participants completed the solidarity game and one of two the treatments as two of the seven incentivized games that were conducted in the South Dallas sessions. The other games are three games assessing risk aversion, and one for time preference (see Figure 1 for these games), a set of four dictator games, and the trust game. The participants were

paid for one of these incentivized games chosen at random at the end of the series, and the average earnings were approximately $70 including a $20 show-up fee. Subjects received no feedback on the outcomes of the games, except for the one randomly selected for payment.

2.5 Predictions

Examining the differences in communities, we can make predictive associations with what could happen with the monetary transfers to other group members in both the baseline and treatment. By examining key variables that likely impact community sharing, we identify the directional change that each factor will have on both sharing in the baseline as well as the insurance purchase in the treatment.

The three important factors are access to credit, risk exposure and insurance experience. Access to credit will be defined in two parts. First, we will consider the median income of the community, and secondly the accessibility of formal financial institutions such as banks or credit unions. Risk exposure identifies risks that are more significant than a "normal" level of exposure. This could include things such as likelihood of natural disasters, as well as unemployment and crime. All these particular issues effect the riskiness of living in a particular community. Insurance experience will include familiarity with insurance. We will also examine the accessibility of insurance through institutions as well as available financial resources to procure insurance.

We will discuss each community with regard to these three factors. Using survey data collected in each community, as well publically accessible information we examine each variable with regard each communities.

2.5.1 Port Lavaca

We have identified Port Lavaca as having moderate access to credit. This is because Port Lavaca has moderate levels of income (The community has the median income out of our three communities). The median income was 20K-30K. We also consider this coastal town to have normal access to financial institutions. This community has a population of 12,248 covering 13.6 square miles. Within this area are 13 financial institutions.[2] There are plenty of resources to access credit. Residents earn enough income that allows them to participate in formal financial institutions. In our survey, 80% of subjects have checking accounts and 67% save money each month. We also find that 48% of participants have access to at least one major credit card.

Port Lavaca being a coastal town we can expect them to have risk exposure related to natural disaster including hurricanes and tropical storms. The last major natural disaster was Hurricane Ike in August of 2008. The city had a mandatory evacuation in preparation for the storm. Port Lavaca has multiple chemical production companies. These companies include Alcoa, Union Carbide, Du Pont, and Formosa. Lavaca Bay has been a coastal area of concern due to these companies and the pollution of the water. In the 1970's the bay experiences mercury contamination due to wastewaters and leakage being disposed of into the bay waters. ("Lavaca Bay Restoration", 2007) Through a survey question, we elicit what subjects think the biggest risk to the community is. We find that 78% of subjects consider the natural disaster the most risk within the community.

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[2] This count is accurate as of May 2014. This can change due to new banks opening, closing or merging.

Due to the impact that weather plays in Port Lavaca we consider their residents to have clear understanding of insurance, as well as access to insurance. Due to the cities location and the effects that tropical storms or hurricanes have, it is also possible for the community to be familiar with governmental assistance after a catastrophic event.

2.5.2 Brownwood

Brownwood is considered to have high access to credit due to the community's median income in our sample. This community has the highest level of income out of our sample, with the median being 30K-40K. We also see clear evidence that Brownwood citizens have normal availability of financial institutions. Brownwood has a population of 18,972 and the city is 12.6 square miles with approximately 22 banks.[2] In our survey, 85% of subjects have checking accounts and 72% save money each month. We also find that 64% of participants have access to at least one major credit card.

This community also has low risk exposure and has no real threats of natural disasters but a possibility of minor natural events including flooding and tornados. The last event was a small tornado outside of town in March of 1999. There was little damage and no one was injured or killed.

We also predict that Brownwood has normal levels of insurance experience. Brownwood is the community that we use as comparison; this is because the risk level associated within this community is the least. Using this as a baseline allows us to compare communities with greater risk to one that has less community or individual risk.

2.5.3 South Dallas

We consider South Dallas to have low access to credit. This is not only due to their low median income of less than 10K, but also due to the lack of formal institutions within the South Dallas neighborhood. South Dallas area has a population of 29,777 people, and covers 12.8 square miles. This is similar in size to both of the other communities. Within the area there are only two formal banking institutions.[2] In our survey, 57% of subjects have checking accounts and less than 50% are able to save money each month. We also find that only 23% of subjects cash their paycheck (if they get one) at an actual bank. Many use the grocery store, check cashing services or a convenience store to have access to their money. Due to this shortage of institutions, we consider South Dallas to have little access to credit and banking. Subjects have significantly lower access to major credit cards than the other communities do. We also find that only 30% of participants have access to at least one major credit card.

We also consider the South Dallas neighborhood of having high levels of risk exposure. This risk comes from various sources including income instability, possible minor natural events (flooding or tornados) or risk of crime. The crime rate in Dallas is much higher than Port Lavaca or Brownwood. The annual crime rate in South Dallas (2013) was 6,953 crimes per 100,000 people. This is compared to Port Lavaca with 2,099/100,000 and Brownwood 889/100,000.The reported crime rate in South Dallas is much higher than the national average. We also predict that South Dallas will have little experience with insurance due to their lack of formal institutions as well as the limitations placed by their limited income.

Table 1: Impact Variables With Regard to Risk Sharing by Community

	Access to Credit	Risk Exposure	Insurance Experience
Port Lavaca	Moderate (Mod Inc, Normal Institutions)	Natural Disaster & Industrial Accidents	Bailouts
Brownwood	High (High Inc, Normal Institutions)	Minor Natural Events	Normal
South Dallas	Low (Low Inc, Low Institutions)	Crime & Income Instability	None

In Table 1, we can see the predicted effects that access to formal credit, risk exposure, and insurance experience on each community as discussed above. By examining three main factors behind this informal insurance agreement, we can anticipate what will happen concerning sharing behavior on the community level.

2.5.4 Predictions of Behavior

Summarized in Table 2 we make directional predictions about the amount a community will resources with disadvantaged group members as well as purchase insurance. If there is less access to credit, we believe this would create a positive environment for individuals to share resources informally. We also predict that restricted access to credit will create fewer insurance purchases. This is due to the availability of financial means to insure, as well as the facilities and companies to purchase said insurance. Solidarity should increase when there is greater risk exposure on the community or individual level. This increase could be due to natural disasters, job

instability or crime. Although the risk exposure between our communities is different, we still assume that individuals who are exposed to greater risk may find informal ways to mitigate against potential losses and exhibit greater levels of group solidarity. We also predict that the more risk an individual is exposed to the more likely they are to purchase a formal market alternative such as insurance. Finally, we believe that if an individual has little exposure to formal insurance we should see a positive relation to sharing within their group, and a negative relation to insurance purchase.

Table 2: Relationship Between Factors of Solidarity and Insurance Purchase

	Solidarity	Probability of Insurance Purchase
Less Access to Credit	Positive (+)	Negative (-)
Greater Risk Exposure	Positive (+)	Positive (+)
Less Insurance Experience	Positive (+)	Negative (-)

By combining Table 1 and 2, we can make predictions on expected risk-sharing and insurance behavior by community. We then examine the levels of informal sharing for each community as well as the probability for insurance purchase and predict how much the formal institution will crowd out the informal sharing. In Table 3 we predict how the communities will respond to informal risk sharing, insurance purchases and how the formal insurance will affect the informal sharing of risk by community.

Table 3: Variable Effects on Solidarity and Insurance Purchase by Community

	Solidarity	Probability of Insurance Purchase	Impact of Insurance on Solidarity
Port Lavaca	-+- moderate	+++ high	Moderate
Brownwood	--- Low	+-+ moderate	Largest
South Dallas	+++ high	-+- Low	Smallest

2.6 Results

To examine the results, first we must look at what individuals do in the baseline treatment of the game. This will allow us to establish levels of informal risk sharing within groups. Next, we will examine how the market alternative of insurance affects informal transfers. We will also look at which communities and individuals choose to participate in the formal insurance market in lieu of the informal market

2.6.1 Baseline Results (Risk Sharing)

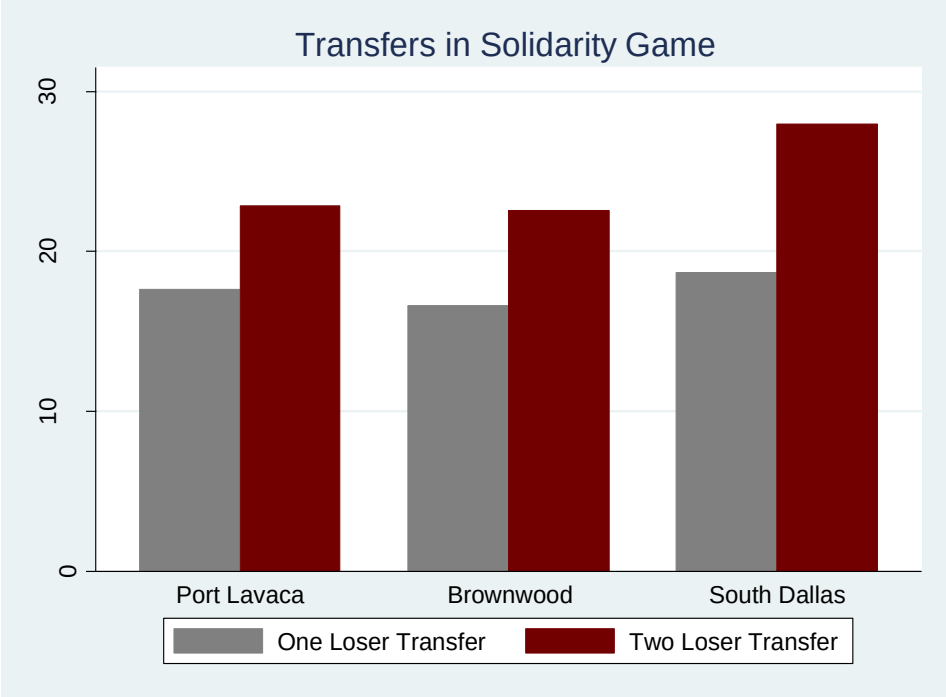
By examining the transfers, we can gauge the degree of solidarity toward worse-off individuals in the group whose disadvantage occurs by chance. These transfers indicate a willingness to participate in the informal institution to share risk with others in your group. Transfers to one or two losers in one's group are show in Table 4. The mean transfer for each cell is reported. We also include the standard deviation below the mean transfer. We can also clearly see in Figure 2 the amount individuals transfer by community in both cases of one and two losers. Transfers vary inversely with community-level income

differences: The poorest community, South Dallas, transfers the largest sums; and the richest sample, Brownwood, transfers the least.

Table 4: Mean Amounts Sent in Baseline by Community

Total Amount Sent – Baseline		
	Baseline Transfer	
	($75)	
	One Loser	**Two Loser**
Port Lavaca (n=71)	**17.63** (12.49)	**22.83** (17.31)
Brownwood (n=98)	**16.60** (11.25)	**22.55** (17.69)
South Dallas (n=201)	**18.68** (11.63)	**28.10** (17.19)
Mean amounts transferred. (Standard deviation in parenthesi s)		

Figure 2: Transfers by Community in Baseline Game



We see that individual participate in the informal institution by transferring positive amounts to less fortunate group members. Seeing this occur in all communities indicated that the informal sharing institutions are occurring in communities in developed countries. This phenomenon is similar to what researchers find in developing countries.

Regression analysis is contained in Table 5. Considering there are two observations per individual, we use a random-effects panel regression observing two observations per subject, the transfer for one loser and two losers. We use models that controls for the number of losers and for locality. South Dallas shows significantly higher transfers than Brownwood, the omitted locality. Port Lavaca is also higher, but not significantly so. Model 2 includes demographic controls: a dummy variable for female,

age in years, and income. In our sample, the measure of annual income consists of ten categories from "less than $10,000" through "greater than $100,000" in ranges of $10,000. We define a variable Low Income, which identifies individuals who earn less than $20,000 annually.

When looking at low-income individuals we find a significantly modest decrease in giving.[3] The variable Age is the subject's age in years, ranging from 18-91 in our sample. We see older individuals share more in our sample. We discover that women send less than men do in either the case of one or two losers, similar to Charness and Genicot (2009). They conclude that women transfer less in risk-sharing circumstances than their male counterparts do.

Model 3 adds preferences measures: risk aversion, time preference and a survey measure of altruism. All variables are defined in Table 6. Because solidarity is an informal type of insurance, we expected to see a positive correlation between risk preferences and transfer behavior. The more risk averse an individual is, the more likely they are to participate in risk sharing to help alleviate any possible stresses from fluctuations of income. This is true with our results. People whom are risk averse transfer more than those who are more risk taking. This could be due to the anticipation of mutual expectations. I help you out, and you will help me out. Risk and time preferences are measured using incentivized tasks as part of the experimental protocol. Figure A.1 in the appendix gives

[3] Changing the low-income variable to include only those earning below $10,000 strengthens the results.

the pictorial representation of the preference measures, as they were introduced to the subject.

Table 5: Panel Regression on Amount Transferred, Baseline Game

Panel Regression, DV=Total Amount Sent			
	Communities	Demographics	Preferences
Two Losers	7.566***	7.296***	7.416***
	(0.65)	(0.66)	(0.69)
South Dallas	3.697*	6.232***	7.022***
	(1.66)	(1.81)	(1.84)
Port Lavaca	0.658	0.203	2.467
	(2.10)	(2.10)	(2.19)
Female		-0.982	-1.891
		(1.48)	(1.48)
Age		0.117*	0.121*
		(0.05)	(0.05)
Low Income		-4.472**	-4.591**
		(1.54)	(1.54)
Risk Averse			5.157**
			(1.92)
Never Save			--2.007
			(1.48)
Altruism			0.970**
			(0.37)
Constant	15.791***	12.484***	1.406
	(1.40)	(2.99)	(4.38)
Observations	738	703	649
Number of id	370	352	325
R^2 - within	0.271	0.257	0.261
between	0.0192	0.0595	0.116
overall	0.0744	0.103	0.149
Standard errors in parentheses *** p<0.001, ** p<0.01, * p<0.05, + p<0.10			

Table 6: Definition of Variables

Variable	What it means
Altruism[4]	0=lowest altruism 16=highest altruism
Low Income	1=Income less than 20K
Never Save	1=Always choosing $50 tomorrow, never a larger amount at 6 months
Risk Aversion	1=choosing 40/40 or 30/60 (2 safest gambles)

The variable 'risk aversion' is a dummy variable equal to 1 if the subject selected one of the two lower-risk lotteries[5], and 0 if they chose one of the four higher-risk lotteries. The coefficient on this variable is positive and significant at the 1% level, indicating that individuals who exhibit risk aversion are more likely to contribute an additional $5.16 to less fortunate group members. The variable Never Save is a dummy variable equal to one if the subject never chooses a larger, later amount in time preference decisions. This carries a negative coefficient and enforces impatient people are less likely to risk-share. Using the Altruistic Values Scale (Nickell 98), we define altruistic behaviors. We find the more altruistic an individual is the transfer of funds to less fortunate group members is significantly larger than subjects who are not altruistic. ($p<0.01$)

Returning to our earlier predictions for risk sharing (table 3) with regard to the three

[4] Using Altruism Value Scale: 4 Questions. 1. People should be willing to help others who are less fortunate 2. Those in need have to learn to take care of themselves and not depend on others. 3. Personally assisting people in trouble is very important to me. 4. These days people need to look after themselves and not overly worry about others. The highest altruism score is 13 out of a possible 16.

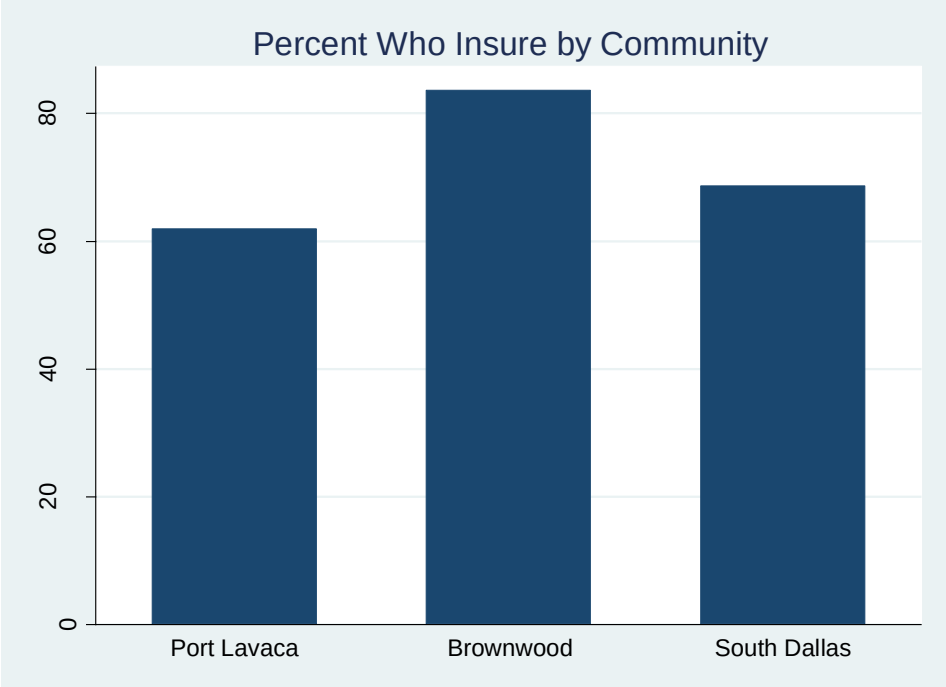
[5] The risk elicitation mechanism we used was Eckel and Grossman (Eckel and Grossman 2002 and Eckel and Grossman 2008b) pictorial representation Appendix Figure A.1. The two lowest gambles are 40/40 and 30/60.

dynamics of this informal mechanism, we confirm the predicted levels of risk sharing anticipated for each community. Port Lavaca has a moderate level of sharing, between the other two communities. Brownwood has the lowest level of informal sharing. Finally, the poorest community South Dallas has the highest level of informal risk sharing, sharing almost $3.70 more without additional controls.

2.6.2 Treatment Results (Insurance)

When there is a formal market alternative for individuals we find that 73.4% of subjects insure across all communities. This insurance rate varies on the community level, with the richest community Brownwood insuring the most and Port Lavaca the least. South Dallas the least experienced community insures at a rate between these other two Texas towns. Figure 3 shows the percent of subjects who purchase insurance separated my community. Interestingly, Port Lavaca the town with the most risk of disasters insures at the lowest rate of only 60%. This could be due to bailouts the community has received in the past when experiencing a disaster. Considering they have significant experience with recovery from storms, the process of a governmental bailout could be engrained in their perception of risk exposure and insurance.

Figure 3: Percent of Subjects Who Insure by Community



To explore the decisions in depth, we conduct a probit regression model, reporting marginal effects in Table 7. The dependent variable is equal to one if the subject paid the fee (equal to $20 show-up fee) for insurance. Port Lavaca, the riskiest community in terms of natural disasters insures significantly less than Brownwood (the omitted community) confirming the possible theory of bailout experience. We also see that individuals who make under 20K a year are less likely to purchase insurance. This is consistent with our prediction, that low-income individuals may not be able to afford the insurance premium. Risk aversion is positively correlated with the likelihood to purchase formal market insurance. Purchasing the insurance takes all the risk out of the game.

This is inconsistent with our predictions Table 2 regarding insurance purchases. We expect that Port Lavaca would have the highest insurance rate considering they have finical access to credit, high risk factor and insurance experience. However, they had the lowest insurance rate and we conjecture that this is a result of governmental assistantship after natural disasters. Brownwood who we believed would have average insurance rate did have the highest insured rate of our communities. They insure more than 80% of the time. South Dallas who we expected to insure the least actually insures more than Port Lavaca.

We next examine the impact of insurance availability on risk sharing. We examine individuals' transfers in both the baseline game in comparison to the treatment. Table 8 contains the total amount that subjects transferred to both one and two unlucky group members in all possible treatments. This will allow us to observe differences in the treatment. It is also important to distinguish between people who purchased insurance and those that did not. The table indicates that the availability of insurance sharply reduces transfers. We observe that individuals who purchase insurance transfer less, but in most cases transfers are also reduced by those who do not purchase insurance. This indicates that just offering a formal institution crowds out the informal institution. The level of crowding out differs by community. In the richest community, Brownwood, transfers decrease about 60 percent for individuals who insure which is 84% of the population. Port Lavaca sees the most crowding out with response to the formal institution. In South Dallas, the poorest community and the community with the least insurance experience, transfers do not fall as substantially and the least amount of crowding out occurs.

Table 7: Which Subjects Purchase Insurance

Who Insures?	
DV= Who Paid Fee (Dummy=1 if Paid)	
	Insurance
South Dallas	-0.077
	(0.07)
Port Lavaca	-0.228**
	(0.09)
Female	0.055
	(0.06)
Age	0
	(0.00)
Low Income	-0.099+
	(0.06)
Risk Aversion	-0.008
	(0.07)
Never Save	0.006
	(0.06)
Altruism	0.015
	(0.01)
Observations	328
$\chi 2$	12.48
Pseudo R2	0.0307
LnL	-197.1

*** p<0.001, ** p<0.01, * p<0.05, + p<0.10

Notes: Marginal effects from a Probit Model. Standard errors in parentheses. The dependent variable equals 1 if the subject paid the fee in the treatment and 0 otherwise.

Port Lavaca's decrease in transfers indicates that the option to insure crowds out much of the informal risk sharing. This is interesting considering they were the community who purchased the least amount of insurance. This indicates that the take-up rate of insurance does not affect the level in which insurance crowds out the informal risk sharing.

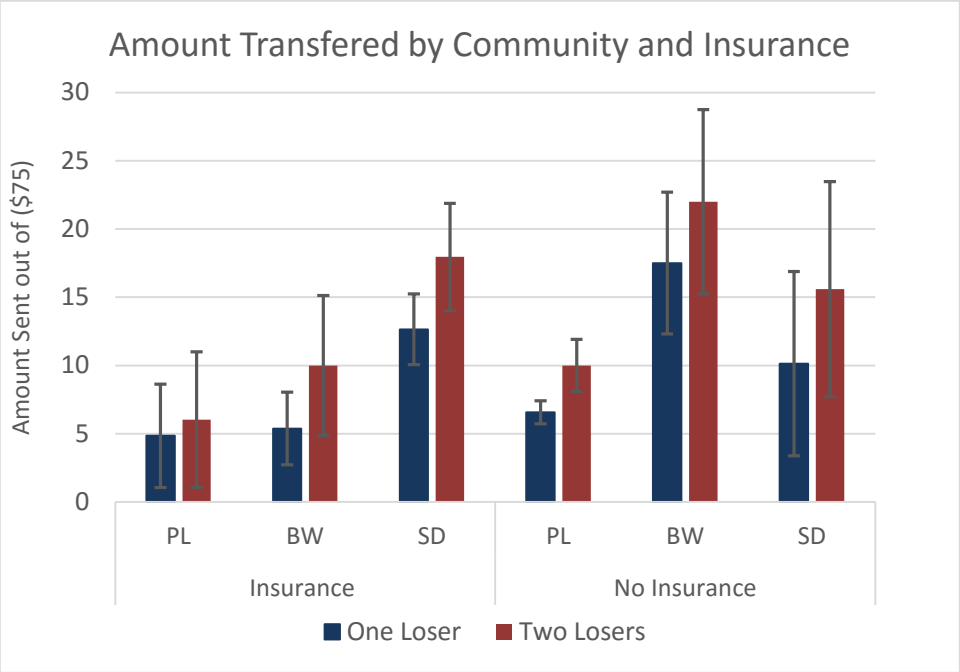
This could suggest that community norms significantly affect the willingness to purchase insurance.

Table 8: Amount Sent in Baseline and Treatment All Communities

Total Amount Sent – Baseline and Treatment						
	Baseline Transfer		**Insurance Purchased**		**Insurance Not Purchased**	
	($75)		**($75)**		**($75)**	
	One Loser	**Two Loser**	**One Loser**	**Two Loser**	**One Loser**	**Two Loser**
Port Lavaca	**17.63** (12.49) (71)	**22.83** (17.31) (71)	**4.85** (9.83) (26)	**6.04** (12.93) (26)	**6.56** (10.60) (16)	**10.00** (13.78) (16)
Brownwood	**16.60** (11.25) (98)	**22.55** (17.69) (98)	**5.38** (9.21) (46)	**10.00** (17.73) (46)	**16.67** (10.61) (9)	**21.11** (12.44) (9)
South Dallas	**18.68** (11.63) (201)	**28.10** (17.19) (201)	**12.64** (11.06) (70)	**17.95** (16.78) (70)	**10.13** (9.22) (30)	**15.60** (15.80) (30)
Standard Deviation reported in top parentheses followed by number of subjects in bottom parentheses						

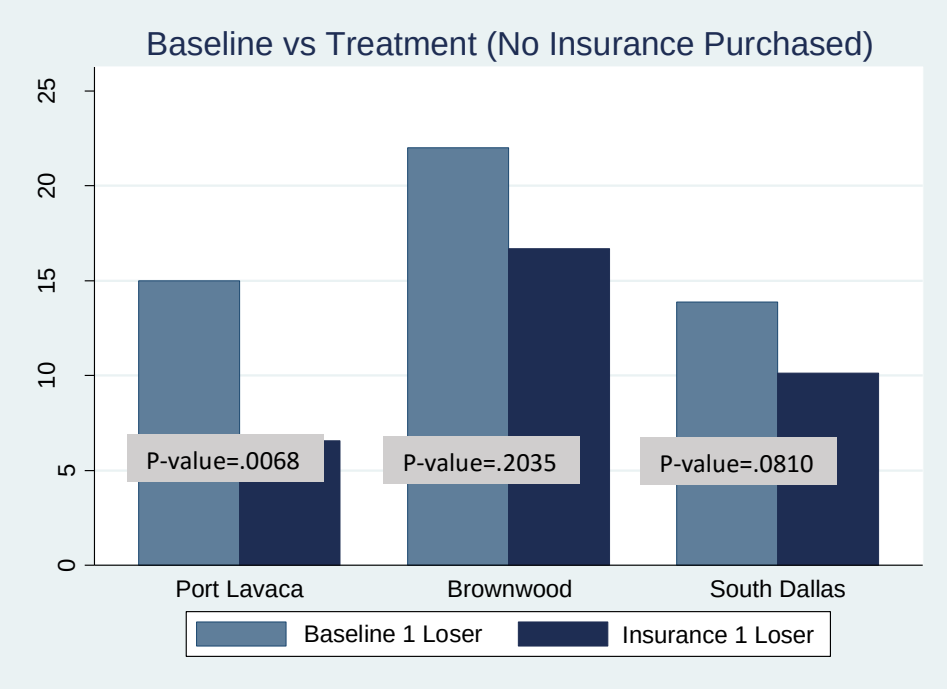
Figure 4 allows us to observe the crowding out insurance availability creates graphically. We can see that for all communities the transfers are less if the individual did not pay the show-up fee to insure themselves a positive outcome.

Figure 4: Transfers by Community and Insurance Purchase



If a subject chooses not to pay the $20 fee for insurance to insure themselves a positive outcome, they are left to play the baseline game. For this reason, we could expect that the transfers from individuals, who do not purchase insurance, would be similar to those from the baseline. Figure 5 shows graphically that this is not the case. Transfers are reduced significantly when there is just an option of formal insurance. We can see given the p-values that all the differences are significant other than Brownwood, the wealthiest community. These community still transfers' larger positive amounts even though there is a formal market alternative. They behave very similarly as in the baseline. The same holds true for transfers in the case of two losers in the group. (See Figure A.2 in appendix) The disparity in the transfers indicates that the opportunity of insurance changes behavior crowding out solidarity.

Figure 5: Baseline vs. Treatment if Insurance is Forgone



We find that in South Dallas, transfers fall the least, and insurance does not crowd out the informal mechanism as much. The transfers only fall by approximately $\frac{1}{3}$ for insured and uninsured. This indicates a greater willingness to share with or without the formal market alternative. This could be related to income or possibly the inexperience with formal financial institutions. This also strengthens the argument that risk sharing is occurring in poor developed areas much as it is in the developing countries.

In Table 9 we turn to a regression analysis of the amount transferred, controlling for treatment, community and number of losers in-group. The dependent variable is the amount sent in the insurance treatments. The model contains two decisions for each

individual – for a situation with one loser and the total sent to two losers; therefore, we use a random-effects panel regression.

Table 9: Panel Regression on Amount Transferred, Treatment

	Treatment	Treatment/Fee	Model 1	Model 2
Two Losers	**5.677*** **	**5.668*** **	**6.918*** **	**6.914*** **
	(0.66)	**(0.66)**	**(0.92)**	**(0.92)**
Insurance	**-8.646*** **	**-6.610*** **	**-5.492*** **	**-1.646**
	(0.66)	(1.18)	(1.54)	(3.03)
Fee Paid		-2.835*	-2.645	-7.593*
		(1.37)	(1.71)	(3.27)
Two Loser Treatment			-2.259	-2.259
			(1.99)	(1.97)
Fee Paid x Two Losers			-0.355	-0.306
			(2.08)	(2.06)
Port Lavaca			-1.546	0.52
			(2.45)	(2.60)
South Dallas			3.335+	2.215
			(2.00)	(2.12)
Insurance x PL				-7.950*
				(3.61)
Fee Paid x PL				4.438
				(4.11)
Insurance x SD				-2.869
				(3.28)
Fee Paid x SD				6.426+
				(3.61)
Constant	17.185***	17.187***	15.182***	15.320***
	(0.97)	(0.97)	(1.71)	(1.77)
Observations	787	787	787	787
Number of id	199	199	199	199
R^2- Within	0.296	0.302	0.306	0.324
R^2-Between	0.000697	0.0000233	0.0271	0.0397
R^2- Overall	0.114	0.115	0.136	0.15

Standard errors in parentheses* p<0.001, ** p<0.01, * p<0.05, + p<0.10**
DV= Amount sent in insurance treatment

First, we regress on just transfers with insurance offered. Second, we add in if the subjects paid the fee to purchase insurance guaranteeing them a positive outcome. We including a dummy variable (Fee Paid=1) to denote this. Model 1 adds communities, including a dummy variable for both South Dallas and Port Lavaca. Model 2 includes both community and interaction variables. We find the treatment effects of insurance are robust to all controls. We also do a likelihood ratio test for Model 1 vs. Model 2 and report that the treatment effects are different across the communities. (LR chi2(4)=17.99, Prob>chi2=0.0012)

Table 10: Summary of Predictions and Outcomes

	Risk-Sharing	Purchase Insurance	Impact of Insurance on Risk-Sharing
Port Lavaca	-+- moderate	+++ high	
Brownwood	--- Low	+-+ moderate	
South Dallas	+++ high	-+- Low	
Port Lavaca	Moderate	Low (61.9%)	Most crowding out
Brownwood	Low	High (83.6%)	Highest crowding out if insurance purchased
South Dallas	High	Moderate (68.6%)	Least crowding out

Table 10 summarizes the previous predictions and overall results. The grey shaded are shows the predictions due to explanatory factors including credit access, risk exposure and insurance experience. The bottom half of Table 10 shows the results that we observed in the communities. We can see although the informal model was a good predictor of risk sharing levels, it missed the mark on insurance rates. The predictions from community factors were accurate with respect to the sharing behavior in the group.

2.7 Conclusion

By combining these rich data sets, we were able to observe the variations in sharing across three distinct communities when there are formal market alternatives such as insurance. Understanding the drivers behind this informal type of insurance or sharing behavior could help us understand alternatives individuals use in response to the lack of access to formal financial institutions. This ultimately affects poor neighborhoods or poor individuals, even in developed countries. We do see strong evidence that this phenomenon of lack of formal institutions is not isolated to less developed countries. We present strong evidence that it is also true for poor areas of developed countries, within our study. We find that just the availability of a formal institution crowds out the informal institution. This happens by transfers decreasing significantly to the less fortunate group members. This was not only true for people who purchased the insurance, but also those who declined paying for a guaranteed outcome. Communities varied on the level of crowding out experienced due to the formal institution's presence within the actual community. We also examined the rates at which people insured. By identifying who insures, we could understand possible motivation of individuals who do not purchase insurance. These

individuals are likely using an informal structure to mitigate against bad outcomes, much like we see in our experiment. Community norms significantly affect the willingness to purchase insurance. This effect is most prominent in lower income areas. We provide evidence of this through the variation in individuals that purchase insurance within our three communities. We see that most of our subjects do purchase insurance, however the 26% that do not purchase the insurance are in significantly worse shape with the informal sharing when there is a formal option available. This is due to the formal institution crowding out the informal sharing. This creates concern for individuals that are using risk sharing as an alternative to formal market options like insurance. If individuals within their community began to partake in the formal institutions, the informal safety net will develop large holes, leaving community members falling through. This is especially true in low-income poor urban neighborhoods.

3. DID THE ICE BUCKET CHALLENGE DRAIN THE PHILANTHROPIC RESERVOIR?: EVIDENCE FROM A REAL DONATION EXPERIMENT

3.1 Introduction

Charitable organizations often need imaginative methods to increase donations. In the charitable giving literature, there is a heavy emphasis on the efficiency of various methods to increase donations. To our knowledge, our paper is the first to study the impact that successful campaigns have on other charitable causes. In this paper, we ask, "When a charitable organization conducts a successful campaign, where do the donations come from?" This paper fills a void in the literature by studying not only the effects of successful campaigns on contributions to the targeted charity, but also the unintended consequences to other non-targeted charitable organizations. On a social level, we investigate the effects of charitable giving campaigns on other charitable causes, which have relevant policy implications for practitioners.

Recently, charitable organizations have taken to social media in force. In the presence of a targeted media campaign, charitable contributions should rise, as shown by the success of multiple campaigns. One example of a successful campaign is "Water is Life," where the charity coined the hashtag "#FirstWorldProblems" and created videos of people in developing countries stating complaints that exist in a first world country. This campaign raised awareness as well as funds to provide clean water to those in need. Similarly, UNICEF ran a social media campaign, "Likes don't save lives," informing possible supporters of how to donate to vaccinate children, and urging them to donate, not

to just “like” or share the post. This media campaign was a successful strategy, motivating people to act by contributing dollars to the cause.

More recently, in the summer of 2014, social media was inundated with videos of people pouring ice and cold water over their heads to participate in the Ice Bucket Challenge. The Ice Bucket Challenge was a campaign intended to raise awareness and donations for amyotrophic lateral sclerosis (ALS) or Lou Gehrig’s disease. The Ice Bucket Challenge was very effective and it is estimated that more than 2.4 million people participated in this peer-to-peer fundraising challenge. In this campaign, recipients are challenged by their friends, family or co-workers to dump a bucket of ice water over their head within 24 hours. If they do not accept the challenge, they donate $100 dollars to the charity that is responsible for funding research on ALS (Phing, 2014). Many celebrities participated, including Robert Downey Jr., Jennifer Aniston, Leonardo DiCaprio, Oprah, 50 Cent, Kermit the Frog, and many more. This challenge generated over $115 million dollars for ALS, and raised awareness for this incurable disease (ALS website). Total donations were twenty times larger than what was raised in the previous year.

Although a large body of research has investigated the effects of various campaigns on their intended charities on both the intensive and extensive margins, little is known about the impact these campaigns have on giving to other charities. A successful campaign may attract new donors and additional spending by existing donors, yet, it is unclear where the additional donations come from. At one extreme individuals may increase donations without decreasing donations to other charities, while at the other, all of the increase in donations to the targeted charity come from decreasing donations to

other charities. The latter case crowds out giving to the untargeted philanthropic organization. As a result, the charity with the successful campaign creates a positive boost, but does so through possibly devastating effects for other charities.

Our research employs a "real donation" laboratory experiment (Eckel and Grossman, 1996) to test whether a successful campaign reduces giving to other charities. It is important to understand the impact on other organizations to understand its true effect. William MacAskill, writing for *Quartz* news outlet, emphasizes that, "$3 million in donations doesn't appear out of a vacuum. Because people on average are limited in how much they're willing to donate to good causes, if someone donates $100 to ALS association, he or she will likely donate less to other charities" (MacAskill, 2014). MacAskill calls this type of behavior "funding cannibalism."

To investigate this possibility, we measure the effect on donations to other charities when a campaign for a single charity is run using a controlled lab experiment. It would be difficult if not impossible to determine the impact of a real campaign in the field. Specifically, it would be difficult to obtain the necessary data needed to examine the effects of a campaign, as we would need to obtain individuals donations to the targeted charity, every other charitable donation for many years, their political contributions, as well as miscellaneous gifts including money to friends, family and the homeless. This data is not readily available, and using survey methods is suspect because individuals misreport contributions. On the other hand, using an experiment in the lab allows us to control various influences in the environment to identify whether the donations to the successful

charity campaign are coming from individuals increasing their budget for charitable giving, or if they are instead taking money that they would have given to other charities.

In Study 1, we test whether a successful campaign crowds out giving to other charities; subjects participate in two dictator games with three charitable recipients. In the baseline, subjects have the opportunity to allocate funds between themselves and three charities. Then subjects participate in one of three sessions. In each session, we select one of the charities and conduct a video campaign. After participants see the video campaign, we "challenge" subjects to give to that charity. Participants are then asked to make the same decision as the baseline, allocating money between themselves and the three charities. By comparing the subjects' allocations before and after the campaign, we measure the effect of the campaign on giving, both to the target of the campaign and the other two charities. We conjecture that if crowding out occurs at the basic level in the lab, then the effects will be exponentially larger in the field. This is because charities would use campaigns that employ methods that would increase participation such as social media. The crowding out in the controlled lab experiment will not be able to capture the network and social pressure aspect. Using this experimental design, we investigate the question: When a charitable organization conducts a successful campaign, where do the donations come from?

To answer this question, we must first create a successful campaign. To verify this goal, we test if the subjects significantly increased contributions to the charity that ran a campaign. We do succeed in running a successful campaign for the charities. We see an increase in giving to the targeted charity across all sessions. To examine where the increase

in contributions came from, we measure how overall giving was impacted. By comparing total donations in the baseline and treatment, we can identify if individuals increased their charitable budget, or if they reallocated funds from other causes. In all three sessions, we find total giving remained unchanged in the treatment. That is, the increase in giving to the target charity came entirely at the expense of the other charities. This finding provides initial evidence of a crowding out effect.

In a Study 2, we investigate the effects of priming general charitable behavior. Using the same design as before, subjects allocate money between themselves and the same three charities. However, subjects do not see a targeted video campaign about a particular charity; instead, they see a video highlighting general giving. By priming general giving behavior, we observe if this generates an increase in total donations. Looking at the sum of individual donation in the baseline and treatment we find that subjects do not increases their total giving significantly. Priming general philanthropic behavior does not change subject's donation behavior significantly.

3.2 Literature Review

To our knowledge, there have been no published works on our particular research question. There have been many experiments involving charitable giving, but none involving the crowding out effects on net charity contributions because of a charity campaign. Much of the charitable giving literature investigates mechanisms that enhance giving. A number of papers have investigated matching and rebates as methods for increasing donations to charitable causes (Eckel and Grossman 2008a, Eckel et al. 2005, Eckel and Grossman 2003, Karlan and List 2006, Karlan and List 2011, Meier 2007, Chen,

Li and MacKieMason 2006, Meer 20014, Huck and Rasul 2011, Rondeau and List 2008). This literature finds that matching donations increases overall contributions and that matching tends to outperform rebates with regard to increasing donations. Charities can offer matching to increase contributions but it is not clear if the increases come at the expense of other charities. If individuals have a giving budget, an increase in one contribution should come at the expense of another planned contribution.

Similarly, social information and leadership have been explored as a method to increase contributions (Frey and Meier 2004, Croson and Shang 2008, Shang and Croson 2009, Gneezy, Nelson and Brown 2010, List 201). This literature indicates that information about other's donation behavior increases contributions and has merit to the individual's donation decision. There is literature showing seed money is beneficial to increase fundraising (List and Lucking-Reiler 2002, List 2011, Andreoni 1998). Another strand of research looks at campaigns effects on future giving. If individuals have an opportunity to donate, and never be solicited for another donation, contributions increase. Furthermore, individuals do not choose to opt out of future solicitations. Just providing people the ability to opt out increases the number of donors and the amounts they give to the cause (Mullaney et. al 2015). Impacts of future charitable giving are examined when charitable causes offered matching for donors (Meier 2007). Although multiple ways have been studied on how to increase donor giving, no research has explored where the increase in donations are coming from. There is research addressing what happens to future donations within the same organization, but a large hole exists in examining future impacts on other charitable causes.

Researchers have also observed the phenomena of multiple years with a stagnant percent of GDP being directed to charitable causes (List 2011, Perry 2013). In the United States, individuals spent a total of $358.38 billion dollars in 2014 on donations to charitable causes (CNBC, 2015). This amounts to two percent of income as a percentage of the US GDP. The percentage that Americans give to charities has remained flat over the last 40 years (List 2011, Perry 2013). This suggests that Americans are not increasing total charitable contributions as a percentage of income. Thus, there is initial empirical evidence supporting our hypotheses regarding crowding out. This stagnant rate of giving indicates that if a campaign is run, it might not cause total charitable contributions to fluctuate.

3.3 Experimental Design

3.3.1. Study 1

In Study 1, our experiment is a within-subjects design, where each subject participated in both the baseline and the treatment. The baseline game is a dictator game with charity recipients. Subjects are given a $15 endowment and the opportunity to allocate the funds between themselves and three unique Texas charities. The only restriction is that donations plus the amount kept by the subject must add up to $15, and any money contributed must be in whole dollar increments. In all three sessions, the subjects were endowed with the same budget ($15) and made the same decisions as before. However, before they made their allocations, the subjects were shown a short video about one of the three charities. This short video is a charitable campaign, and is the main intervention we use in the experiment. In the video, an undergraduate student relays

relevant information about one of the three charities, including information and statistics about the organization and their success in helping their unique cause in Texas (See Appendix C). All three videos provided similar information about the targeted charity and the language in the videos is taken from the charity's websites. The information and data about each charity was collected from the charity's official website. The order in which we assigned the charities to be targeted was chosen at random before any experimental sessions.[6] We ran three sessions, one for each charity. This video was played on a central projector in the front of the room after the subjects completed the baseline decision. Before the subjects saw the video, the lead experimenter made a short speech getting the subjects attention and letting them know an important message would follow. After the video was played, the experiment challenged the subjects to give now. The allocation decisions in the treatment are the same as in the baseline.

After the subject completed both the baseline and the treatment, the student monitor was brought to the front of the room and one of the two decisions was chosen for payment by a die roll.[7] If the monitor rolled a 1, 2 or 3 subjects were paid for the baseline decision. If a 4, 5 or 6 were rolled the treatment decision was paid. This was announced to the participants. Subjects were paid the remainder of their endowment not donated (and a $5 show up fee); donations to the charities were totaled and donated online. While the experimental team organized payment, the subjects filled out a brief demographic survey

[6] Using a random draw without replacement, and before any sessions were run, the order was decided for all sessions.

[7] The monitor was chosen at random at the beginning of the experiment. She/he did not make allocations and received $20 for their participation.

(see appendix D). The survey included questions about charitable giving behavior and the Self-Report Altruism Scale (SRA) (Rushton, 1981). When subjects finished the survey, they were taken to the payment window where they received their earnings in person privately.

The subjects in this experiment were at no point forced to give or donate to any of the three charities. The experimenter speech and informational video in the treatment of each session was used to simulate the social pressure often inherent in a non-profit's giving campaign. This allowed the video campaign to simulate effects of a non-profit charity's fundraising campaign on donations to other non-profit charities.

Procedure

Our experiment had 67 individuals participate across three sessions. All subjects were recruited using ORSEE (Greiner, 2004) In each session, we ran a campaign for one of the three charities. Table 11 summarizes the distribution of subjects in each session.

Table 11: Subject Breakdown by Session

Session	Treated Charity	Subjects
1	Feeding Texas	24
2	Texas Campaign for the Environment	22
3	Operation Kindness	21
Total		67

All sessions were run at the Economics Research Laboratory at Texas A&M University in November 2014. All subjects received a $5 for showing up. We restricted participation to only undergraduate students who had not participated in charitable giving experiments before.

When subjects arrived to the Economic Research Laboratory, they checked in and received subject identification numbers and waited until the session began to proceed into the lab. The subjects received the experimental materials (see appendix D) upon entering the lab, which included experimental instructions, charity information and decision sheets. After reading the basic instructions of the experiment (see appendix D), the experimenters chose one participant at random to act as the monitor; their duties entailed ensuring experimenters followed all protocols as while checking for accuracy and donation to the charities. The monitor was brought to the front and given a protocol sheet that they had to sign off on before receiving their payment of $20 including the $5 show-up payment. Average earnings were $14.09[8] with $5.91 being sent on average to all three charities. The monitor oversaw the payment of subjects as well as the payment online to the three charitable organizations for the decision (baseline or treatment) that was selected for payment.

Charities

We selected a set of charities that would appeal to our college-student subjects; one charity that dealt with animals, one that dealt with hungry Texans and finally a charity

[8] Including the $5 show-up fee.

that worked with the environment. We chose charities at the state level, to ensure no local charities were chosen that could be favoured or better known by our subject pool. The charities that were chosen are listed below:

Operation Kindness (OK) is a nonprofit no-kill animal shelter in North Texas. Established in 1976 OK is the largest no-kill shelter in the state. This charity helps with the homing of misplaced and surrendered pets through shelters and fostering animals.

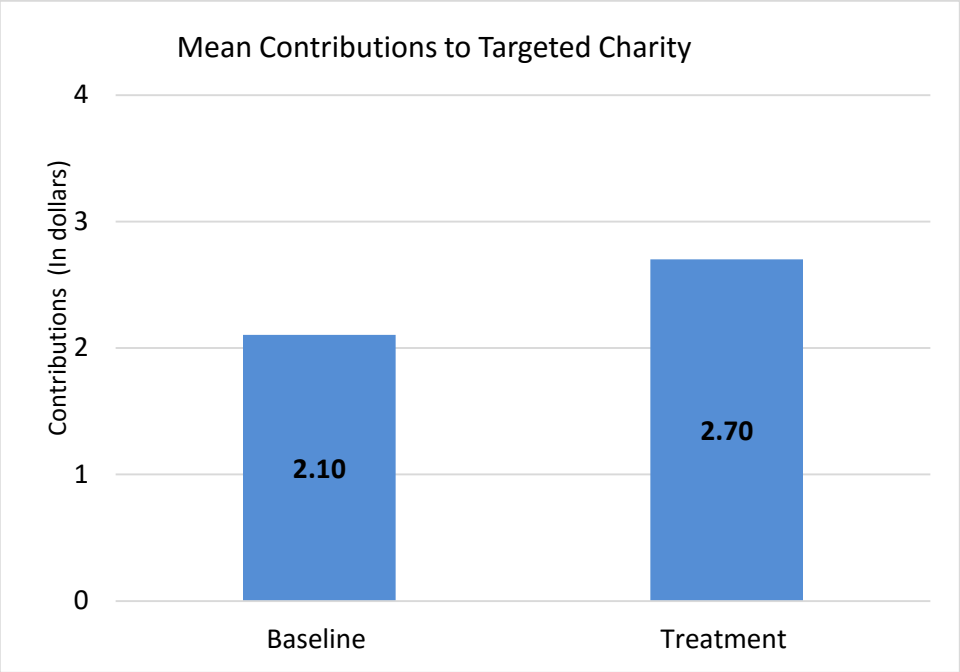
Feeding Texas (FT) is a nonprofit charity whose mission is to alleviate hunger in Texas. This charity was formerly the Texas Food Bank, and the charity uses multiple food banks to help solve hunger.

Texas Campaign for the Environment (TCE) is a nonprofit charity that focuses on educating and informing Texans about the environment. They also focus on pollution and public health in Texas. It works in both local policy and state policy areas trying to make Texas environmentally better.

3.4 Results

To identify whether the media campaign was successful, we observe the contributions to the targeted charities before and after the campaign. Figure 6 shows mean contributions to the target charity, with all three session pooled together. In the baseline decision, the mean contribution to the targeted charity was $2.10. After subjects were exposed to the campaign, the contributions to the targeted charity increased to $2.70.

Figure 6: Mean Contributions to Targeted Charity



The baseline contributions are significantly different from the treatment contributions. (p=.0113). We confirm this result by observing the contributions for the subsample of subjects who contributed to the charity, leaving out subjects who kept all funds for themselves in both decisions. Our result is consistent when we only consider individuals who made transfers to the charitable organizations (See appendix B, Figure B.1). The result is also robust with regard to contingency giving. This indicates that the result is not being driven by new donors that are entering the charitable market after the campaign. The levels of increase depend on the charity themselves. In Table 12, we report the p-values from a t-test for mean contributions to each charity before and after the treatment. The shaded boxes indicate the targeted charity for each session. We see a

significant difference in two of the three charities. All charities saw an increase in donations[9].

Table 12: *P*-Values for Mean Contributions Before and After Treatment

Charity	Session 1 (FT)	Session 2 (TCE)	Session 3 (OK)
FT	*P=0.0650+*	*P=0.1339*	*P=0.2803*
TCE	*P=0.2127*	*P=0.5758*	*P=0.1048*
OK	*P=0.0412**	*P=0.4064*	*P=0.0812+*
P values for t-test of means between control and treatment. *** p<0.001, ***p<0.01, *p<0.05,+p<0.10			

In Table 13, we report results from a panel regression. The dependent variable is individual contributions to the targeted charity. Each individual has two observations, one for the baseline decision and one for the contribution in the treatment. In Model 1, we see that post-campaign individuals sent $0.597 cents more than in the baseline on average. In Model 2, we control for gender, age and weekly spending. The results remain robust with individuals sending $0.591 more. We do not find any significant demographic indicators for increased contributions.

[9] The individual contribution differences are reported in Graphs A3, A4, and A5 in the appendix. Here they are shown by sum contribution as well as mean level contributions to each charity by session.

Table 13: Panel Regression on Amount Sent to Targeted Charity

Panel Regression: DV = Giving to Target Charity

Model	I	II	III
Post-Campaign	0.597**	0.591*	0.591*
	(0.23)	(0.24)	(0.24)
Female		0.481	0.474
		(0.60)	(0.59)
Age		0.359	0.371
		(0.35)	(0.35)
Weekly Spending		-0.184	-0.17
		(0.20)	(0.20)
Donations Last Year			-0.025
			(0.58)
GSS Altruism			-0.163
			(0.58)
Constant	2.104***	1.39	1.448
	(0.29)	(1.04)	(1.21)
Observations	134	132	132
Number of individuals	67	66	66

Robust standard errors in parentheses, clustered by individual. *** p<0.001, ** p<0.01, * p<0.05, + p<0.10

In Model 3, we use additional controls including a dummy variable that takes on a value of one if the subject donated money to a charity within the past year. We find no significant correlation between past giving behavior and giving in our experiment. We also use the GSS altruism measure[10] and find no significant correlation using this standard survey altruism method. We do see that the result of an increase of .591 cents remains consistent even after controlling for all the variables.

Next, we identify where the increase in funds came from. Subjects gave 39 percent of their endowments to charities in the first decision. This indicates that the mean for total giving was $5.89 out of a $15.00 endowment in the baseline. After the video campaign, subjects gave $5.87, still 39% of their available funds. Figure 7 offers a graphical representation of the unchanged giving budget. The amounts are indistinguishable from each other (p=0.90).

We find the subjects donate the same total amount in both the baseline and treatment. This indicates that total giving was unchanged due to the campaign. If a successful campaign had increased an individual's donation budget, we would have seen mean treatment giving greater than baseline contributions. Instead, we find that subjects are reallocating funds from another charity after exposure to the video campaign. The increase in giving to the targeted charity comes entirely from reduced donations to other charities.[11] In Figure 8, we test the difference in the distributions of contributions in pre-

[10] See Appendix D for GSS altruism question. These were given as part of the post survey.
[11] The individual variations between sessions can be seen in Appendix B, figures: B.3, B.4, and B.5

campaign and post campaign decisions. If new donors were entering the market, the contribution would see a shift to the right and the distributions would be different. The distributions are indistinguishable from each other using a Kolmogorov-Smirnov test (p=1.0000). The amount that individuals are sending to the charities are distributed the same before and after the video campaign. This provides solid evidence that subjects are just reallocated the money that they donated in favor of the targeted charity. This result indicates that the extensive margin behavior is unchanged.

Figure 7: Mean Total Giving to All Charities

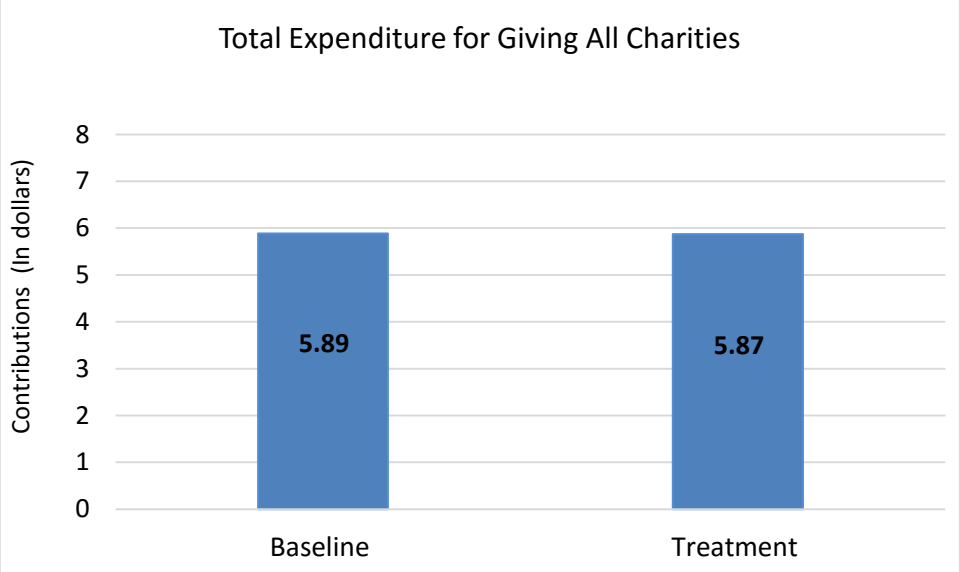
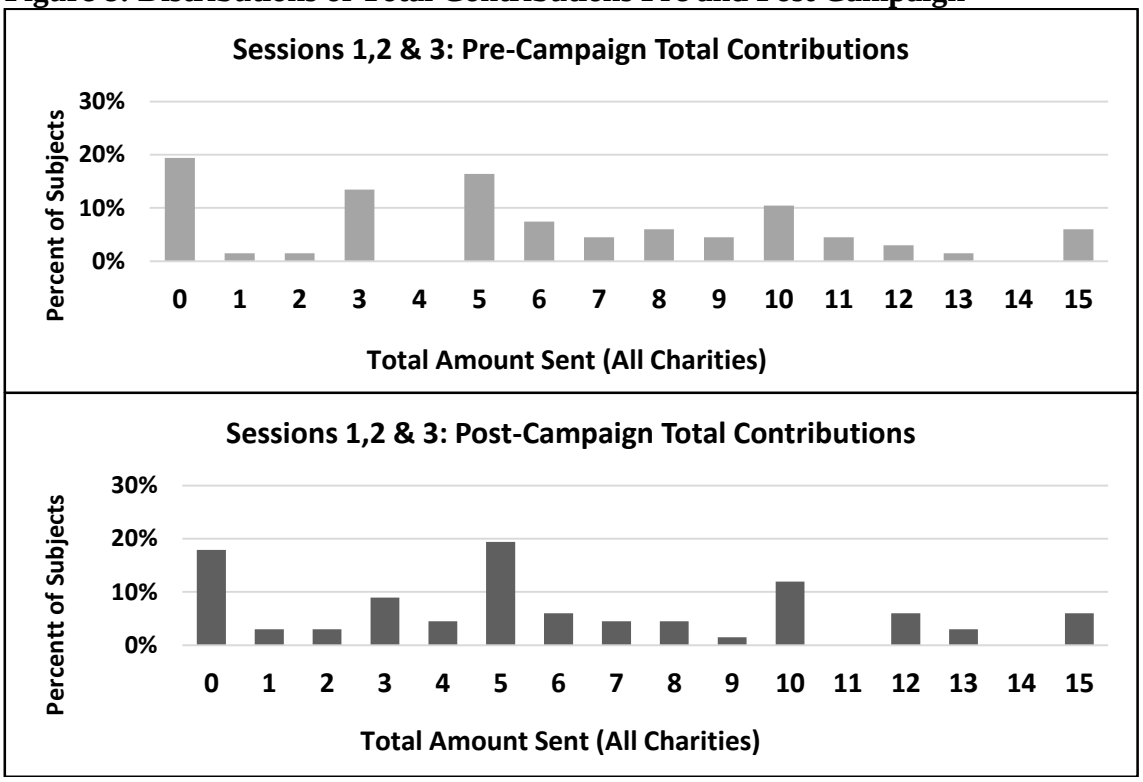


Figure 8: Distributions of Total Contributions Pre and Post Campaign



We run a panel regression to confirm that the charitable budget is unchanged. By using similar models as before, we can identify whether the result is being driven by any particular demographic characteristic, past charitable behavior, or altruism. Table 14 offers results from a panel regression. The dependent variable is total contribution. We find that post-campaign the budget decreases by $0.03, although the decrease is not significant. This result proves robust when adding in demographic information (Model 2) and previous donation and altruism (Model 3). This result holds when looking at each session separately. We see all target charities get an increase in contributions from the media campaign. In each session, the increase comes at the expense of the non-targeted charities.

Table 14: Panel Regression on Total Amount Sent to All Charities

Panel Regression: DV = Total Contributions			
Post-Campaign	-0.0294	-0.076	-0.076
	(0.24)	(0.24)	(0.25)
Female		1.421	1.432
		(1.09)	(1.09)
Age		0.718	0.753
		(0.45)	(0.47)
Weekly Spending		0.004	0.041
		(0.43)	(0.44)
Donate Last Year			0.184
			(1.18)
GSS Altruism			-0.324
			(1.25)
Constant	5.896***	3.389*	3.138
	(0.54)	(1.60)	(2.45)
Observations	134	132	132
Number of id	67	66	66

Robust standard errors in parentheses, clustered by individual. *** p<0.001, ** p<0.01, * p<0.05, + p<0.10

3.5 Study 2: Design and Results

After running the first three sessions (OK, TCE, FT), we established that individuals have a set budget of funds they allocate to all charitable causes. The motivation behind this design was that a general campaign would reduce the apparent tradeoffs between the three charities. Ultimately, we wanted to see if priming general charitable behavior would increase total giving.

For the robustness check, we created a fourth session based on the same design as before. In each of the two periods, they again decide how to allocate 15 dollars among themselves and three charities. In the second period, we use a different intervention. Instead of using a video targeting a specific charity, we prime general giving behavior using a Texas A&M service project, The Big Event.

The Big Event is where students do service projects for the community for one day in the spring semester. These projects can include raking leaves, painting houses, building fences, volunteering at an animal shelter, building handicap ramps, and many other various projects. In 2014, 21,000 students participated in the event. With over 43 percent of the student body participating, this indicates that the cause is widely known throughout the campus. The Big Event has the mission to promote campus and community unity. This particular service project is the largest one-day student run service project in the world. Due to the success, it has been expanded to over 110 other schools worldwide. The treatment we offered subjects was a video, the same as before, but with information about the Big Event and the plethora of things, it does for the community.

The second study was run in July and August 2015 at the Economic Research Lab at Texas A&M. Average earnings for subjects were $14.07, including a show-up fee, with an average payment of $5.93 sent to charity. We had 42 subjects participate in these sessions.

In Figure 9, we see the total contributions for both the first three sessions and the fourth session (Study 2). Directionally we see an increase under the general charitable campaign, although it is not significant (p=0.172). Subjects sent $5.76 in the baseline compared to $6.12 after watching the video informing them of the Big Event.

Considering Study 2 was run eight months later, we want to verify that the behavior we observe is the same as the previous three sessions. In Table 15, we report the p-values for Study 1 (sessions 1, 2 and 3) against Study 2 with regard to pre-campaign contributions, post-campaign contributions as well as change in contribution. We find no significant differences in charitable contributions in the first decision before any video was played. This is also true with total giving levels after the videos. We find no difference in the behavior of subjects in Study 1 or Study 2. Finally, we observe the difference in giving pre-campaign and post-campaign in both studies. We find no difference in the deduction or increase in contributions between the two studies.

Figure 9: Study 1 and Study 2 Mean Total Giving to All Charities

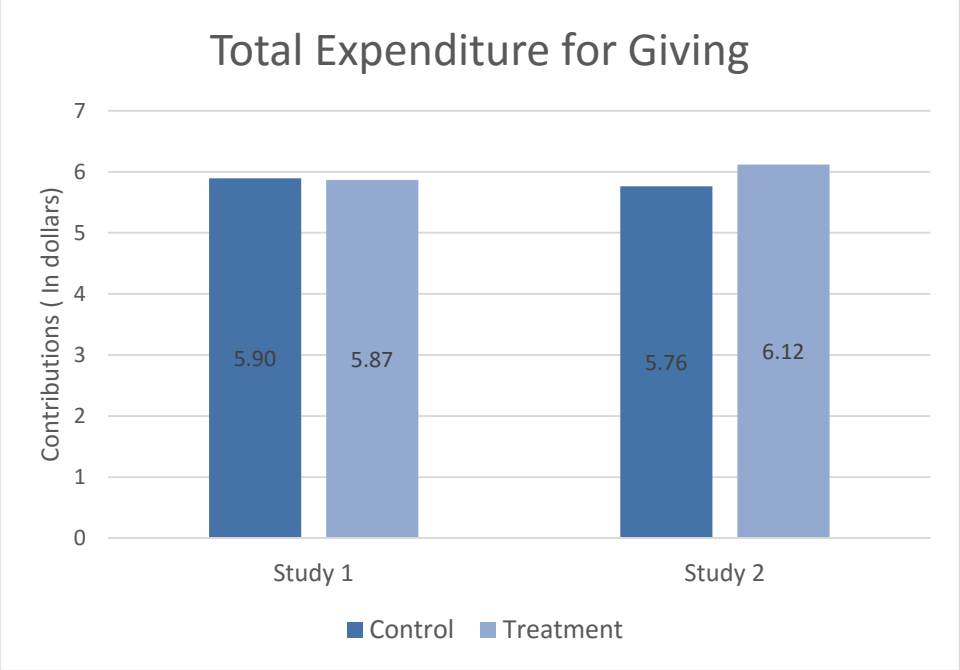


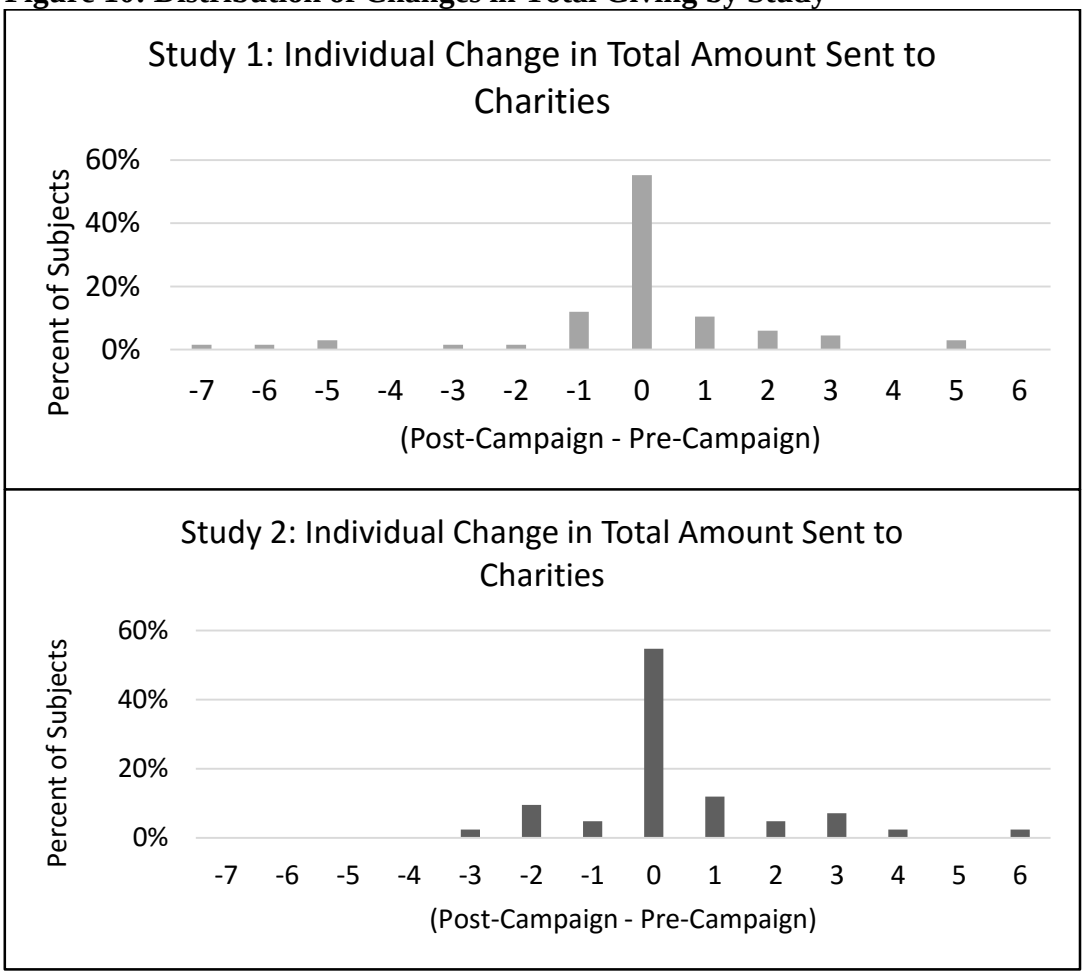
Table 15: T-Test Study 1 versus Study 2

	Study 1 vs. Study 2
Pre-Campaign Giving	*p=0.872*
Post-Campaign Giving	*p=0.770*
Change in Contribution	*p=0.292*

Figure 10 shows the distributions of change in donations in the first three sessions compared to the distribution of the change in donations in the fourth session, study two. Using the Kolmogorov-Smirnov test we find no significant difference (p=1.0000). This indicates that individuals in all four sessions had similar behavior in reallocation before and after the media campaign. Examining study 2, we conclude that running a general

charitable campaign does not increase contributions to the charities significantly. This strengthens the original finding that individuals have a charitable budget, and successful campaigns that target a specific charity create substitution between charitable causes.

Figure 10: Distribution of Changes in Total Giving by Study



3.6 Discussion

In our lab experiment, we find significant crowding out of donations from non-target charities. We find people are not increasing their total charitable expenditure after receiving a video campaign. Thus, the total philanthropic reservoir (charity budget) remains the same before and after a successful campaign. This is consistent with historic data, which illustrates that charitable giving, as a percent of GDP, has remained consistent at 2 percent over the last four decades in the United States. The experimental media campaign increased giving to the targeted charity; but did so at a cost to the fundraising efforts of others. Subjects reallocated funds from other charities to the targeted charity, indicating that donors viewed the charitable causes as substitutes. We also find priming for general charitable behavior does not increase giving. We fail to find a manipulation that increases subjects' charitable budget.

Our experiment confirms an interesting phenomenon. Individuals are redirecting their charitable expenditures and crowding out giving to others. This is an important factor to consider when designing campaigns to increase charitable gifts. When a charity, like the ALS foundation has a successful campaign, increasing donations by over $100 million dollars, this surge in money likely comes at the expense of other organizations. Extrapolating our lab results to the field, it is likely we would discover that other charities contributions suffered at a magnitude similar to the success of the ALS campaign.

Ultimately, charities should consider the crowding out effect when organizing their next campaign. The timing and effectiveness of a charity's campaign could depend largely on the efforts of other organizations. Considering our research indicates donors have strict

charitable budgets, it could be beneficial to make sure that you run a campaign early in the fiscal year, to make sure you achieve some potential donors giving budget. Charities that run successful campaigns could be doing so at the expense of other causes. For instance, the ALS surge in donations could have drained an organization that works towards a cause affecting a larger proportion of the population such as hunger or cancer. In turn, this creates a moral hazard in social media campaigns that could in turn actually harm society indirectly. The fact that we were able to demonstrate these results in the laboratory means that the effects of a peer influenced, social media campaign outside of the laboratory would likely to be greater and more significant. Typically, campaigns can use social influence and networking to increase the effectiveness. This indicates that removing these factors would ultimately make our lab experiment show the lower bounds of possible consequences of a successful campaign. The long-term effects of running a widespread campaign like this are unknown.

3.7 Conclusion

In this paper, we report evidence from a laboratory experiment that examines the impact of a successful targeted media campaign on overall levels of charitable giving. Through our design and analysis, we detect our hypothesized "crowding out effect" on the non-targeted charities. Our results indicate that there is a statistically significant effect of crowding out from non-targeted charities. We find that while overall levels of giving remaining constant, non-targeted charities suffered when compared to their baseline

earnings. We also explore the effect of priming general giving behavior and find no increase in total money allocated to causes.

Future work is needed to examine the impact of the substitution effect on charities when related causes are being targeted. For example, if you run a successful campaign for an animal shelter, how does that campaign affect donations to other animal rescue organizations. This would help us gain a deeper understanding of the substitutability of charities in the eye of the donor. While offering insights into the crowding out we see between unrelated charitable causes.

Our findings come at a time when questions about social media giving campaigns have piqued significant attention in both the press and economic literature. Our experimental work fills a void about effects of campaigning on total charitable expenditures as well as its influence on other charitable organizations. This experiment offers insight to practitioners on the impact that targeted campaigns have on charitable fundraising. Considering the seasonal nature of charitable contributions, one could imagine how our results could have a large impact on running a campaigns and their timing.

4. THE EFFECTS OF PUNISHMENT ON COOPERATION IN SIMPLE GAMES

4.1 Introduction

In standard games, a distinct effect can be seen on cooperation between subjects when social norm mechanisms are introduced. Interestingly, the implementation of the ability of a group to enforce social norms, through punishment, has different effects on the public goods game and on the trust game. In a standard public goods game, the implementation of a punishment mechanism increased contributions and cooperation between subjects in general. In a standard trust game, however, the implementation of a punishment mechanism reduced trust and trustworthiness between subjects.

Public goods games have been used to simulate a public good context in which the dominant strategy is free riding. When subjects are given the option to punish counterparts by decreasing their earnings after public good contributions are revealed, literature shows a significant increase of contributions (Fehr and Gachter 2002). There also appears to be no significant importance whether one person or all persons are given the opportunity to punish (O'Gorman et. Al 2008). Additionally, people will enforce social norms even at a significant cost to themselves, due to either emotional responses or forward thinking long-term strategy (Carpenter 2007). However, contributions are not affected by group size. If a subject is the designated punisher within a group, this leads to an increase in punishment, even when this is not a profit maximizing strategy (Devlin-Foltz and Lim 2008).

By contrast, trust games exhibit results that are antithetic from those of public goods games, when the option of punishment is included. Punishment tends to decrease levels of altruistic behavior such as trust and trustworthiness as seen in trust games. (Fehr

and Rockenbach 2003). Whereas public goods games see increases in contributions with punishment, trust games see decrease in both the trusting behavior of the first mover as well as the trustworthiness of the second mover when punishment is available. In a trust game in which the threat of punishment is used increase trusting behavior, it has been shown that if the option of punishment is available, but not used, trustworthiness increases. However if the threat of punishment is used, trust is low. This means that the lack of punishment as a factor leads to lower contributions (Fehr and List 2004).

There are important distinctions to be made in attempting to explain the disparity between the effects of punishment in the public goods game and the trust game. These two games are often structured differently from one another and it could be for this reason that we see this difference. The aim of this paper is to discover if the structure and design of the games themselves are influencing the disparity in punishment effects. We will explain these two games briefly to understand the structure of these simple incentivized games.

The standard Public Goods game (hereafter PGG) is played in groups (typically 3 or more). Each participant has an endowment and can contribute any portion into a group account simultaneously. Any tokens that the participant does not contribute are places into an individual account with a 1:1 return ratio. The amount in the group account is then multiplied by a certain amount (called the multiplier) and divided equally between all participants in the group. The equal division among all group members leads to an equal redistribution between everyone regardless of the initial contribution. An important design feature of the PGG is that each token contributed to the group account yields less than one token to the contribution individual. Thus, if everyone contributes an equal number of

tokens to the group account, everyone is better off. This creates a socially optimum solution for all players to contribute all tokens into the group account. However, since each token contributed yields less than one token, if all players are profit maximizing and rational, the Nash Equilibrium is that no one contributes any tokens to the group account. Typically, the PGG is repeated over several rounds.

The Trust game (hereafter TG) is played with in groups of two with sequential movement. This game was developed by Berg et al. in 1995. This results in a distinctive first mover and a second mover. This is a distinctive difference from the PGG. In the TG each player has an equal endowment. In the TG the first mover sends a portion of her endowment to the second mover. This amount sent is multiplied by a number greater that 1 (typically 3). The second mover receives the multiplied amount and can choose how much to return to the first mover. The second mover can also choose any of the multiplied monies received as well as their initial endowment if preferred. With the second mover, choosing the reallocation of funds makes the redistribution mechanism have the potential of being unequal. Due to the redistribution, game theory predicts that the second move should not return any amount if they are profit maximizing and rational. This results in a Nash equilibrium that the first mover does not exhibit trusting behavior (sends nothing) and the second mover sends nothing in return.

Table 16: Original Game Structure: Key Differences Between Games

Public Goods (PGG)	**Trust (TG)**
Repeating	One-Shot
Simultaneous Contributions	Sequential Contributions
Equal Redistribution Mechanism	Unequal Redistribution Mechanism
More than 2 players	2 players

To tease apart the opposite effects of punishment in the TG and PGG, I manipulate the games to be as similar as possible to each other. Table 16 identifies the key elements if the standard original games. To do this we manipulate the PGG and the TG to mimic each other as close as possible. The only feature of the design that we did not change was the redistribution mechanism. The reason the redistribution was unchanged is that this ultimately differentiated the games. This results in the games being parallel on all levels other than how the players receive the distribution after contributions. Table 17, identifies the modified games using a 2x2 design. Using a 2x2 experimental design, I can examine the games in both sequential and simultaneous movement. This allows the games to be more like each other with respect to the individual's decision and movement. I also manipulate the number of times the game is played, allowing me to observe the impact of playing the games for one period vs. multiple periods with respect to punishment effects. The expectation is punishment would affect cooperation in both games in the same way once the changes were made and the games were similar. If the changes result in similar

effects of punishment, the results would indicate that the original disparities in punishment effects are due to the structures and designs of the original games.

Table 17: 2x2 Design Matrix

	Sequential Contribution	**Simultaneous Contribution**
One- Shot	Sequential One-shot (Similar to original TG)	Simultaneous One- shot
Repeated	Sequential Repeated (5 Rounds)	Simultaneous Repeated (5 Rounds) (Similar to Original PGG)

Using the four treatments in the design allow an analysis to decipher what is causing the punishment disparities. Allowing punishment in each treatment, I manipulate one factor at a time allowing identification of potential causes of the different punishment effects. After running all combinations, I find that introducing a punishment mechanism increases cooperation in the repeated public goods game as the literature shows. However, in the one-time PGG we do not see an increase in contributions after punishment is available. This is more similar to what we see in the trust game. In the trust game there is never an environment that creates an increase in trust and trustworthiness with the availability of punishment. This indicates that the disparity in punishment effects is not driven by the difference in game design.

4.2 Literature Review

Many researchers have discussed the effects of various changes made, within these two games, on contribution levels. In the TG literature, punishment has been observed under multiple situations. Within the trust literature, it is well known that sanction and

punischment destroy cooperation between players (Fehr and Rockenbach 2003). The negative impacts of such sanctions have been explored in multiple settings. (Fehr and List 2004) conducted a one-shot trust game in which each subject was given the option to punish their counterparts. Each subject was asked to contribute and elect a threshold of acceptable returns from his or her counterpart. If the threshold was not met, costless punishment was inflicted by reducing the earnings of the player that did not contribute the appropriate amount. (Houser et. Al 2008) conducted an investment trust game in which a first mover was asked to contribute and make a request for a particular percentage returned from the second mover. A sanction could be inflicted if the requested percent returned was not met. This sanction could come from the investor (first-mover) or nature (experimenter). The researchers find that punishment incentives affect returns, but not the intentions. They also find that sanctions increase cooperation if the return request are small, but decrease cooperation when the return request are high. The sanctions result in negative effects of sanction id the request are large, ultimately resulting in hindrance.

(Charness et. Al 2008) is, to our knowledge, the only one to successfully encourage higher contributions in a trust game with punishment. Charness's subjects were asked to play a standard trust game in one treatment, and in the other treatment, punishment was allowed. In this game, subjects were allowed to vote for a third party punisher. The result was that each participant, the first mover and the second mover, contributed more because of a third party punisher being present. Third party punishment is different from the design that we are going to implement. Through our design, we will be able to identify if the punishment is as effective coming from one of the participating parties, instead of an

elected third party. If a third party is the punisher than they are able to examine both parties' contributions and reactions to the other parties' contributions or returns. Therefore, third party punishment is quite different considering that they are able to identify both individuals' outcomes and act accordingly, and both parties participating in the game can be punished. In our design, only one party, the first mover will be able to punish.

(O'Gorman et al., 2009) conducted a public goods game in which the number of punishers in a group was manipulated across treatments. Each group consisted of four persons. During one treatment, only one subject out of four was randomly selected to have the option to punish anybody else in the group. During the other treatment, all four subjects had the option to punish each other. The number of punishers in a group showed no significant impact on the total level of contributions. The existence of punishment, as expected, did significantly increase contributions.

Fehr (2002) conducted a repeating public goods game in which a cost of punishment was imposed upon the punisher. In spite of the costliness of punishment, it was found that subjects found it worth the cost to encourage future contributions by group members being punished. (Carpenter 2007) conducted a similar study in which subjects were allowed to punish all other group members in one treatment, and only half of the other group members in the other treatment. The results showed that contributions increased in the first treatment compared to the second.

Given the results of the prior literature, we believe that changing various features of these two games can lead to a change in behavior on the part of subjects. Intuition, and

research, would suggest the following assumptions: rational players will not punish if the game is one-shot, the number of punishers in a group will positively correlate with contributions of the first and second mover. There is, as the literature shows, a difference between observed activity in situations involving trust and situations involving public goods. We believe this difference to be a result of inherent differences in the structures of the two games. The most important difference between the two, we believe, is the sequential nature of contribution turns. In a public goods game, contributions occur simultaneously, whereas they are sequential in the trust game. This can lead to a perception, in the trust game, of a submissive/dominant relationship between the first and second mover, respectively. In the public goods game, it is more reasonable to assume a cooperative relationship between players. For this reason, we believe that this dominant position of the second mover in the trust game is a reason for the decrease of contributions with the aspect of punishment. From an intuitive standpoint, the withholding of contributions can be seen as a proxy for control. To take control away from a subject might lead to a decrease in contributions, as he or she attempts to retain control of the game. If one individual changed their contributions from what they would normally contribute to give them an advantage versus the other player, than it could be said that the desire to have control influenced the contribution on this player's behalf. This idea lends itself to a potential explanation for the enigma of the trust game. When punishment is added, the second mover could interpret this as a loss of control. The natural reaction would be to decrease contributions in order to compensate and retain control of the game. The public

goods game does not have this dynamic, as control of the game is divided between the players whether punishment is an option or not.

We will attempt to make the two games as structurally similar as possible in order to encourage a convergence of contribution trends across the two games. To do this, we will distort the division of control in the public goods game by making turns sequential. This public goods game will include groups of two, as in trust games, and it will be a one shot game.

4.3 Experimental Design

4.3.1. Overview of Experimental Design

The experiments were run at the CBEES (Center for Behavioral and Experimental Economic Science) lab at University of Texas at Dallas and at the ERL (Economic Research Lab) at Texas A&M University. The sessions were run in July 2010, October 2014, and June 2015. There are 294 subjects in total. The average earnings over all sessions were approximately $16 dollars. All subjects received a $5 dollar show up fee for coming to the lab on time. This is included in the total payment. All sessions were computerized and programed in z-tree (Fishbacher 2007). In Table 18, you can see the breakdown of the experiments by laboratory and date. All one-shot sessions included one TG without punishment, one TG with punishment, one PGG without punishment and one PGG with punishment. The order was blocked with respect to TG and PGG. However, the punishment was always after the no punishment version. This allows us to make sure there are no order effects due to the order of the games. In the repeated versions, subjects played 20 rounds. This included 5 TG without punishment, 5 TG with punishment, 5 PGG

without punishment and 5 PGG with punishment. Again, the order of the first game was blocked between PGG and TG, with punishment always following the non-punishment version of the game. Table C.1 in Appendix C list the order of experimental sessions for both one-shot and repeated. In the repeated version of the task, subjects kept the same partner for each task (5 rounds). The pairs were rematched between each task (no punishment and punishment). They did not know who their partner was for any round.

After subjects completed the PGG and TG they completed an Eckel-Grossman risk elicitation measure and a survey (see Appendix Figure C.1). Sessions lasted on average slightly over 90 minutes. The repeated games took longer than the single shot games.

Table 18: Experimental Design Information by Treatment

	Sequential Contribution	**Simultaneous Contribution**
One- Shot	Conducted at CBEES lab at UTD July 2010 Sessions of 12 people* Average earnings $16 Time≈ 1 hour N=112	Conducted at ERL lab at TAMU October 2014 Sessions of 24-26 people* Average earnings $18 Time≈ 1.5 hour N=50
Repeated	Conducted at ERL lab at TAMU June 2015 Sessions of 24 & 28 people * Average earnings $15 Time≈ 1.90 hour N=52	Conducted at ERL lab at TAMU June 2015 Sessions of 16, 18, & 18 people* Average earnings $15 Time≈1.70 hour N=52

Subjects completed a total of 5 task in each session. We choose one of the five task at random by having a random subject pull a chip from a bag. We then paid everyone for the corresponding selected task. In treatments with repeated versions of the games there was an exchange rate of 15 tokens= $1 and the exchange rate was 3 tokens=$1 in the one-shot version.

4.3.2. Trust Game (TG)

In the modified trust game, we have groups of two players. Each player receives an initial endowment of 30 tokens. In the sequential version of the trust games Player A sends some, none or all the tokens to Player B. Any tokens that are transferred to Player B are multiplied by 3. After the transfer, Player B has the transferred tokens multiplied by 3 plus their initial 30 token endowment. They can choose to send any amount back to player A. Any amount Player B returns will not be multiplied. In the simultaneous treatments, Player A chooses discreet amounts in five token increments to send to Player B (0, 5, 10, 15, 20, 25, and 30). While Player A is making the decision on the amount to send to Player A, Player B is using the strategy method and making a decision on the amount to return for all possible amounts that Player A can send. This allows the decisions to be simultaneous. Punishment in the TG happens in a third and final stage of the game. Player A can punish Player B. The cost of punishment is 10% of the initial endowment (3 tokens), and it results in a deduction of 50% of the endowment (15 tokens) for Player B.

4.3.3. Public Goods Game (PGG)

In our modified PGG, we use groups of two players. Player A and B both receive an initial endowment of 40 tokens. In the simultaneous version both players select how

many tokens to send to the group account. Any tokens not sent to the group account are placed in their own individual account with returns of 1 to 1. All tokens sent to the group account are multiplied by 1.5, divided even, and returned to both group members. A multiplier of 1.5 creates a marginal per capita return of .75 for each token individually. In the sequential PGG, Player A makes the initial allocation into the group account. Player B can see the Player A's allocation and then chooses how much to contribute to the group account. Again, the money is multiplied and divided evenly. Punishment in the PGG happens in a third stage. Player A can punish Player B. the cost of punishment is 10% (4 tokens) and the sanction cost Player B 50% of their endowment (20 tokens).

4.4 Theoretical Payoffs

In the following we first describe the payoffs in the public good experiment without punishment and then with punishment. The equations are designed for one-shot games, with sequential movement. However, to expand the payoffs to repeated or simultaneous, you could easily extend the profit functions. These theoretical payoffs look at the most basic version. In each period both of the n subjects in a group, consisting of subject i and subject j, receive an equal endowment of y tokens (for our experiment n=2 and y=40) A subject i can either keep these tokens for him- or herself or invest g_i tokens ($0 \leq g_i \leq 40$) into a group account. The monetary payoff for each subject i in the pairing is given by

$$\pi_i = y - g_i + \alpha (g_i\ g_j) \qquad (3)$$

In the public good experiment without punishment, where α is the marginal per capita return from a contribution to the public good. The α in our experiment is equal to .75. The decisions about g are made sequentially. Each decision period consists of two stages. At the first stage, subject i must decide his or her contributiong_i. In the second stage, the subject j is informed about i's contribution g_i and may then decide his or her contributiong_j.

The major difference between the no-punishment and the punishment conditions is the addition of a third decision stage after the second stage of contribution decisions. At the third stage, subject i is given the opportunity to punish subject j after he or she is informed about the contribution of subject j. Subject i can punish subject j by assigning a so-called punishment point p_j to j. If subject i decides to assign this punishment point, it will reduce the second stage payoff of subject j, π_j, by 50 percent of subject j's endowment of y such that p_j = .5(y) (or 20 tokens). However, the second stage payoff of subject j can never be reduced below zero. Subject i has only the option to punish or not to punish subject j. Only one punishment point is allowed to be assigned to subject j if punishment is chosen. The cost of punishment for subject i is equal to 10 percent of subject i's endowment of y such that p_i = .1(y) (or 4 tokens). The pecuniary payoff of subject i and subject j in the punishment condition of the public game can be given, respectively, as π_i andπ_j, and can be written as

$$\pi_i = y - g_i + \alpha\ (g_i\ g_j) - p_i \qquad (4)$$

$$\pi_j = y - g_j + \alpha\ (g_i\ g_j) - p_j \qquad (5)$$

The total payoff from the public goods periods, both punishment and no-punishment, is the sum of the payoffs from all periods.

We will now describe the payoffs in the trust experiment without punishment and then with punishment. The trust experiments are very similar to the public good experiments. In each period both of the n subjects in a group, consisting of subject i and subject j, receive an endowment of y tokens (for our experiment n=2 and y=30). A subject can either keep these tokens for him- or herself or contribute g_i tokens ($0 \leq g_i \leq 30$) to subject j. In the simultaneous version of the TG (g_i = 0, 5, 10, 15, 20, 25, 30). Any tokens sent to subject j are multiplied by multiplier a. For our experiment α =3. Subject j is then informed about subject i's contribution g_i and then can either keep these tokens or contribute g_j tokens ($0 \leq g_j \leq (y + g_i)$ back to subject i. In the simultaneous version, the subject makes a decision for all seven possibilities. However, the amount they can return in each case is identical using the discreet amounts available. The monetary payoff for subjects i and j, respectively, is given by

$$\pi_i = y - g_i + g_j \quad (6)$$

$$\pi_j = y + \alpha g_i - g_j \quad (7)$$

Where α is a multiplier applied when subject i makes a contribution g_i to subject j. As in the public good experiments, the major difference between the no-punishment and the punishment conditions is the addition of a third decision stage after the second stage of contribution decisions. At the third stage, subject i is given the opportunity to punish subject j after he or she is informed about the contribution of subject j. Subject i can punish

subject j by assigning a so-called punishment point p_j to j. If subject i decides to assign this punishment point, it will reduce the second stage payoff of subject j, π_j, by 50 percent of subject j's endowment of y such that p_j = .5(y) (15 tokens). However, the second stage payoff of subject j can never be reduced below zero. Subject i has only the option to punish or not to punish subject j. Only one punishment point is allowed to be assigned to subject j if punishment is chosen. The cost of punishment for subject i is equal to 10 percent of subject i's endowment of y such that p_i = .1(y) (3 tokens). The pecuniary payoff of subject i and subject j in the punishment condition of the public game can be given, respectively, as π_i and π_j, and can be written as

$$\pi_i = [y - g_i + g_j] - p_i \qquad (8)$$

$$\pi_j = [y + \alpha g_i - g_j] - p_j \qquad (9)$$

The total payoff from all four conditions is the sum of the period-payoffs, as given in (3), (4), (6) and (8) for subject i and (3), (5), (7) and (9) for subject j.

Though these two games are very similar, there are a few distinctions worth noting. In a Public Good game, the roles of the two subjects are parallel. Though their turns are sequential, both subjects contribute tokens to a group account. The tokens are then distributed back to the subjects evenly, regardless of their contributions. The Trust game involves roles that are more distinct. The first subject contributes tokens to the second subject. The second subject then has control over the distribution process, having the opportunity to contribute as much or as little as he or she would like. Thus, the distribution process in the Trust game is defined as g_j whereas in the Public Good game the distribution is predefined as $[g_i + g_j]/2$.

4.5 Results

4.5.1. Sequential, One-Shot

The first treatment of the game we will investigate is the sequential one-shot game. This game structure is identical to the standard trust game. The games are two-players, played one time, and the decisions are made in sequential time (player 1 moving first followed by player 2.) For this reason, we do not expect punishment to have different effects on the trust game than what is seen in the literature. However, by manipulating the PGG into this setting we can identify if the sequential game, being played once effects punishment outcomes. In this treatment, we are identifying if changing the public goods game to mirror the structure of the TG alleviates the effectiveness of punishment from a typical PGG. By examining the mean contributions in Table 19, in both the TG and PGG, we can see no increase in trust or trustworthiness from punishment in the standard TG. However, we do see a significant increase on Player A's contribution to the PGG. However, when considering both Player A and Player B's contributions, punishment does not increase the amount contributed to the public good overall.

Table 19: Mean Contributions Sequential, One-Shot

	Trust Game		**Public Goods Game**	
	Player A: Trust	**Player B: Reciprocity**	**Player A: Contribution**	**Player B: Contribution**
No Punishment	16.01 (1.36)	22.30 (2.63)	24.70 (1.67)	23.38 (1.91)
Punishment	16.55 (1.32)	21.16 (2.41)	26.75 (1.78)	23.11 (1.99)
Mean Contribution with standard errors in parentheses				

First, I will observe the TG in more depth. Figure 11, shows the tokens that Player A transferred to Player B. The blue bars indicate transfers in the cases of no punishment and the red bars when there is a punishment option available. Subjects assigned to the role of Player A behave very similar to the baseline even when punishment is a possibility. If punishing were increasing the trusting behavior, the distribution of transfers would shift to the right. In Figure 12, we can see percent returned by Player B after the money sent by Player A had been multiplied by three. On the left side of the plot is the reciprocity exhibited by Player B when no punishment is available. The picture on the right side captures the reciprocity in the punishment setting. Punishment is not increasing trustworthy behavior, much as it did not affect trusting behavior. If the potential threat of punishment from Player A were changing Player B's decision, we would see an increase in the amount returned. Examining the percent returned back to Player A, we see that subjects in the Player B role returned 46 percent when no punishment is available and only 42percent when there is a potential of punishment. This indicates that punishment has no impact on their reciprocity. Overall using the standard trust game design we find that punishment is not increasing trust or trustworthiness in the TG, as the literature has shown.

Figure 11: Player A Trust Sequential, One-Shot

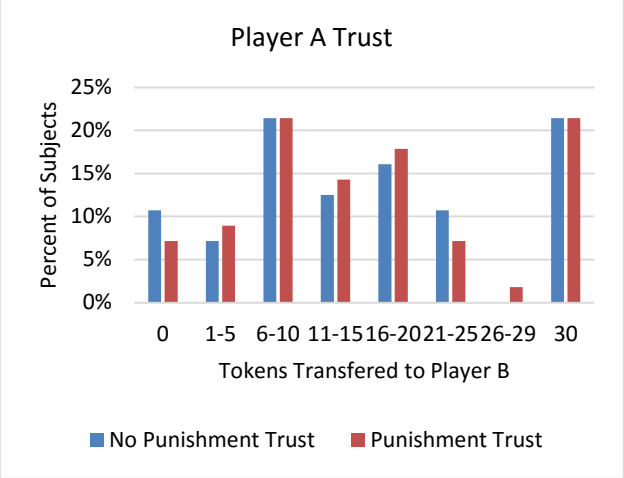
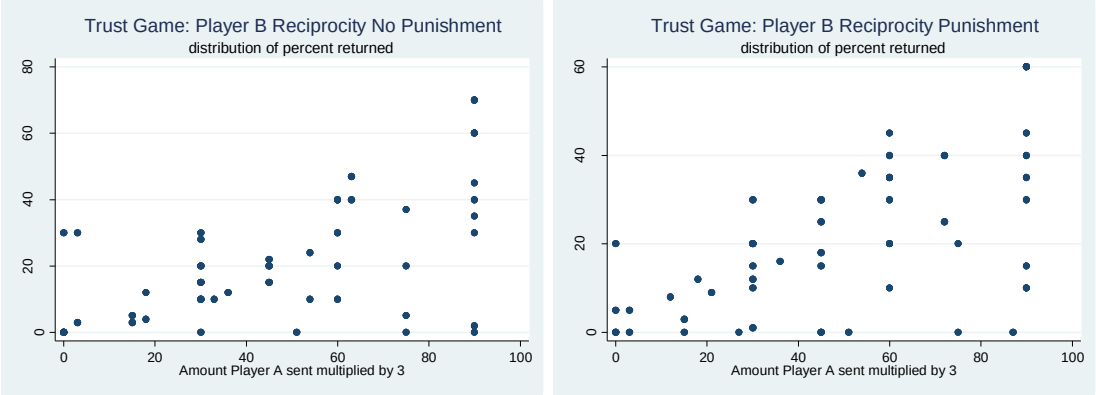


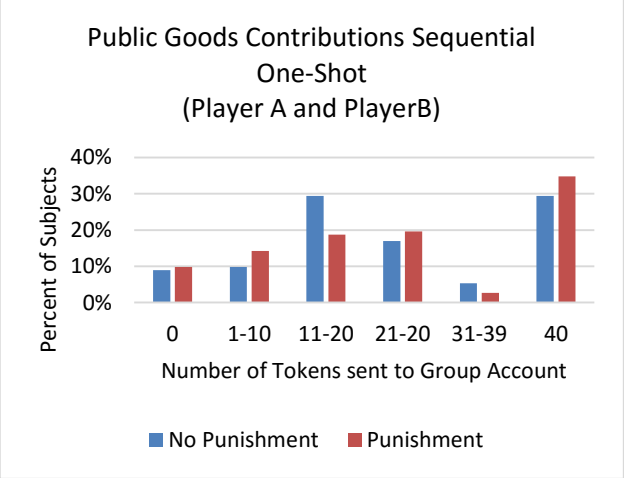
Figure 12: Player B's Reciprocity Sequential One-Shot



Next, we can look more in depth at the impact of punishment in a one-shot sequential setting on the PGG. This is unlike the traditional PGG in that the contributions to the good are happening sequentially. Ultimately, this means that Player A's contribution could have a large impact on how many tokens Player B contributes to the group account. In both the non-punishment and punishment treatments, we see Player B contributing less than Player A. To understand this more we can observe Figure 13 that maps the

distribution of mean contributions of both Player A and Player B. The total average contributions to the PGG went up when punishment was available. The slight increase was derived entirely from Player's A slight increase (not significant) in contribution when they have the option to punish.

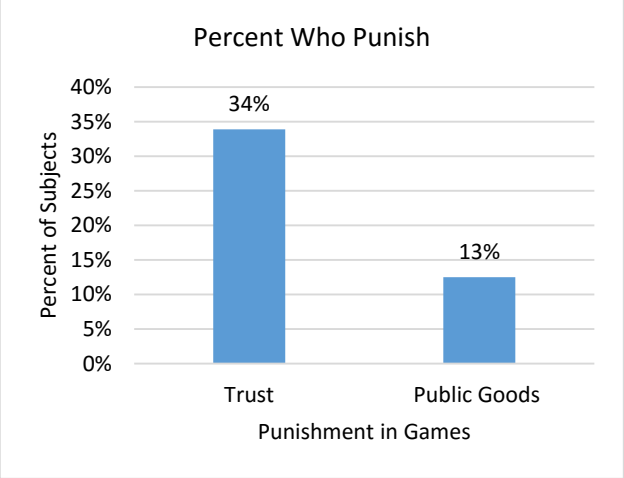
Figure 13: Public Good Contribution Sequential One-Sot



One interesting result is the significant difference is the percent of individuals who punish in the two games. In the TG 34 percent of Player A's punished their counterpart. This is in comparison to punishment being used 13 percent by the same Player A's in the PGG. This indicated that individuals are more likely to punish in the TG in this particular environment. A graphical representation can be shown in Figure 14. This figure shows punishment by Player A's in both the TG and the PGG. The 34 percent punishment rate that we observe in the TG is similar to findings in the standard TG literature.

Manipulating the PGG to be identical to the trust game in all elements other than the redistribution mechanisms indicate that punishment is not effective in a one-shot, sequential environment. It also indicates that punishment is used in the TG significantly more than in the PGG.

Figure 14: Percent Who Punish Sequential, One-Shot



4.5.2. Sequential, Repeated

The second treatment of the game we will observe is the sequential repeated game. This game structure is different from both the standard TG and PGG. This design draws on the repeated environment from the PGG and the sequential decision making from the TG. Using a hybrid of the distinguishing features will allow tease apart and examine if the limited play or the order of decision-making effects contributions. Again, individuals move sequentially, player 1 moving first followed by player 2 (like a traditional TG.) However, these games are each repeated for 5 round of no punishment and 5 rounds with

punishment. This indicates that there is structure designed to impact reputation. Subjects are rematched randomly with another person between each block of five games.

By examining the mean contributions in Table 20, we observe that punishment decreases both trust and trustworthiness in the TG. This indicates that when punishment is available Player A is sending less to player B. Player B is also returning less to Player A. The average percent Player B returns to Player A without punishment is 59 percent. This falls to 56 percent after punishment is introduced. However, the percentage returned in increased compared to the one-shot version of the game. The PGG shows a significant increase (1% significance level) in contributions for both players after punishment is introduced. This finding is similar to what we see in a standard PGG.

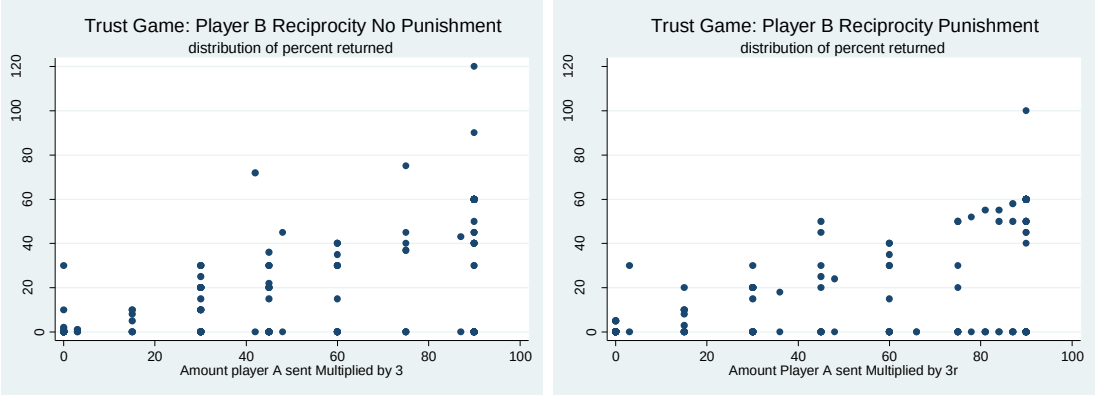
Table 20: Mean Contributions Sequential, Repeated

	Trust Game		Public Goods Game	
	Player A: Trust	Player B: Reciprocity	Player A: Contribution	Player B: Contribution
No Punishment	18.82 (1.00)	33.08 (2.15)	29.97 (1.23)	28.31 (1.34)
Punishment	17.80 (1.08)	29.87 (2.27)	33.27 (1.10)	33.32 (1.08)
Mean Contribution with standard errors in parentheses				

Again in Figure 15, we can see percent returned by Player B after the money sent by Player A had been multiplied by three. The left graph show the no punishment situation; while the right graph is when punishment is available. Punishment is not increasing the amount Player B returns to Player A when the five rounds are averaged. This indicates

that punishment is not promoting a more trustworthy environment when the game is repeated and punishment is available.

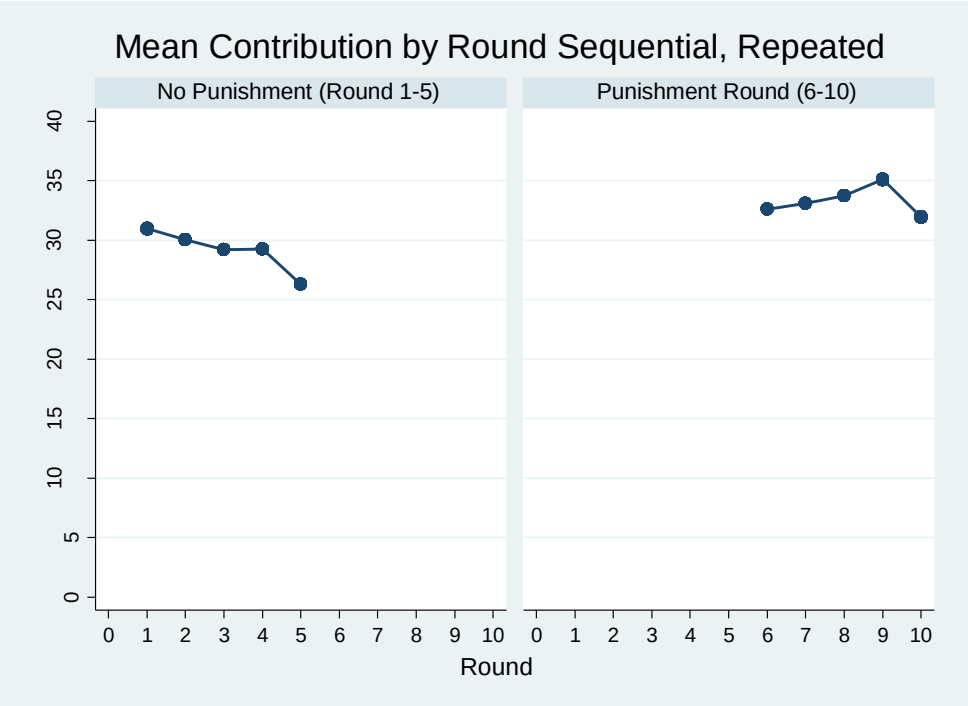
Figure 15: Player B's Reciprocity Sequential Repeated



In the sequential repeated version of the game trust nor trustworthiness were impacted by the punishment regime. Looking at the results for the PGG we see something different. Punishment significantly increased both Player A and Player B's contributions to the public good. Figure 16, plots the mean contributions of both types of player over the 10 rounds. On the left side of the figure we can observe the five periods without punishment. Subjects started with contributions of 30 tokens and that slightly fell to approximately 26 tokens by the last no punishment period. The right hand graph of Figure 16 shows an interesting picture with contributions starting in the punishment rounds at 33 tokens, peaking in round 9 at 36 tokens and ultimately dropping to similar points where the no punishment rounds ended. This indicates that subjects consider the last round of the game a one-shot scenario, with no future reputation being risked. This is something that is very common throughout the public goods literature. There is also evidence of Player

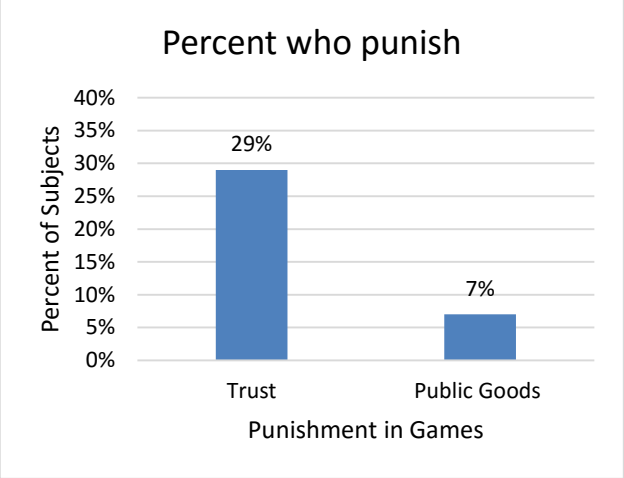
B matching Player A's contribution due to the sequential order of contributions to the good.

Figure 16: Mean Contribution PGG Sequential, Repeated



A final thing observed in the sequential, repeated treatment is the rates of punishment in the two games. Figure 17 shows a clear picture of less punishment than the sequential one-shot setting. However, it still holds that the subjects punish significantly more in the TG than in the PGG. Subjects punish 29 percent of the time in the Trust game with only a mere 7 percent punishing in the trust game.

Figure 17: Percent Who Punish Sequential, Repeated



Making this hybrid version of the PGG and TG, has shown that punishment is effective for public goods provisions in the repeated version, unlike the one-shot version. The sequential order of the game is not changing the impact punishment has on increased contributions. However manipulating the trust game to be played more than once does not create an environment where participants are increasing their trust or trustworthiness. This indicates that the sequential nature of TG does not allow punishment to increase sharing from either player.

4.5.3. Simultaneous, One-Shot

he third treatment that will be examined is the simultaneous, one-shot treatment. In this particular treatment, Player A and Player B are making their decisions at the same time. This creates an environment much like a traditional PGG, where the players do not know what their counterpart contributes until the end of the round. In this case, there are only two rounds of each game, one with punishment and one without. The subjects were paired with four different people, one for each game. By examining the mean contributions

in Table 21, we observe that punishment decreases both trust and trustworthiness in the TG. This indicates that when punishment is available Player A is sending less to player B. Player B is also returning less to Player A. The average percent Player B returns to Player A without punishment is 37 percent. This increases to 38 percent after punishment is introduced. The percent Player B returns is similar to the other one-shot treatment and lower than repeated interactions. The PG shows a non-significant increase in contributions for Player A and a non-significant decrease for Player B to the public good after punishment is introduced. This finding is similar to what we see in a non-standard single PGG.

Table 21: Mean Contributions Simultaneous, One-Shot

	Trust Game		Public Goods Game	
	Player A: Trust	Player B: Reciprocity	Player A: Contribution	Player B: Contribution
No Punishment	11.20 (1.94)	12.28 (3.11)	17.52 (2.57)	22.64 (2.28)
Punishment	9.4 (2.05	10.6 (2.91)	19.08 (2.74)	21.68 (2.65)
Mean Contribution with standard errors in parentheses				

Next, the attention will focus on the transfers indicating trust and the returns that proxy trustworthiness in the TG. Figure 18, shows the tokens that Player A transferred to Player B. The blue bars indicate transfers in the cases of no punishment and the red bars indicate transfers when punishment can be implemented. If punishing were increasing the trusting behavior, the distribution of transfers would shift to the right. In Figure 19, we

can see percent returned by Player B after the money sent by Player A had been multiplied. The left side is the reciprocity exhibited by Player B when no punishment is available, and on the right side is the picture when punishment is possible. Punishment is not increasing trustworthy behavior. If the potential threat of punishment from Player A were changing Player B's decision, we would see a significant increase in the amount returned. This indicates that punishment is not increasing trust or trustworthiness in the TG.

Figure 18: Player A Trust Simultaneous, One-Shot

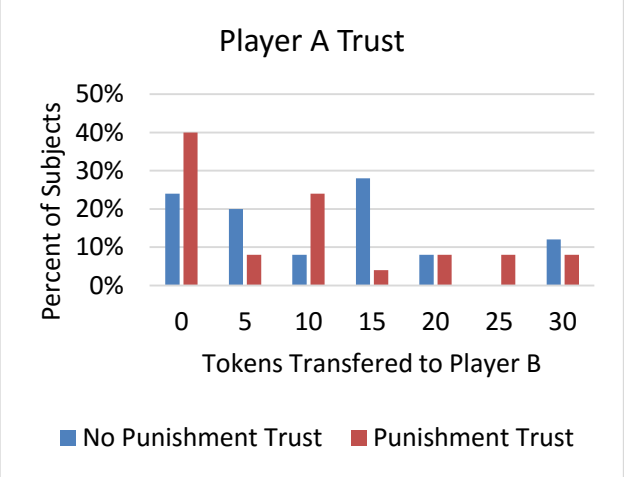
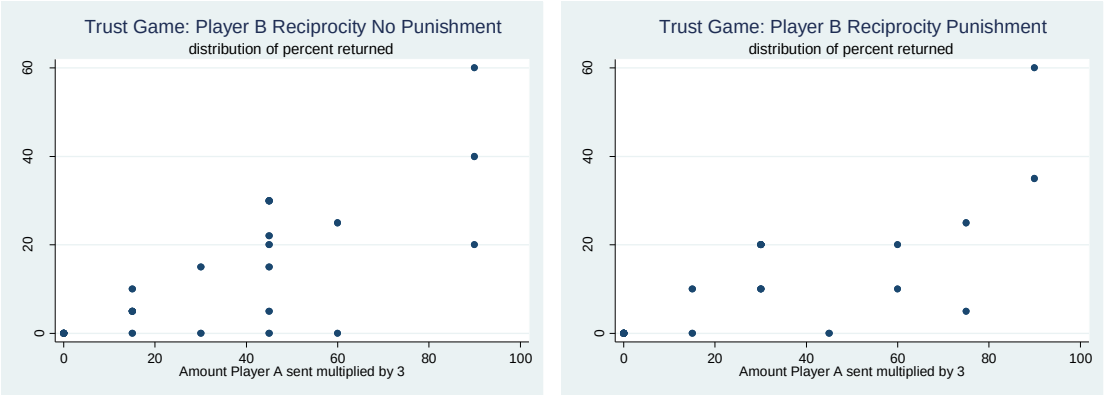
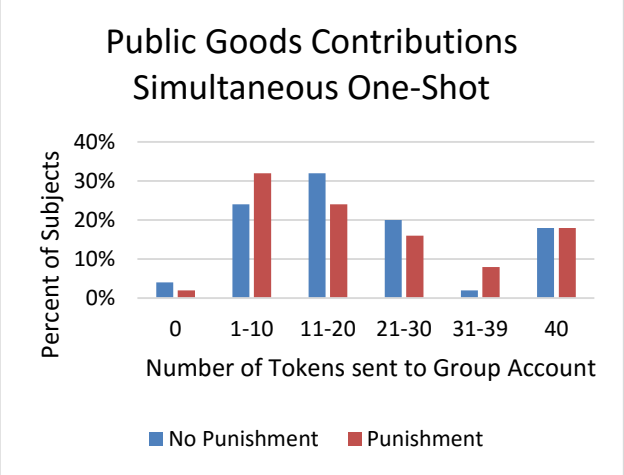


Figure 19: Player B's Reciprocity Simultaneous, One-Shot



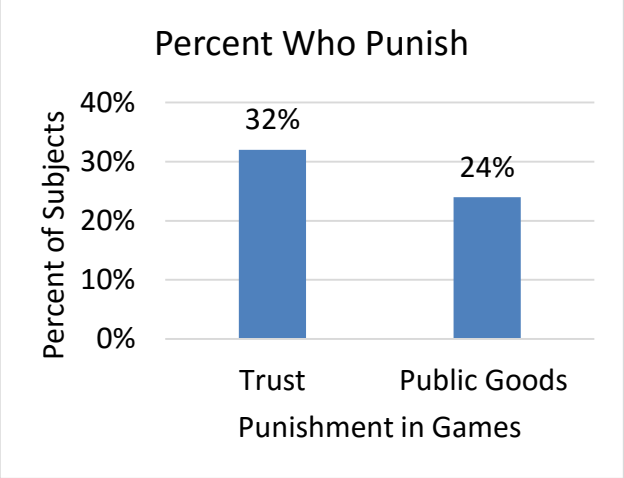
To examine the complete effects of punishment on the two games we need to look more in depth at the PGG. The simultaneous contribution is in line with a traditional PGG, however most PGG are played repeatedly. Figure 20 maps the distribution of contributions of Player A and Player B together. Both players chose how much to contribute to the group account at the same time. Meaning that Player A's contribution should have no impact in the contribution of Player B. The total average contributions to the PGG went up when punishment was available.

Figure 20: Public Goods Contributions Simultaneous, One-Shot



Looking at the percent of individuals who punish in both games, we see a different picture than the previous treatments. Figure 21 indicates that both the PGG and the TG have high levels of punishment. In the TG 32 percent of individuals punish and the PGG has a high punishment rate of 24 percent. This indicates that people punish more when the PGG is simultaneous one-shot, than in any of the two sequential PGG.

Figure 21: Percent Who Punish Simultaneous, One-Shot



Making this hybrid version of the PGG and TG, has shown that punishment is used more often in the one-shot simultaneous game than either of the sequential treatments. However, punishment is not successful increasing contributions in either the TG or the PGG when the game is sequential and played only once. The effects of punishment are predicted by the trust literature, but the PGG literature indicates that punishment should increase contributions even in the one-shot version. However, since out PGG are only two players the increase in contribution could be in part to this design feature.

4.5.4. Simultaneous, Repeated

The final treatment that I will examine is the simultaneous, repeated version of the PGG and TG. This framework is the most similar to the traditional PGG, where subjects make their contributions at the same time and play the game multiple rounds. Traditionally in the previous literature punishment, opportunities increase contributions in this type of public goods setting. The mean contributions can be seen in Table 22. Under this regime

we find that trust and trustworthiness do not increase when punishment is available. However, we do see increase to both players' public goods contributions. This indicates that even though are game settings are manipulated by number of players the overall result, that punishment increase contributions, within the PGG literature still holds true.

Table 22: Mean Contributions Simultaneous, Repeated

	Trust Game		Public Goods Game	
	Player A: Trust	Player B: Reciprocity	Player A: Contribution	Player B: Contribution
No Punishment	20.04 (1.00)	33.60 (2.17)	29.25 (1.15)	28.34 (1.18)
Punishment	19.38 (1.04)	31.95 (2.11)	31.84 (1.11)	32.95 (0.97)
Mean Contribution with standard errors in parentheses				

To further understand the results we will first look at Figure 22, where we can see percent returned by Player B after the money sent by Player A had been multiplied by three in the TG. Player B returns 56 percent of the multiplied transfer to Player A without punishment. There is no significant difference in the percent returned (55 percent) when punishment is available. The findings are consistent through all four treatments.

In the simultaneously repeated version of the TG transfers from Player A to Player B did not increase with the possibility of punishment. We also did not find an increase in trustworthy behavior, or the amount returned from Player B to Player A. On the contrary when looking at the results for the PGG we see something different. Punishment increased both Player A and Player B's contributions to the public good. Figure 23, plots the mean

contributions of both types of player over the 10 rounds. The plot on the left side of the figure left side of the figure plots mean contributions for the five periods without punishment. Subjects started with contributions of 27 tokens increasing by round 4 to over 30 tokens. The last period of the no punishment rounds players decreased their contributions to 26 tokens. The contributions on the right side, when punishment is available show a slightly different picture. We see contributions in the 6th round starting lower but increasing 34 tokens contribution by the second round. This contribution level is sustained until the last round where they drop back to the levels from the first round with punishment. This again indicates that subjects consider the last round of the game a one-shot scenario, with no future reputation being risked.

Figure 22: Player B's Reciprocity Simultaneous, Repeated

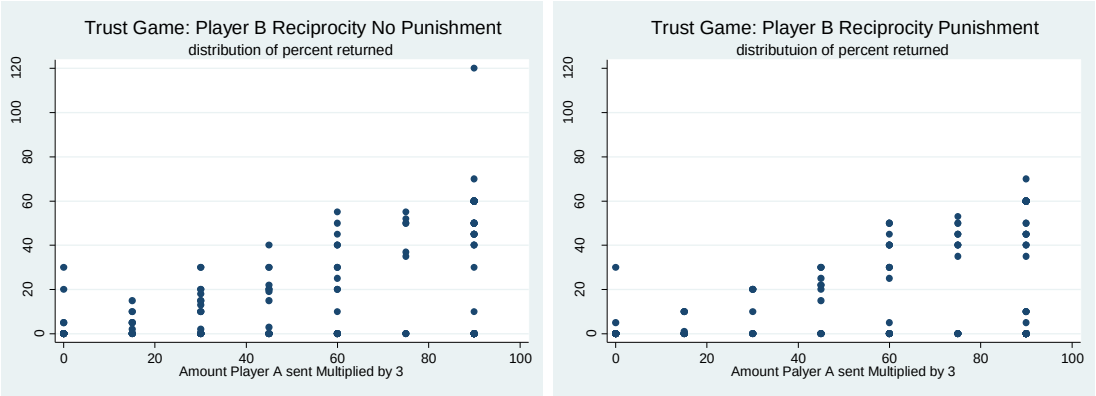
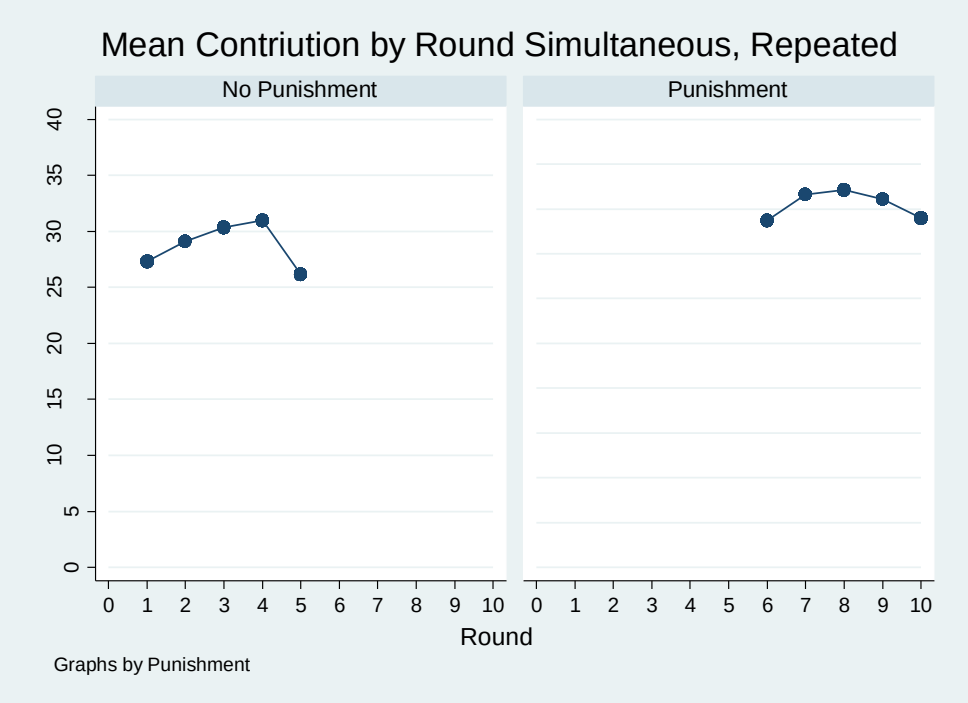
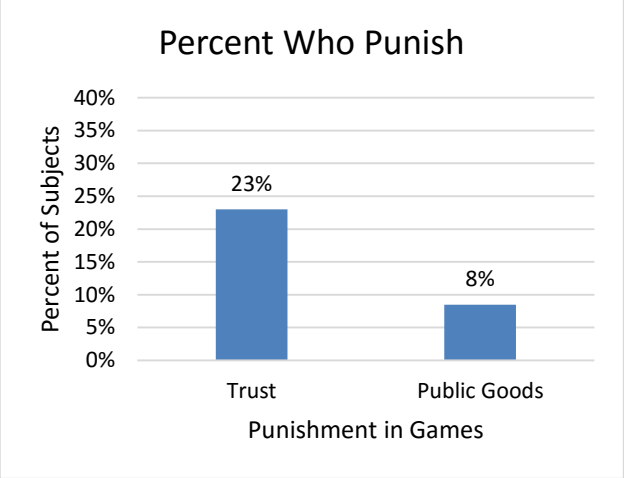


Figure 23: Mean Contribution PGG Simultaneous, Repeated



The final piece of analysis that we must consider in the simultaneous repeated games is the rate of punishment. Figure 24, indicates there is a significant difference in the percent of subjects that punish their counterparts in the PGG and the TG. The TG has a 23 percent punishment rate, while the PGG has only an 8 percent punishment rate. This means that individuals are punishing nearly three times more in the trust setting. The eight percent rate of punishment is consistent with the public goods literature.

Figure 24: Percent Who Punish Simultaneous, Repeated



In the setting that is most similar to the standard public goods game, we find results that indicate the TG and the players' contributions are not sensitive to the order individuals make their decision in nor the duration of interactions. Ultimately, in the traditional PGG setting we see a significant increase much like the standard repeated, sequential version of the game.

4.5.5. Discussion of All Treatments

Using four treatments designed, I manipulated versions of the TG and PGG to understand the different effects punishment has in each game. Reporting mean contribution and percent of subjects who publish for all four treatments combined in Table 23 gives a nice snapshot of the overall results. I was unsuccessful in ever-increasing trust or trustworthiness in the TG. The results were mixed for increase in contributions of the PGG. We also found very strong differences in the rates in which people punish. Subjects always punished at a significantly higher rate in the TG, than in the PGG. This is true across all four treatments and variations of the games. This indicates that the structure of

the trust game is an environment where individuals either feel more comfortable punishing, or believe the punishment will have a stronger impact. The findings indicate that the duration of the play (one-shot and repeated) nor the order of decisions (simultaneous and sequential) can make punishment effects similar in the two games.

Table 23: Mean Contributions All Treatments

		Sequential Contribution				Simultaneous Contribution			
		Trust		Public Goods		Trust		Public Goods	
One-Shot	No Punishment	16.01	22.30	24.70	23.38	11.20	12.28	17.52	22.64
	Punishment	16.55	21.16	26.75	23.11	9.40	10.60	19.08	21.68
	% Who Punish	34%		13%		32%		24%	
Repeated	No Punishment	18.82	33.08	29.97	28.31	20.04	33.60	29.25	28.34
	Punishment	17.80	29.87	33.27	33.32	19.38	31.95	31.84	32.95
	% Who Punish	29%		7%		23%		8%	

4.6 Conclusion

Although I created an experiment where the designed games are as similar as possible, while keeping the unique integrity of the game intact, I was unsuccessful at identifying what part of the game creates the disparity in punishment effects. The results are conclusive that the different outcomes from punishment are not related to the order of play, meaning which player moves first, and what information is available to Player B. This is an interesting finding because using a sequential solicitation for PGG does create a player matching incentive when Player B received information on Player A's

contribution. This experiment also offers evidence that the disparity in punishment effects is not related to one-shot or repeated action. The only difference that was expressed in the data on the rounds played was that punishment in the PGG works better if there are repeated interactions. This means that individuals are more likely to respond to the availability of punishment if the interaction is continued over several periods.

In no case was punishment an effective tool in increasing Player A's transfer to player B in the Trust game. This indicates that the punishment in the trust game is not related to the order of play nor the one-shot or repeated interaction.

The results do show strong support for punishment being more prevalent in the TGG in any situation that in the PGG. The redistribution structure of the game may have much explanatory power in this anomaly. In the trust game, individuals have no return on investment if Player B does not return any invested funds. However, in the PGG you are guaranteed a return of .75 on every dollar contributed no matter what your counterpart does. This makes the potential loss much greater in the trust game than the PGG.

Manipulating the games has ruled out two possible explanations for the different effect of punishment on two popular incentive games in experimental economics. By understanding and identifying the structure of the games, we are able to interpret the results and rule out the game manipulations as motivators of the disparity in punishment. Several more avenues need to be explored before we can completely understand the differences. In future work, it might be beneficial to consider the games redistribution mechanism and lower the MPCR in the PGG. However, this change is difficult to construct without changing the overall premise of the games themselves. Another thing that might

be of interest to observe would be group size. All games examined in this study had two players. It is likely the punishment might increase contributions in a group trust game, although this type of game is not common and difficult to construct and keep the structure true to a TG.

5. GENERAL CONCLUSIONS

The three essays that comprise this dissertation have looked at how pro-social behavior can effect giving and altruism in multiple scenarios. Specifically we look the impact of community characteristics and social norms in section 2, positive pro-social engagement in section 3, and finally negative pro-social punishment enforcement in section 4.

In section 2, we use data collected in the field to examine risk-sharing norms across three unique communities Heterogeneity in risk-sharing behavior could result from variation in individual preferences, or from systematic differences in norms of behavior in different social settings. The individuals in the communities differ with regard to income, race, education, and employment. The communities differ with respect to access to credit, risk exposure and insurance experience. We identify the social and cultural norms that motivate individuals to share risk. In addition, comparing behavior with and without a market insurance option allows us to gauge the strength of sharing norms across communities.

We find that patterns of sharing differ markedly across communities. The poorest community has the highest level of risk sharing in the game. When insurance is offered, 70 percent of our subjects choose to purchase the formal product. We also find that with the possibility of insurance, transfers drop sharply in the higher-income communities, but are impacted the least in the poorest community. Thus individuals who are not insured have a much higher level of community support and higher expected outcomes in the communities with the highest level of risk-sharing norms. To us, this indicates that the

risk-sharing norms in the poor community are very strong, and are not "crowded out" by the availability of a market alternative.

Our results indicate that the prevalence of informal risk sharing in the absence of formal institutions is not restricted to less developed countries, but can also be found in poor areas in developed countries. We also see that the impact of a formal market alternative can vary greatly, depending on the strength of social norms of risk sharing in the communities.

In section 3, we addresses pro-social behavior with respect to charitable giving. In this section we identify the impacts of a successful campaign on charitable organizations. We conduct a "real donation" lab experiment to test whether a successful campaign crowds out giving to other charities. The campaign we use is a simple video campaign giving basic information on one of the charities. By comparing the subjects' allocations before and after the campaign, we measure the effect of the campaign on giving, both to the target of the campaign and the other two charities. In all cases the campaign succeeded in increasing giving to the target organization, indicating that the campaigns were effective. However, in all cases, total giving (the sum of donations to all three charities) remained unchanged. That is, the increase in giving to the target charity came entirely at the expense of the other charities. This provides strong evidence for a 'crowding-out' effect for a targeted campaign. We see that subjects do not increase their charitable expenditures after receiving the video treatment. Instead, they reallocate funds from other charities to the targeted charity. This provides the first evidence of the impact on total giving of a successful fundraising campaign. We see that campaigns impact individuals in

the intensive margin, giving strong evidence that individuals have a budget to allocate to potential charitable causes. We do see increase in giving to the targeted charity, but at the expense of non-target charities. This gives strong evidence that multiple charities can act as a substitute for donors, even if the causes are not related.

To examine this more in depth we design an additional treatment where we prime general charitable behavior, in an attempt to reduce the apparent tradeoff between charitable causes. We find a general charitable prime does not produce an increase in money sent to the three charities. These results support our prior evidence that subjects have a set budget of spending with regard to charitable contributions.

In section 3, I explore a puzzle presented by past experimental literature on two popular games, the trust game and the public goods game. Prior research shows that punishment has opposite effects in the two games, increasing cooperation in the public goods game, but reducing trust and reciprocity in the trust game. While both games are used to study cooperation, they differ in several key ways. I focus on two in particular: the trust game is played sequentially (first mover followed by second mover), while in the public goods game play is simultaneous; and the trust game is usually played only once, while the public goods game is typically repeated for multiple rounds. By constructing hybrid versions of the games to make them more similar, I attempt to identify the cause of the difference in impact and disentangle the effects of punishment. Using two person trust and public goods games, I offer four different treatments in a two-by-two design that varies both the order of moves (sequential vs. simultaneous) as well as the number of rounds (repeated vs. one-shot). Looking at the contributions in each of these treatments with and

without punishment helps isolate whether the order of participation or the number of rounds played is affecting the disparity in the punishment effects. The findings indicate that even in the new games, punishment is unsuccessful in increasing trust or reciprocity in all four versions of the trust game. However, punishment is effective in the modified public goods game, but only in the repeated versions. Punishment is used significantly less in the public goods game than in the trust game. This indicates that the structural differences in the game are not the root cause for the disparity in punishment effects.

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APPENDIX A

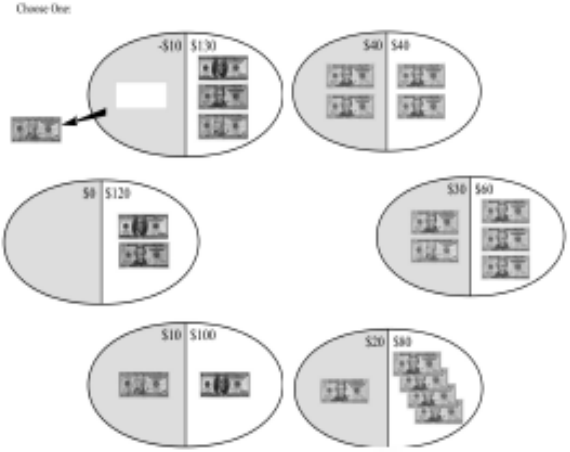
Table A. 1: Demographic Information by Community

Characteristic	South Dallas	Port Lavaca	Brownwood
# of Subjects	201	71	98
% Female	61%	71%	63%
% African-American	96%	9.90%	7.10%
% Caucasian	1.8%	46.0%	79.6%
% Hispanic	2.2%	36.6%	9.2%
Mean Age	43.5	49.1	48.9
% Marital Status			
Married	17.9%	49.1%	50.0%
Single	54.4%	18.3%	11.2%
Divorced/Separated	14.4%	15.5%	16.3%
% Employment			
Student			
Full-Time	10.4%	39.4%	50.0%
Part-Time	10.9%	5.6%	5.1%
Temporary	16.9%	7.0%	5.1%
% Highest Education			
No HS Degree	22.9%	22.5%	6.1%
HS Graduate	41.8%	39.4%	25.5%
Some College	26.5%	25.4%	40.8%
College Graduate	8.8%	12.7%	27.6%
Median Income	Less than 10K	20K-30K	30K-40K

Figure A. 1: Preference Measures

Financial Risk Preference Instrument

Subjects must choose one of 6 possible 50/50 gambles that they wish to participate in. The gambles start at $40/$40 (no risk) then increase in variance and expected value to a maximum expected value gamble of $0/$120. The last option (an indicator of risk-seeking behavior) increases only in variance and not expected value ($-10/$130). An example graphic of the decision sheet presented to participants is displayed below.



Time Preference Instrument

Subjects are asked a series of 6 questions and in each case they choose between receiving $50 tomorrow or a higher amount (ranging from $51 to $150) 6 months from tomorrow. Below is an example of the decision sheet that was presented to the participants.

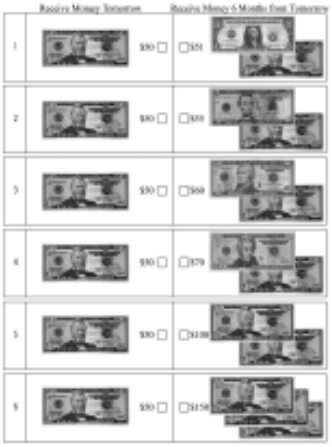
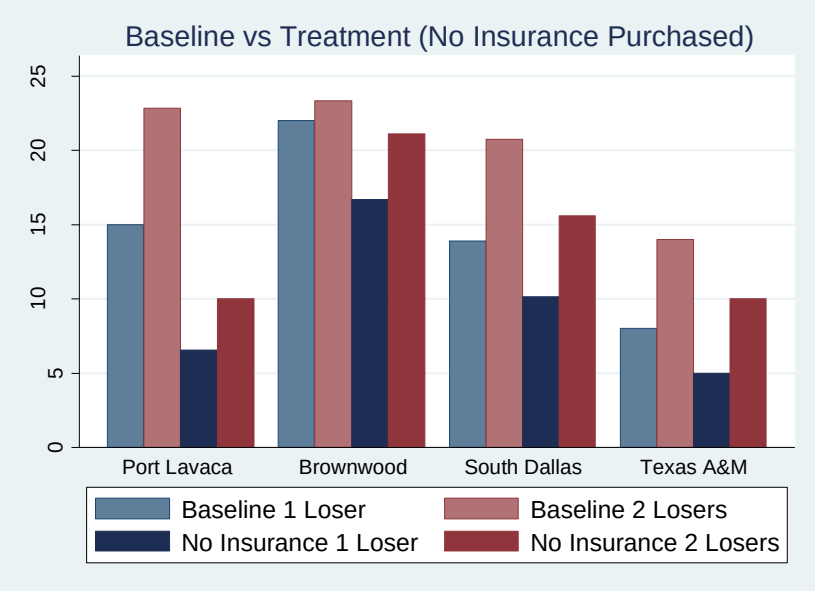


Figure A. 2: Baseline and Treatment When Insurance is Forgone



APPENDIX B

Figure B. 1: Mean Giving to Targeted Charity, Conditional on Giving

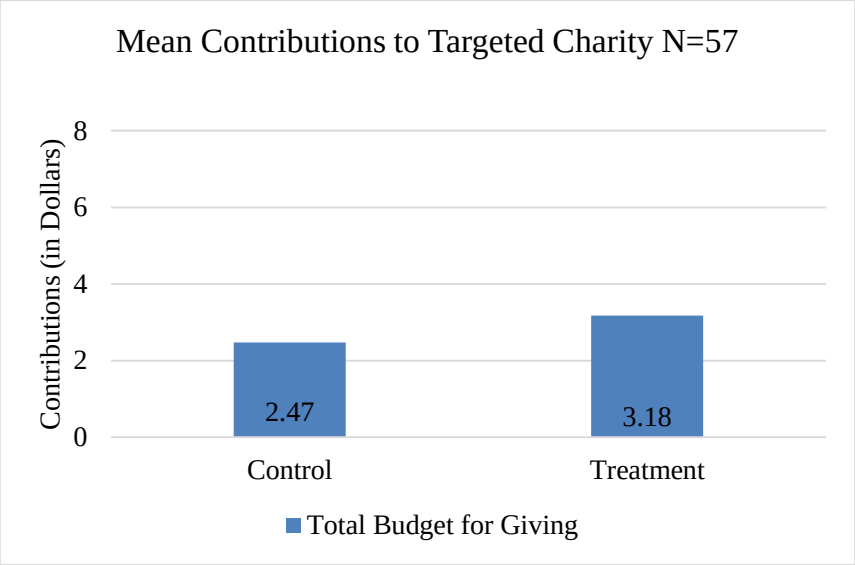


Figure B. 2: Mean Giving to Charity, Conditional on Giving

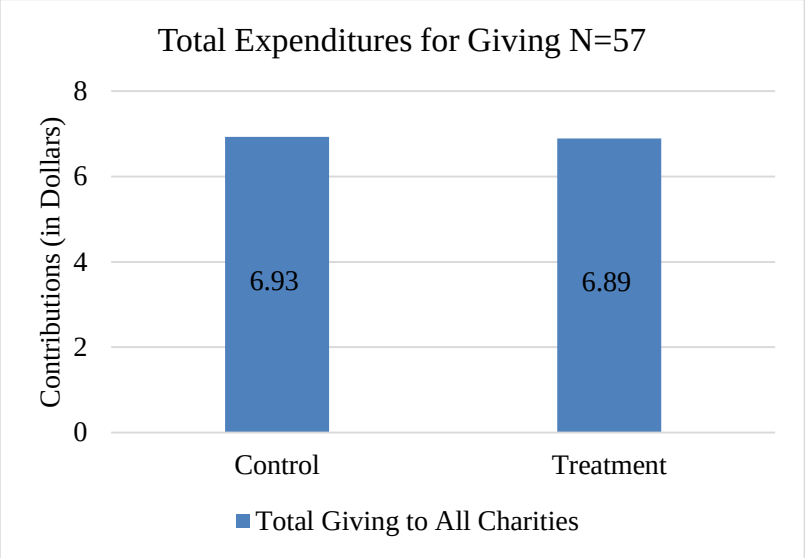


Figure B. 3: Session 1 Mean Contribution

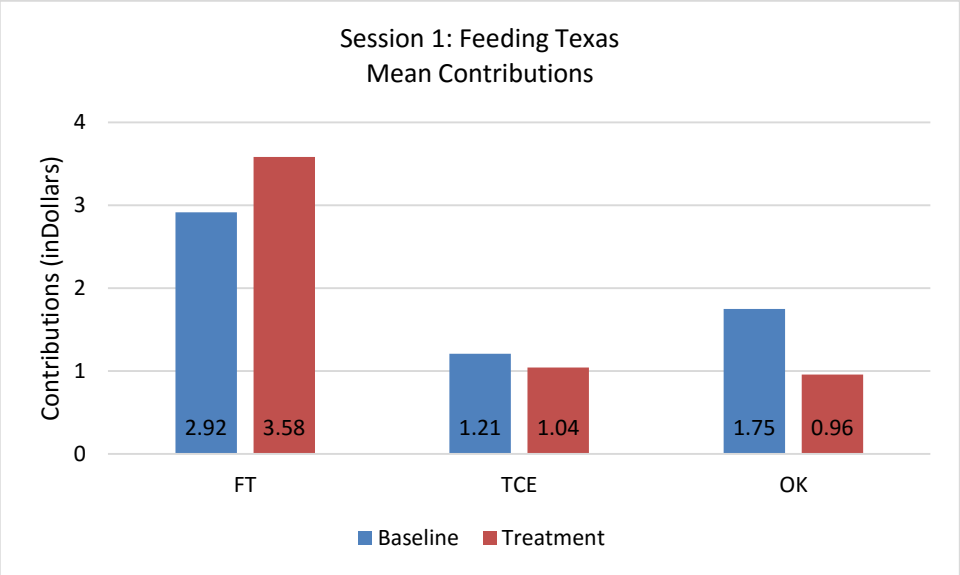


Figure B. 4: Session 2 Mean Contribution

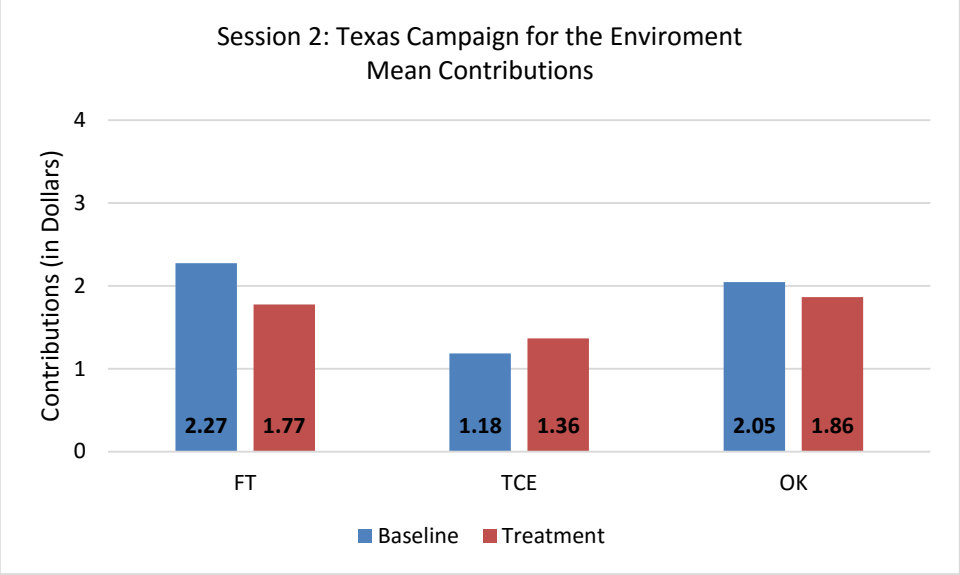
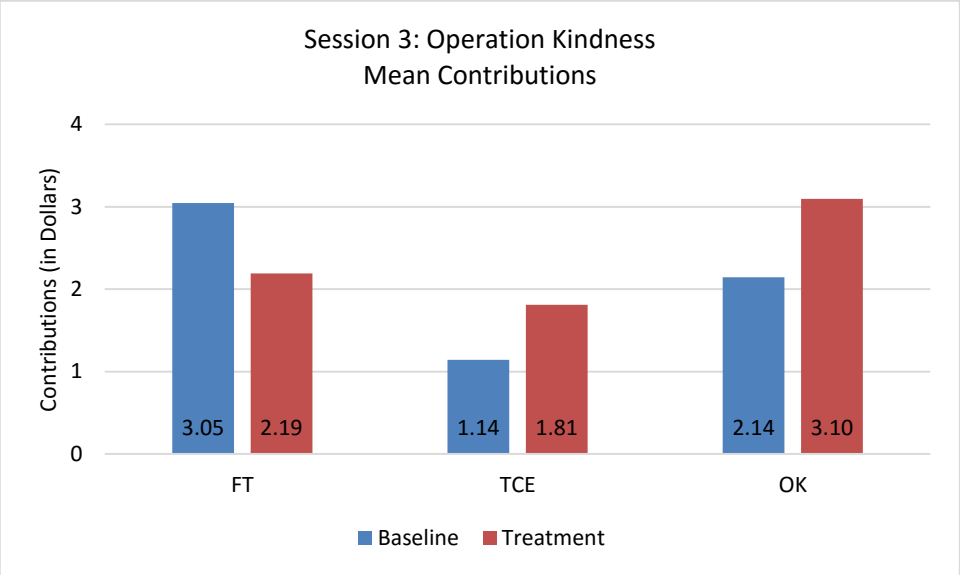


Figure B. 5: Session Mean Contribution



APPENDIX C

Figure C. 1: Eckel-Grossman Risk Elicitation Measure

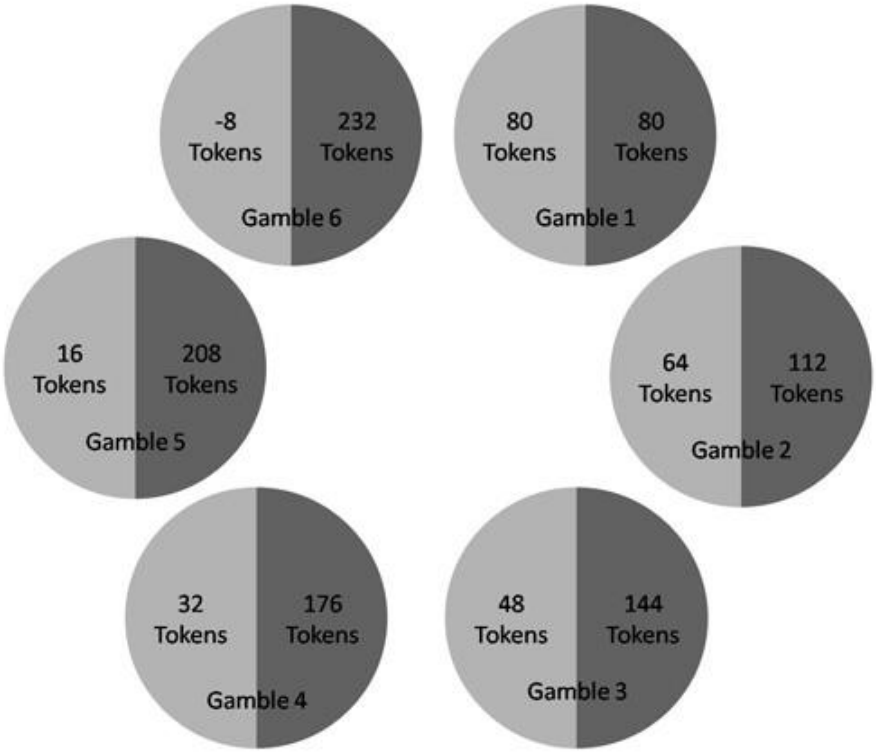


Table C. 1: Experimental Design Ordering

Order Blocking

Order A	**Order B**
Public goods with no punishment (1 or 5 Rounds)	Trust with no punishment (1 or 5 Rounds)
Public goods with punishment (1 or 5 Rounds)	Trust with punishment (1 or 5 Rounds)
Trust with no punishment (1 or 5 Rounds)	Public goods with no punishment (1 or 5 Rounds)
Trust with punishment (1 or 5 Rounds)	Public goods with punishment (1 or 5 Rounds)
Eckel-Grossman Risk game (One-Shot)	Eckel-Grossman Risk game (One-Shot)
Survey	Survey

APPENDIX D

INSTRUCTIONS AND DECISION SHEETS

Instructions for "Using Experimental Games to Understand Risk Sharing Behavior in Three Communities

Baseline: Instructions were read aloud with pictorial examples from the experimenter

Please open your booklets to Activity 8 on Page 3. Does everyone have this page?

OK, please turn the page. You will see a sheet that says Activity 8 Example. This sheet is for practice. You will make your choices on a different page. Let's walk through the example.

For Activity 8, you will be placed in a group of 3 people, you and two others. You will not know who they are, and they will not know who you are.

If this activity is chosen for payment, then you will pull a chip out of this bag. The bag has two chips with a —W on them and one chip with an —L on it. If you pull out the chip with the —W on it, then you win and make $75. If you pull out the chip with the —L on it, then you lose and make $0.

If you draw a —W you can choose to send some of your winnings to the people in your group who draw an —L if you want to.

There are several possible things that could happen.

1st: All three people draw —W. In this case each of you makes $75.

2nd: All three people draw —L. In this case each of you makes $0

Next, you could draw —L and either one or both of the other people in your group draw a —W. In this case, the amount of money you make depends on the amount of money that the people who draw —W send to you.

You will need to make a decision for two different situations. In the first situation, you will decide how much you want to send if one person draws —L and the other person draws —W. In the second situation you will decide how much you want to send if two people draw —L. You do not have to send anything if you don't want to. You need to choose for both situations because you do not know ahead of time who will draw an —L or a —W. The other people in your group will make the same decision.

You will write in the amount that you want to send if one person draws —L here [point to poster], and you will write in the amount that you want to send if two people draw —L here [point to poster].

Payoff: If this is the activity chosen for payment, then we will come around the room and each of you will pull a chip out of the bag. We will mark in your booklet whether you draw a —L or a —W and then put the chip back in the bag and continue around the room.

We will then put all of the booklets in a pile and shuffle them. We will then pull out 3 at random to form the groups. Again, you will not know who is in your group and they will not know who you are.

If you draw a —W, you make $75 minus the amount you decide to send to people who draw a —L. If you draw a —L then you make $0 plus the amount that the people in your group decide to send. Are there any questions?

OK, please turn your booklets to page 7 and write in the amount you would like to send, if anything.

Treatment (Insurance)
For Task 4, you will be placed in a group of 3 people, you and two others. But, just like the last task, you will not know who they are, and they will not know who you are.
Part of this task is similar to the one we just finished, but parts of it are different. Please listen to the instructions before making your choices.

Just like the last task, if this task is chosen for payment, then you will pull a chip out of this bag. The bag has two chips with a "W" on them and one chip with a "L" on it. If you pull out the chip with the "W" on it, then you win and make $75. If you pull out the chip with the "L" on it, then you lose and make $0.

Only for this task, you also have the option of spending the $20 you received for coming today. You do not have to pay this fee. But, if you choose to pay the $20 fee then it will guarantee that you will draw a "W" and will win $75 for sure if this activity is chosen for payment.

If you want to pay the fee, all you have to do is put a check mark in this box [mark on poster]. If you do not want to pay the fee, put a check mark in this box [mark on poster].

Like the previous task, if you draw a "W" you can choose to send some of your winnings to the people in your group who draw a "L" and lose their money, if you want to.

Note that in this situation, the only way it is possible for the other group members to draw a "L" is if they decided <u>not</u> to pay the fee.

So first thing, you will have to put a check mark here or here if you want to pay the fee or not. Then you will write in the amount that you want to send if one person does not pay the fee and draws "L" here [point to poster], and you will write in the amount that you want to send if two people do not pay the fee and draw "L" here [point to poster].
<u>Payoff:</u>
If this task is selected for payment, we will then put all of the booklets in a pile and shuffle them. We will then pull out 3 at random to form the groups. Again, you will not know who is in your group and they will not know who you are.
Are there any questions?

OK, please turn your booklets, decide if you want to pay the fee and write in the amount you would like to send, if anything to the members in your group who do not pay the fee and draw an "L".

Instructions for "Did the Icebucket Challenge Drain the Philanthropic Reservoir?"

Please do not talk to any other subjects during the experiment or turn ahead to any pages until you are instructed to do so.

You are going to participate in a study of decision-making. The study will last about 20 minutes. You will receive compensation based on the decisions you make, which will be paid to you in cash at the end of the study. You will receive $5 as a show up fee regardless of the decisions made today. How your decisions affect your compensation is explained below.

A monitor will be randomly chosen from the experiment participants. The monitor is responsible for verifying that all the decisions are made according to the instructions. The monitor is also responsible for making sure that any money donated to organizations in the course of the experiment actually is donated online to the organization at the conclusion of the experiment. At the end of the experiment, the monitor will sign a form verifying that procedures were followed as described in the instructions.

There are two rounds in this study. At the end of the study, one round will be selected at random for payment, and the choices that you make for that round will be implemented. Any amounts donated to a charitable organization will be donated online, and you will be able to access the receipts by sending a request to the ERL. The amount not donated will be paid to you in cash at the end of the experiment.

Allocation Problems

For this study, you will be allocated $15 in each of two rounds. Your compensation will be determined by your choices in ONE of the two rounds, selected at random. The decision that you will be paid for is dependent upon a die roll, with 1-3 paying Decision 1 and 4-6 paying Decision 2. You do not know, and we do not know, which round will be selected for payment, so your best bet is to make your decisions in each round as if that is the round that is paid.

In each round, you must decide how to allocate the $15 between yourself and three charitable organizations. All three charities are Texas non-profit organizations.

The three charities are:

Operation Kindness: Founded in 1976, Operation Kindness is the oldest and largest no-kill shelter in North Texas.

Feeding Texas: Feeding Texas, formerly known as the Texas Food Bank Network (TFBN) is a statewide, 501(c) nonprofit organization. Its mission is to lead a unified effort for a hunger-free Texas.

Texas Campaign for the Environment: Texas Campaign for the Environment is dedicated to informing and mobilizing Texans to protect the quality of their lives, their health, their communities and the environment.

Examples: Let us look at three examples of decisions, as they will appear on your DECISION SHEET. These are just sample allocations. When you make your actual decision, you may choose any allocation you like.

Note: The sum of the four columns must be no more than your $15 endowment. All amounts must be in whole dollar increments.

Example 1:

For example, you might decide to allocate all of the money to one organization. In this case, you and the other two organizations would receive zero.

Endowment	**Operation Kindness**	**Feeding Texas**	**Texas Campaign for the Environment**	**Yourself**
$15	$0	$15	$0	$0

Example 2:

You might decide to allocate the money equally among the three organizations and yourself.

Endowment	**Operation Kindness**	**Feeding Texas**	**Texas Campaign for the Environment**	**Yourself**
$15	$4	$4	$4	$3

Example 3:

Or you might make some other allocation:

Endowment	**Operation Kindness**	**Feeding Texas**	**Texas Campaign for the Environment**	**Yourself**
$15	$2	$4	$1	$8

NOTE: The only requirement is that the four columns must add up to the $15 endowment. A calculator is provided.

Decision Sheets For "Did the Icebucket Challenge Drain the Philanthropic Reservoir?"
Decision Sheet 1:

Round 1:

Please allocate your given endowment between the four categories below.

Note: The sum of the four columns must be no more than your $15 endowment. All amounts must be in whole dollar increments.

Endowment	**Operation Kindness**	**Feeding Texas**	**Texas Campaign for the Environment**	**Yourself**
$15	$_____	$_____	$_____	$_____

Decision Sheet 2:

In this round, you will once again have the opportunity to allocate a new $15 endowment between the three charities and yourself.

Before you make any decisions, a randomly chosen video will be played detailing additional information on one of the charities.

Please wait for the video to conclude before making any allocation decisions

STOP

Please do not turn the page until instructed.

Decision Sheet 2:

Please allocate your given endowment between the four categories below.

Note: The sum of the four columns must be no more than your $15 endowment. All amounts must be in whole dollar increments.

Endowment	**Operation Kindness**	**Feeding Texas**	**Texas Campaign for the Environment**	**Yourself**
$15	**$_____**	**$_____**	**$_____**	**$_____**

STOP

Please do not turn the page until instructed.

GSS Altruism Survey for "Did the Icebucket Challenge Drain the Philanthropic Reservoir?"

(Part of post survey)

All on 1-5 scale (1=Never, 2=Once, 3= More than once, 4=Often, 5=Very Often)

1.) I have helped push a stranger's car that was broken down or out of gas.
2.) I have given directions to a stranger.
3.) I have made change for a stranger.
4.) I have given money to a charity.
5.) I have given money to a stranger who needed it (or asked me for it).
6.) I have donated goods or clothes to a charity.
7.) I have done volunteer work for a charity.
8.) I have donated blood.
9.) I have helped carry a stranger's belongings (books, parcels, etc).
10.) I have delayed an elevator and held the door open for a stranger.
11.) I have allowed someone to go ahead of me in a lineup (in the supermarket, at a copy machine, at a fast-food restaurant).
12.) I have given a stranger a lift in my car.
13.) l have pointed out a clerk's error (in a bank, at the supermarket) in undercharging me for an item.
14.) I have let a neighbor whom I didn't know too well borrow an item of some value to me (eg, a dish, tools, etc).
15.) I have bought 'charity' holiday cards deliberately because I knew it was a good cause.
16.) I have helped a classmate who I did not know that well with an assignment when my knowledge was greater than his or hers.
17.) I have, before being asked, voluntarily looked after a neighbor's pets or children without being paid for it.
18.) I have offered to help a handicapped or elderly stranger across a street.
19.) I have offered my seat on a bus or train to a stranger who was standing.
20.) I have helped an acquaintance to move households.

Video Script for "Did the Icebucket Challenge Drain the Philanthropic Reservoir?"

Video 1: Feeding Texas: "Hi, my name is Adam Zindler, and I'm a member of the Fightin' Texas Aggie class of 2016, a-a-a-whoop! Listen up, y'all, 'cause I would like to tell you a little bit about a charity called 'Feeding Texas'. So these guys do a heck of a lot of good here in Texas. 'Feeding Texas' is a non-profit, dedicated to wiping out hunger all across the state. They're active in communities all across Texas, with a network of food banks that gave away more than three hundred million pounds of food just last year. That means helping almost three and a half million Texans every year. They're out there trying to solve the problem of hunger in our communities, including here in Brazos County, and they need your help. Thank you for considering 'Feeding Texas' as you make your decisions today."

Video 2: Operation Kindness: "Hi, my name is Adam Zindler, and I'm a member of the Fightin' Texas Aggie class of 2016, a-a-a-whoop! Listen up, ya'll, 'cause I would like to tell you a bit about a charity called 'Operation Kindness'. These guys do a heck of a lot of good here in Texas. 'Operation Kindness' is the oldest, and largest no-kill shelter in Texas, and they've saved more than seventy five thousand animals. That's about three hundred animals they care for daily, and more than three thousand dogs and cats they help every year. 'Operation Kindness' is a non-profit, and doesn't get any government funding. This means that they need donations from people like you to stay afloat. Thank you for considering 'Operation Kindness' as you make your decisions today."

Video 3: Texas Campaign for the Environment: "Hi, my name is Adam Zindler, and I'm a member of the Fightin' Texas Aggie class of 2016, a-a-a-whoop! Listen up, ya'll, 'cause I'd like to tell ya'll a little bit about a charity called 'Texas Campaign for the Environment', and they do a heck of a lot of good here in Texas. What's our most precious resource? That's Texas itself, obviously, and the 'Texas Campaign for the Environment' is out there protecting the state we love so much, because we want our children, and our children's children to live in a place just as beautiful, and as healthy, as where we live now. Ninety percent of their funding comes directly from donations, and three quarters goes directly towards environmental advocacy, community organizing, and public education. If you love Texas, vote with your dollar, and help these folks out. Thank you for considering the 'Texas Campaign for the Environment' as you make your decisions today."

Experimenter Script for "Did the Icebucket Challenge Drain the Philanthropic Reservoir?"

Experimenter reads everything in italics but only things in italics.

General Instructions:

This is a study of economic decision making. Your earnings in this study depend on the decisions that you make. You will be paid these earnings privately in class at the end of the session today. Please take a minute to turn off your cellphones. There is no talking during the study except to ask questions. If you have questions at any time, please raise your hand and someone will come and assist you. Please make sure to hold on to your Subject ID number. You will need it throughout the experiment, and for payment.

Instructions:

Howdy,

This study will last about 30 minutes. You will receive compensation based on the decisions you make, which will be paid to you in cash at the end of the study. How your decisions affect your compensations is explained below.

*A monitor will now be selected at random. *Randomly select monitor*. The monitor is responsible for verifying that all the decisions are made according to the instructions. The monitor is also responsible for making sure that any money donated to organizations in the course of the experiment actually gets donated online to the organization at the conclusion of the experiment. At the end of the experiment the monitor will sign a form verifying that procedures were followed as described in the instructions.*

(Have monitor come to the front of the room. Hand them the monitor sheet and explain to them their duties.)

There are two rounds in this study. At the end of the study, one round will be selected at random for payment, and the choices that you make for that round will be implemented. Any amounts donated to a charitable organization will be donated online, and you will be able to access the receipts by sending a request to the ERL. The amount not donated will be paid to you in cash at the end of the experiment.

Allocation Problems

For this study, you will be allocated $15 in each of two rounds. Your compensation will be determined by your choices in ONE of the two rounds, selected at random. The decision that you will be paid for is dependent upon a die roll, with numbers 1,2,& 3 paying Decision 1 and numbers 4,5,& 6 paying Decision 2. The monitor will roll the die that decides the round in which you will be paid. You do not know, and we do not know, which round will be selected for payment, so your best bet is to make your decisions in each round as if that is the round that is paid. In each round you must decide how to allocate the $15 between yourself and three charitable organizations. All three charities are Texas non-profit organizations.

Please take a few moments to look over the three charities.
WAIT 1 MINUTE and wait until everyone looks up.
Please turn to the next page Titled "Examples."

Examples:
Let's look at three examples of decisions as they will appear on your DECISION SHEET.
These are just sample allocations. When you make your actual decision you may choose any allocation you like in whole dollar increments. Note: The sum of the four columns must be no more than your $15 endowment. Again, all dollar amounts must be in whole dollar increments.
Example 1:
For example, you might decide to allocate all of the money to one organization, in this case Feeding Texas. In this case, you and the other two organizations would receive zero dollars.
Example 2:
You might decide to allocate the money nearly equally among the three organizations and yourself. All three charities would receive four dollars and you would receive three.
Example 3:
You might choose another allocation. In this example, the three charities would each get different amounts and you would receive eight dollars.
Are there any questions?
Please turn to the next page, titled "Decision Sheet 1:"

Decision 1:
Please allocate your given endowment between yourself and the three charities below.
Note that all columns must add up to $15 in whole dollar increments and all allocations must be in whole dollar amounts.

WAIT 2 Minutes or until you think everyone is done
Please turn to the next page titled "Decision Sheet 2:"

Decision 2
In this round, you will once again have the opportunity to allocate a new $15 endowment between the three charities and yourself.
Please welcome Marc Rauckhorst, he has an important message for ya'll.
*Hi, my name is Marc Rauckhorst, Fightin' Texas Aggie Class of 2016 A-A-A-Whoop!!! Now we're going to show you a video about a charity that does a bunch of great things here in Texas. We challenge you to really think the about the effects your donation will have on homeless animals here in Texas. Your donation truly matters. Pay close attention to the upcoming video. It contains *vital* information.*

PLAY VIDEO

Thanks for listening. We challenge you to give now. Please turn to the following page, also entitled Decision Sheet 2: Please allocate your given endowment between yourself and the three charities below. Note: The sum of the four columns must be no more than your $15 endowment. All amounts must be in whole dollar increments.

After 2 minutes or when you feel everyone is done:
Thank you for participating in the first section of the experiment. Please make sure that your random Subject Number is written in the top right of the front page of this packet.
Has everyone written their Subject Number on the front of their packet?
One of the experimenters will come around and pick up your sheet shortly. Please stay seated and quiet and do not change any answers already written. Thank you.

Have monitor come to the stage and roll the die to figure out which decision will be paid. Make sure the monitor announces what die number he/she rolls. Monitor will leave the room with two people to fill in spreadsheet and make payment envelopes.
Depending on decision selected by die roll, fill out spreadsheet, and make envelopes for payment.

While monitor is exiting room, pass out survey.

Survey
*Everyone should now have a survey packet that looks like this *hold up packet* in front of them. Write the Subject Number assigned to you in the top right corner on the front page of this packet.*
Has everyone written their Subject Number on the front of their packet?
Please answer the following questions to the best of your ability.
When you are finished, raise your hand and an experimenter will pick up your survey and give you a receipt form to fill out. When payments are ready, an experimenter will escort you to the payment window. You will need your subject ID card to receive the payment at the window.