ESSAYS ON FAIRNESS PREFERENCES: AN EXPERIMENTAL APPROACH

A Dissertation

by

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Submitted to the Office of Graduate and Professional Studies of
Texas A&M University
in partial fulfillment of the requirements for the degree of

DOCTOR OF PHILOSOPHY

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August 2016

Major Subject: Agricultural Economics

ABSTRACT

Fairness is a central topic in ethics, political science and economics. A variety of economic questions, such as charitable donation, welfare programs, taxation of income and inheritances, are closely associated with people's fairness preferences. Hence it is important to take fairness and justice into account for making appropriate social welfare policy. This dissertation contributes to the related literature in understanding people's fairness preferences by using an experimental approach. It consists of three interrelated essays, all of which focus on fairness preferences and redistributive decision making under risk and uncertainty. The first essay is an experimental investigation into fairness preferences and redistribution under different rooted risks, i.e., pure-luck versus option-luck. Our experimental results reconcile the conflict between the accountability principle and consequential egalitarianism by suggesting that people are more inclined to the accountability principle in the presence of relatively lower income inequality but support consequential egalitarianism in case of large income inequality or salience of extreme low payoff. The second essay explores the dynamic evolution of a laboratory economy in which fairness preferences, risk preferences, and income inequality are jointly determined under different redistributive policies. By using a panel vector auto-regression model, we find different patterns of interplay between subjects under different redistributive regimes. The third essay studies how charitable giving is influenced by the performance of charitable organizations and associated organization costs. We study charitable giving in a laboratory experiment, in which donors are confronted with a tradeoff between helping people in need and the possibility of being cheated. We find evidence that individuals exploit the shadow of fundraising cost to excuse their selfishness with

a self-serving biased belief that fundraisers are corrupt. In contrast, the charitable contribution significantly increases when the moral excuse is removed by excluding the manipulation of costs by the fundraisers.

DEDICATION

To my parents and my wife, to whom I owe too much.

ACKNOWLEDGEMENTS

I am especially indebted to my advisor and co-author Marco Palma, who made this research possible. I gratefully acknowledge Alex Brown, Ximing Wu, and Yu Yvette Zhang for their guidance and encouragement. I also wish to thank Andrew Barr, Daniel Chavez, Laura Dague, Catherine Eckel, Hongyu Guan, Mark Hoekstra, Samir Huseynov, Bachir Kassas, Yajuan Li, Edwin Price, Michelle Segovia, William Shaw, Richard Woodward, Jia Yan, Peilu Zhang, Ping Zhang, Yinjunjie Zhang, and Yu Zhang for invaluable comments and helpful discussions. I also thank participants at ESA, SAEA, and Shanghai Tech for their helpful comments. All errors are my own.

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1. INTRODUCTION

Justice and fairness have been in the heart of moral philosophy and political science, from Aristotle to Jeremy Bentham and Immanuel Kant, and then John Rawls and Robert Nozick. Fairness is also an important subject in economics, since deep understanding of fairness and justice can improve welfare policy evaluation and social welfare function. A wide range of economic questions, such as charity, public health programs, education, taxation of income and inheritances, are in close connection with how fairness preferences of people are shaped and changed in distinct contexts. Fairness preference is a crucial determinant of social mobility and long-run growth in a society (Alesina and Rodrik, 1994; Alesina and Angelotos, 2005; Alesina et al., 2012). In spite of the growing consensus in the importance of fairness in understanding distributive decisions and making effective public policy design, many open questions still await further investigation.

Studies on justice and fairness can be classified into two branches in general, normative analysis and positive analysis. Normative justice theories debate over what standard of justice is appropriate, such as the contemporary debate over moral desert between Rawls (1971) and Nozick (1974). A desert claim, which is the core of moral philosophy, includes three ingredients, a deserving subject, a deserved object and a desert basis (Stanford Encyclopedia of Philosophy). In other words, a desert claim is that person A deserves object B because of reason C. The right desert basis is often controversial. For example, Rawls argues that a person does not have legitimate credit for natural born endowments (such as superior intelligence or athletic abilities), since it is purely the result of the 'natural lottery'. On the contrary, Nozick claims that it contradicts the basis of Rawls' deontological liberalism by treating peoples' natural talents as collective assets. While the reasonable judgment of correct desert basis involves professional education in philosophy and political science, ordinary people also have their own standards for fairness and justice, and apply them consciously or unconsciously. How people understand fairness can affect not only their personal life, but also the public policies through political process such as deliberation and voting.

Although we will discuss the debate on fairness principles among moral philosophers, normative analysis is beyond the main scope of this dissertation. This dissertation mainly focuses on a positive analysis. In other words, this dissertation is not about the debate over which theory is more right or fair than others. Instead, the goal of my dissertation is to contribute to the economics literature in understanding fairness preferences, specifically understanding how people build and change their fairness view depending on specific contexts. Although policy analysis is not the main aim of this dissertation, an understanding in theoretical depth is expected to have a salient contribution to policy designs associated with specific fairness preferences and justice concerns.

Economic experiments are employed in this dissertation. Experiments in the laboratory enable us to elicit fairness preferences of people and test competing theories in a controlled environment. In other words, experimental methods allow better control over confounding factors and obtain more robust causal inferences.

A vast previous literature finds evidence on social preferences that explain the deviations from pure self-interested behaviors in laboratory and field experiments (Camerer, 2003). Fairness plays an important role when people make individual or group decisions. However, a universal standard for fair allocation of resources and wealth does not exist. Instead, people have different views of fairness judgment, which vary greatly according to the specific contexts. Exploring fairness preferences is particularly challenging when there is ambiguity about desert due to choice and effort under risk and uncertainty.

This dissertation consists of three interrelated essays, all of which focus on fairness preferences and redistribution under risk and uncertainty. The first essay is an experimental investigation into fairness preferences and redistribution under different rooted risks. The experimental design enables us to elicit different views about distributive justice conditional on the nature of risks, i.e., whether the risk is exogenously assigned in a pure-luck control condition or endogenously chosen by subjects in an option-luck treatment. According to the accountability principle (AP), a person should be responsible for his/her loss due to his/her own choices. AP predicts significantly lower redistribution when people have the opportunity to alleviate their financial loss in the option-luck treatment. In contrast, consequential egalitarianism (CE) supports the same level of redistribution no matter whether or not people have the discretionary power to control the risk, since only the consequential inequality in economic outcomes matters in consideration of fair distribution, e.g., a discharge of accountability for discretionary choice. Our experimental results reconcile the conflict between the accountability principle and consequential egalitarianism by suggesting that people tend to support the accountability principle in the presence of relatively lower income inequality but are more inclined to consequential egalitarianism in case of large income inequality or salience of extreme low payoff. The results are also important to interpret some prosocial behaviors and shed light on social and economic policies, such as health insurance plans, job assistance programs, and so on. The redistributive policy making needs to consider people's fairness views that depend on the context richness.

The second essay explores the dynamic evolution of a laboratory economy in which fairness preferences, risk preferences and income inequality are jointly determined under different redistributive policies. In each of twenty periods of the Partner treatment, subjects choose between a safe and a risky option and then make redistributions within the fixed pair of subjects, while in the Spectator treatment stakeholders make the same choice tasks but a third party makes the redistributions. The different rules mimic the distinct redistributive policies in the real world, e.g., community-based or government-regulated. This essay introduces a panel vector auto-regression model to compare different dynamic interplays between the two treatments. We find that in the Spectator treatment, stakeholders do not affect each other, but mainly rely on the spectators' decisions. In contrast, in the Partner treatment, stakeholders show a stronger interplay in reciprocity and choices under risk.

The third essay mimics the environment of charitable contribution to investigate how charitable giving is influenced by the performance of charitable organizations and associated organization costs. In real life, donors are confronted with a tradeoff between helping people in need and the possibility of being cheated. Donors may justify not giving while being able to excuse their selfishness with a self-serving biased belief that the fundraisers are corrupt. In this laboratory experiment, we find evidence that participants are more likely to exploit the ambiguity of fundraising cost to bias their belief and contribute less when the incentive for selfishness is greater. In contrast, the charitable contribution significantly increases when the moral excuse is eliminated by excluding the manipulation of costs by the fundraisers. The findings of this experiment deepen our understanding of the motives for charitable giving and provide policy relevance to make charitable fundraising more effective. Legitimate fundraisers can differentiate themselves by providing transparent information about how donations are spent. Since relevant information is a public good having free-riding problem, governments and NGOs should implement more efficient policies to detect and disclose information about charitable organizations and recipients.

2. RECONCILING CONSEQUENTIAL EGALITARIANISM AND ACCOUNTABILITY PRINCIPLE: AN EXPERIMENTAL STUDY [1]

We conducted a laboratory experiment to investigate how people's fairness preferences and redistribution depend on different rooted risks; specifically the risk is exogenously assigned in a pure-luck control condition or endogenously chosen by subjects in an option-luck treatment. According to the accountability principle, a person should be responsible for his/her loss due to his/her own choices. In contrast, egalitarianism supports higher level of redistribution, since only the consequential inequality in economic outcomes matters in consideration of fair distribution, namely a discharge of accountability for discretionary choice. Our experimental results reconcile the conflict between the accountability principle and the consequential egalitarianism by suggesting that people are more inclined to accountability principle in the presence of relatively lower income inequality but support consequential egalitarianism in case of large income inequality or salience of extreme low payoff.

2.1 Introduction

Justice and fairness have been central research subjects in the fields of moral philosophy and political science. Fairness began to play an important role in economics from the early 1990s (Konow, 2003; Rabin, 1993), since it is closely associated with a variety of social and economic questions, such as charitable donation, fair trade agreements, taxation of income and inheritances, health insurance coverage. Due to the rising income inequality in post-war developed nations, related studies become even more critical and invoke greater interests from researchers and policy makers

[1] Part of this chapter is reprinted with permission from 'Xu, Zhicheng, and Marco Palma, Consequential Egalitarianism vs. Accountability Principle: An Experimental Investigation. *Applied Economics Letters* 23(8): 562-565.'

(Piketty, 2014).

In spite of the growing consensus of the importance of fairness in understanding distributive decisions and for making efficient public policy design, researchers are far away from agreement about criteria for justice and interpretation of fairness preferences, especially when income inequality is related to individual choice, risk and controversial responsibility. In this chapter, we experimentally investigate redistributive decision-making and fairness preferences under different rooted risks, namely pure luck and option luck.[2] In the first phase of the option-luck treatment, subjects can endogenously determine whether they want to buy insurance to alleviate a potential loss before one of three possible risky outcomes is realized with equal probability. Simply put, subjects have the freedom to make a choice over safer or riskier options. On the other hand, in the pure-luck control condition, subjects are not provided such a chance. Instead the risk distributions are exogenously assigned. The second phase of the pure-luck control condition and the option-luck treatment is the same. Subjects are provided with the opportunity to redistribute their earnings obtained based on their choice and luck. In the redistribution phase, subjects are randomly paired and are asked to redistribute their earnings based on information about their initial choices and final outcomes of the first phase.[3] Participants can choose to redistribute any amount from their earnings (including zero) to their counterparts. By design, therefore, the only difference between the pure-luck control and the option-luck treatment lies in the source of inequality, i.e., the reason subjects lose money is either due to pure luck or to their own discretionary choices.

The experimental design enables us to examine two contradictory views about

[2]Dworkin (2002) defines a distinction between two types of luck adopted in our experimental context. Brute luck, such as genetic inheritance, causes things over which we do not have control, while option luck, as a matter of choice, is something one has control at least partially by deliberation.

[3]More details are included in section 3 of this chapter.

distributive justice by comparing how people respond to income inequality in the pure-luck control condition and in the option-luck treatment. In the view of the accountability principle, the loss from pure luck and option luck should not be equally evaluated. In other words, a person should be responsible for his/her loss due to his/her own choices in the view of the accountability principle. On the other hand, egalitarianism supports higher levels of redistribution, since only the consequential inequality in economic outcomes matters in consideration of fair distribution, namely a discharge of accountability for discretionary choice.

The results support some findings from previous experimental studies that individuals trade off self-interest and fairness allocation (e.g., Fehr and Schmidt (1999), Bolton and Ockenfels (2000), Charness and Rabin (2002), etc). However, our results suggest that people's normative beliefs about fair distribution are driven neither entirely by consequential egalitarianism nor by the accountability principle. Instead, our results reconcile the contradictory views by suggesting that people make redistributive decisions based on the accountability principle in the presence of relatively lower inequality but support egalitarianism in case of large income inequality or salience of extreme low payoffs.

The tradeoff between accountability and egalitarianism captures some important features of prosocial motives and provides some implications for public policies. For example, the Patient Protection and Affordable Care Act evoked a hot debate over the appropriate coverage of health insurance plans from different views of fairness. Chronic diseases such as obesity and cardiovascular disease are sometimes caused by genetic makeup.[4] Consequential egalitarianism and the accountability principle are closer to achieve agreement about fair health insurance policy in this situation. But

[4]Genetic factors are estimated to account for over 40% of the population variation in Body Mass Index (Hjelmborg et al., 2008; Stunkard, Foch, and Hrubec, 1986)

obesity also often results from unhealthy life habits, e.g., night eating, binge eating, and lack of exercise. The accountability principle suggests that obese individuals should pay for their own healthcare if the disease is a result of their own behavior, while a large healthcare coverage including those chronic diseases mainly resulting from people's own choices and behavior is supported from the view of consequential egalitarianism. With respect to the findings from our experiment, most people may follow the accountability principle in the scenario of relatively low inequality, considering that the consequential inequality caused by obesity is relatively small compared to more lethal diseases such as AIDS and terminal cancer. Most people would agree that an HIV infected child deserves medical care and sympathy, since the child is not responsible for mother-to-child transmission. An adult is usually believed to be responsible for his sexual behavior to prevent HIV, and differences in fairness viewpoints related to healthcare coverage may arise under egalitarianism or accountability. In our experiment, salient low income and inequality, parallel to the considerable loss from lethal diseases above, lead people be more inclined to consequential egalitarianism.

The rest of this chapter proceeds as follows. In Section 2 we provide an overview of the relevant literature on fairness and redistribution mainly in moral philosophy and experimental economics. Section 3 describes the design and procedures of our experiment. Section 4 discusses theoretical predictions. Section 5 delivers the main results and discusses the nexus between our findings in the laboratory and reality. Section 6 concludes.

2.2 Literature Review

The studies on justice and fairness can be classified into two general branches, normative analysis and positive analysis. Normative justice theories debate over what standard of justice is appropriate, such as the debate over moral desert between Rawls (1971) and Nozick (1974). Positive analyses including our paper focus on how people actually think and behave. In other words, the goal of this line of work is to contribute to the literature evaluating how accurately different theories interpret people's distributive behaviors regarding fairness. Since public consensus and support are critical in public policy implementation, deeper understanding in people's fairness preferences is helpful for policy makers.

In spite of ubiquity and appeal in our ethical lives, the bases for desert judgment are far from agreement. One of the most influential principles of justice is strict egalitarianism that would always propose an equal split of the earnings even in cases involving work effort and production (Lamont and Favor, 1996). Rawls (1971)'s alternative principle, which he calls Difference Principle relaxes the strong suggestion of strict equality so long as the inequalities in question would make the least advantaged in society materially better off. Rawls and his followers (Pojman, 1997; Rachels, 1978; Rawls, 1971) support the view of presupposed responsibility regarding desert that one cannot claim any credits in virtue of an action or attribute for which one is not responsible. Behind the 'veil of ignorance', people are unable to know their natural endowment including innate aptitude (such as superior intelligence or athletic abilities), inherited social status and wealth, all of which they are not responsible for. Hence people would agree to support equality by a social contract.

However, the nexus between luck and responsibility makes the right basis for desert often controversial. Liberal egalitarianism argues that only inequalities that arise from factors under the individual's control should be accepted. Hence people should be responsible only for their choices not for their luck (Arneson, 1989; Lippert Rasmussen, 2001; Roemer, 2009; Vallentyne, 2002). This principle is equivalent to the accountability principle in the economics literature (Konow, 1996, 2000).[5] Libertarians such as Hayek (1960) and Nozick (1974), on the other hand, refuse to redistribute even if pure luck has a strong impact on inequality in economic outcomes. Accordingly, government intervention should respect liberty and ownership of citizens. Cupit (1996) also challenges the connection between desert and responsibility. Being beautiful or born in a wealthy family are plausibly regarded as bases for desert of various treatments, such as appraise, compensation, and so on, despite the individual not have any accountability associated with it. Meritocracy can also be viewed as an intermediate system in which values are determined by merit, including achievement by effort and intellectual talent.

As economists, we pay more attention to positive analysis. Behavioral and experimental economic studies then examine which of the theories is the fairest from the standpoint of ordinary people rather than philosophers. There is an extensive literature in behavioral and experimental economics regarding social preferences and distributive justice. Laboratory experiments provide ideal control for a large number of potential confounding factors, and build a bridge between philosophical theory and political process.

Social preferences are used to explain the deviations from pure self-interested behaviors in the laboratory and field experiments (Camerer, 2003). There is a continuously growing body of experimental studies on fairness preferences and redistribution; however, there is no consensus among economists over the interpretation of fairness in different contexts, especially when there is ambiguity about desert due to luck, choice, and effort (Konow, 2003). The results from our experiment suggest the

[5] As defined by Konow (2000) (p.1073-74), 'the accountability principle requires that a person's fair allocation (e.g., of income) vary in proportion to the relevant variables that he can influence (e.g., work effort) but not according to those that he cannot reasonably influence (e.g., a physical handicap)'.

dependence of fairness judgment on the distinct contexts, rather than a universal standard.

According to the evidence from experimental economics, the least controversial finding might be that people are more willing to accept income inequality resulting from work effort rather than from windfall (Cherry, Frykblom, and Shogren, 2002; Durante, Putterman, and van der Weele, 2014; Fershtman, Gneezy, and List, 2012; Hoffman et al., 1994; Oxoby and Spraggon, 2008). Earnings from work effort are perceived as more legitimate entitlement than from pure luck. This is in line with Locke (1988)'s labor theory of property rights, which holds that property originally comes about by the exertion of labor upon natural resources. For instance, Hoffman et al. (1994) find that the dictators behave more selfishly if the distributive right is earned by scoring high in a knowledge quiz compared to randomly assigned. Cherry, Frykblom, and Shogren (2002) find almost no sharing when the dictators earned their gains by real effort. Oxoby and Spraggon (2008) report similar results from an experiment in which earnings are determined based on the number of correct answers in exam questions. Fershtman, Gneezy, and List (2012) conducted dictator games and trust games preceded by tedious effort or GMAT exam questions. They also find a stronger tendency for selfish distribution compared to the standard dictator game and trust game. In Durante, Putterman, and van der Weele (2014), individuals' redistribution power is exogenously determined, by performance in a knowledge quiz or by skills in a game, at the same time the experiment allows variations in the efficiency of redistribution and whether redistribution decisions were made ex-ante or ex-post. Their results suggest that when income uncertainty is unfolded, subjects are reluctant to redistribute to their counterparts if they earn money by effort or skill. To investigate how people make tradeoffs between entitlement, needs, and self-interest, Cappelen et al. (2013) conducted a real-effort dictator experiment using student subjects from rich countries (Norway and Germany) and poor countries (Uganda and Tanzania). They find evidence for a mixture of the three motivations in distributive decisions. To see how unequal opportunities affect the demand for distributive justice, Eisenkopf, Fischbacher, and Fllmi-Heusi (2013) conducted an experiment in which subjects earn money from a quiz and then redistribute it among themselves and other participants. They find that unequal opportunities to study some questions in advance evoke stronger preference for redistribution compared to the situation when the allocation is purely determined by luck. But they deny purely meritocratic and purely egalitarian distribution. Parallel to the theoretical framework, some previous experiments (Krawczyk, 2010; Michelbach et al., 2003; Thum and Weichenrieder, 2000) replicate the situation of 'veil of ignorance' (Harsanyi, 1953; Rawls, 1971). For example, in Krawczyk (2010), pre-committed redistribution is strongly supported. Before knowing who would actually win, participants selected transfers plan that the winners will pay to the group. And the average transfers were about 20% lower in the situations in which winning was determined by performance rather than by pure luck. However, his design involves the difficulty for subjects to understand the effects of effort. Additionally, in his experiment, participants have to rely on their fairness concern regarding effort and luck in the group to make redistribution decision. The more sophistication in the design implies more confounding factors.

Our experiment is associated with fairness principles that disagree on responsibility for loss and gain under risk. de Barros et al. (2009) summarize the commonly shared view about income inequality regarding different sources that 'people usually tolerate (and maybe agree with) income inequality arising from differences in choices made, effort extended, and talents put to use by individuals, while they view as fundamentally unfair inequality arising from differences in opportunities.' However, since the multiplicity of desert judgment and fairness views lies in the ambiguity in varying situations, distributive justice and fairness preferences become more complicated and controversial when choice, luck, merit and effort come together.

Dworkin (2002) defines a distinction between two types of luck adopted in our experimental context. Brute luck, such as genetic inheritance, causes things over which we do not have control, while option luck, as a matter of choice, is something one can have a part of control by deliberation. According to the 'common view', people would always perceive responsibility for their own option luck and entitlement for their earnings. However, pure luck does not necessarily mean redistribution, while responsibility does not deny redistribution. Going back to the example in the introduction section, should a patient suffering from obesity-caused diseases be responsible for his/her health insurance cost? Further, does an AIDS patient who acquired HIV by unprotected sex also deserve medical care and equal respect compared to a newborn that contracted the disease from the mother? Our experimental results suggest that the answer to these questions may vary depending on the views on choice and luck, as well as the specific situations. Therefore, it is usually more complicated if there is a nexus between desert and responsibility, especially when the intensity of loss matters.

2.3 Experimental Design and Procedures

There are three sets of treatments and controls in our experimental design. Consider the *Low Income Inequality* (LII) treatment as an illustrative example. Each session in the treatment includes two phases, an insurance purchase choice followed by the distribution phase. In the first phase, given an endowment of 20 points (Experimental Currency Unit, each point=$0.50), participants were informed that one of three possible outcomes would be realized with equal probability. Before the outcome was unfolded, the subjects were asked to decide whether or not to buy an insurance to alleviate potential losses. The insurance cost is 5 points in the LII treatment. As shown in Table 2.1, the subjects were informed that in case of outcome A, 20 points would be delivered regardless of insurance purchase. Hence net payment is 15 points for a participant who purchased insurance, or 20 points for a participant without insurance. Outcome B caused a loss that can be recovered by the insurance. If the subject did not buy the insurance, only 10 points will be delivered. Outcome C is an inevitable loss irrespective of insurance purchase. The subjects received a net payment of 10 points in the case of outcome C. In the experimental sessions, subjects are also shown the payoff table.

Table 2.1: Net Payoff Matrix in the *Low Income Inequality* Treatment

	A	B	C
Purchased insurance	15	15	10
Did not purchase insurance	20	10	10

Therefore, in the treatments, participants can endogenously choose the risk situation they prefer. In the meantime, the freedom to choose implies associated accountability.

During the second phase of the treatments, participants were anonymously and randomly matched with a sequence of eight other participants and the high-income subject in each pair was asked to make redistribution decisions to each counterpart. In case of a tie in earnings, the computer skipped the redistribution task. In each matching, the high-income participant was provided with information about the insurance buying decision of their counterpart, as well as their realized outcomes. Distributors can transfer any amount they want from their earnings, including zero.

Only one of the redistribution outcomes was chosen to be binding. The redistribution outcomes were not shown to everyone until the end of experiment. Therefore, our design excludes wealth effects and reputation effects.

In the corresponding control condition for low income inequality, subjects were not provided with such a chance to buy insurance. The risky scenarios are exogenously assigned to them as pure luck. Each participant was randomly assigned to one of the risky scenarios in Table 2.1. That is, one half of them have 2/3 probability to earn 15 and 1/3 probability to earn 10, while the other half have 1/3 probability to earn 20 and 2/3 probability to earn 10. The redistribution phase in the control groups is the same as the treatment groups. By this design, therefore, the only difference between the control condition and the treatment is the reason why subjects lose money, due to pure luck or by their own choices.

The other two treatments, namely medium income inequality (MII) and high income inequality (HII), are described in the Table 2.2. The difference is the size of the payoffs which result in medium and high income disparities respectively.

Table 2.2: Net Payoff Matrix in MII and HII Treatments

MII Treatment			
	A	B	C
Purchased insurance	15	10	5
Did not purchase insurance	20	5	5
HII Treatment			
	A	B	C
Purchased insurance	10	10	0
Did not purchase insurance	20	0	0

The experiment was conducted at the Economics Research Laboratory (ERL) at Texas A&M University in October and November 2014. The experiment was computerized with the software 'z-Tree' (Fischbacher, 2007). The recruitment was conducted with the Online Recruitment System for Economic Experiments (ORSEE) (Greiner, 2004). A total of 228 participants are students of both genders (85 females) and various majors studying at Texas A&M University. We use a between-subject design. Each subject participated in one session only. Each session lasted approximately 30 minutes. Before entering the experimental laboratory, participants were told that they would receive a show-up compensation fee of $10 upon completion of the tasks and they would also have the potential of extra earnings based on their randomly assigned role, luck and performance. But they were not provided with details of the experiment. The average payment was $16, including the show-up fee. After being seated at separate computer terminals, subjects received written instructions that were also read aloud by the experimenters. To ensure complete understanding by all subjects, a set of test questions that were computerized in z-Tree had to be answered correctly before the experiments began.

2.4 Theoretical Predictions

A variety of social preference based models have been developed to interpret the motives behind phenomena and predict how people respond to different contexts. Outcome-based models of inequality aversion (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999) assume that people's fairness concerns are the final income inequality, irrespective of the causes for inequality. Obviously, outcome-based models predict the same redistribution in the pure-luck control and option-luck treatment in case of the same income distribution in the first phase. Further, within the option-luck treatment, the redistributive decision makers will not consider the difference between facing a counterpart suffering from an avoidable loss (event B) and another counterpart losing the same amount of money due to an unavoidable loss (event C). For example, participant 1 did not purchase insurance and received 20 points because of good luck (event A). Participant 2 purchased insurance but received 10 point because of event C. Participant 3 did not purchase insurance but also received 10 point because of event B, in which case the loss could be avoided by purchasing insurance. The outcome-based models predict no discrepancy in redistribution between the matched pair of participant 1 and 2 and the pair of participant 1 and 3.

However, those models were challenged by social preference models based on intention and procedures and supported by plentiful experimental evidence (Charness and Rabin, 2002; Cox, 2004; Dana, Cain, and Dawes, 2006; Dufwenberg and Kirchsteiger, 2004; Engelmann and Strobel, 2004; Ku and Salmon, 2013; Rabin, 1993). Therefore, this strand of social preference models predicts diverse fairness views about the same income distribution with different sources of inequality.

Similarly, the accountability principle of fairness draws attention from behavioral and experimental economists (Becker, 2013; Cappelen et al., 2007; Konow, 1996, 2000, 2001; Moellerstroem, Reme, and Sorensen, 2015). According to the accountability principle, exogenously assigned and endogenously chosen risks should not be equally evaluated. And neither should be the loss from avoidable risks and pure bad luck. In other words, a person should be responsible for his loss from his own choice that affect risk distribution and associated material loss. Therefore, the redistributive decisions made by subjects can be used to measure the extent to which people believe they should be responsible for their own option-luck.

2.5 Results

2.5.1 Decisions in the First Phase

A total of 78 participants were assigned into the pure-luck control group, while 150 subjects participated in the treatment sessions (option-luck treatment). Table

2.3 shows that 111 (74%) of 150 participants in the treatment group chose the safer option. In our LII treatment and MII treatment, about two-thirds of subjects chose to buy insurance. Not surprisingly, in the HII treatment where the potential loss resulting from not buying insurance is higher, 47 out of 56 subjects (84%) chose to buy the insurance.

Table 2.3: Insurance Purchase Choices Made by Treated Subjects in the First Phase

	Insurance purchase		
	Yes	No	Total
Low Income Inequality	20	10	30
Medium Income Inequality	44	20	64
High Income Inequality	47	9	56
Total	111	39	150

2.5.2 Redistributive Decisions

We then further our analysis by comparing the overall average distributive decisions made by subjects in the second phase of the controls and treatments. The histograms in Figure 2.1 depict the distributions of transfers made by the distributors. The individuals trade off fairness and self-interests. People are reluctant to make equal earnings in both of the controls and treatments. The distinction between the controls and treatments is clear. Overall, the distributors in the treatments did not transfer to their counterparts at all amongst over 60% of transfer decisions. And about one fifth of transfers were less than 20% of gross earnings before redistribution. In contrast, the distributors made significantly higher transfers to counterparts in the control groups. Slightly over 40% of transfers were zero, while more than 40% of transfers were equal or more than 20% of their gross earnings in the first phase. The lower panel of Figure 2.1 conveys similar information by depicting the distribution of absolute transfers (in ECU) instead of percentage of transfers out of pre-distributed earnings.

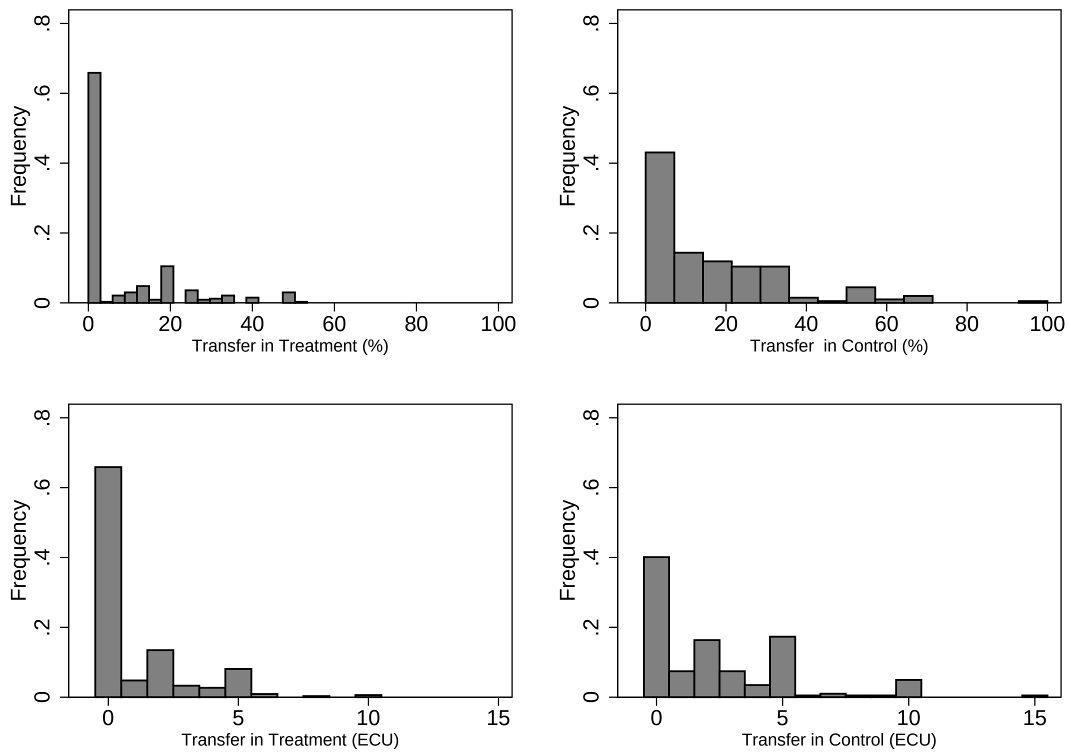

Figure 2.1: Transfer Histograms

Comparison between the pure-luck controls and option-luck treatments are provided in Figure 2.2 and Table 2.4. We again find sharp evidence for the different distributive behaviors with or without the opportunity to protect against risk by purchasing insurance. The average amount of transfer was about 2.38 ECUs (or 15.54% of gross earnings) in the pure-luck control condition, significantly higher than the average transfer in the option-luck treatment, 1.07 ECUs (7.77%) ($p < 0.001$, Mann-Whitney U-test, clustered by subject).

A further investigation into the additional income inequality pure-luck controls and option-luck treatments show more interesting results. The largest disparity of transfer between the option-luck treatment and the pure-luck control lies in the low-income inequality treatment (1.44% vs. 20.73%, 0.22 vs. 3.20 ECUs, $p < 0.001$, Mann-Whitney U-test, clustered by subject). Nevertheless, the differences in medium-income inequality and high-income inequality become much smaller and insignificant. Comparing the distribution between the pure-luck control and option-luck treatment in MII and HII pairs, the differences in transfer amount between are less than 1 ECU and insignificant. This lends support to the notion that people have a stronger tendency to support the accountability principle when the potential income gap is relatively small.

Table 2.4: Transfers Comparison between Controls and Treatments

	Transfer (%)			Transfer (ECU)		
	Treatment	Control	p-value	Treatment	Control	p-value
Total	7.77	15.54	0.001	1.07	2.38	0.001
	N=334	N=202		N=334	N=202	
LII	1.44	20.73	0.000	0.22	3.20	0.000
	N=58	N=50		N=58	N=50	
MII	8.46	13.56	0.330	1.32	2.14	0.271
	N=157	N=72		N=157	N=72	
HII	9.96	14.06	0.069	1.14	2.09	0.115
	N=119	N=80		N=80	N=80	

Notes: p-values are reported for Mann-Whitney U-tests clustered by subject.

However, these comparisons do not refer to the specific level of income inequality. Table 2.5 contains the results according to the specific income inequality. When

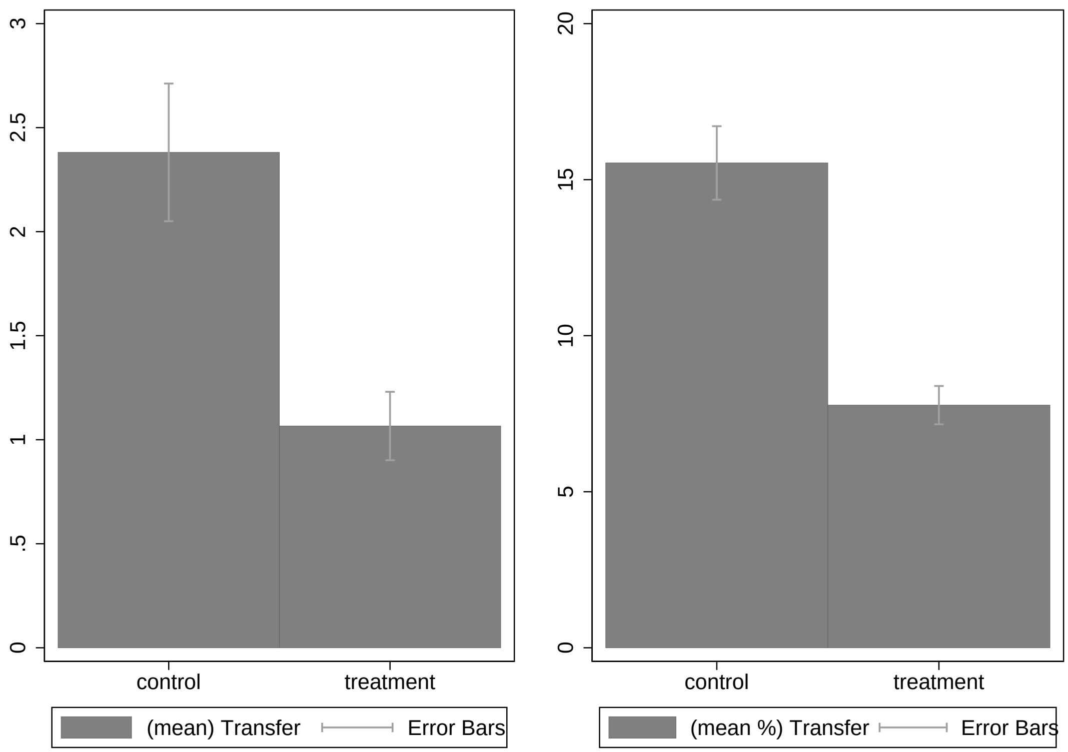

Figure 2.2: A Comparison of Transfer in ECU between Control and Treatment

the income gap is 5 ECUs, the average amount of transfer was about 2.60 ECUs (or 18.42% out of gross earnings) in the pure-luck control, significantly higher than the average transfer in the option-luck treatment, 0.46 ECUs (3.67%) ($p < 0.001$, Mann-Whitney U-test, clustered by subject). When the income gap is 10 ECUs, the differences in transfer between the pure-luck control and option-luck treatment become considerably closer and insignificant ($p > 0.1$, Mann-Whitney U-test, clustered by subject). And the difference becomes negligible when the income inequality is greater than 10 ECUs. The significant difference in redistribution between the pure-luck control and the option-luck treatment is also found when the distributors face counterparts who earned more than 0. In these scenarios, the average amount of transfer was about 2.64 ECUs (16.83%) in the pure-luck control, significantly higher than the average transfer in the option-luck treatment, 1.02 ECUs (6.47%) ($p < 0.002$, Mann-Whitney U-test, clustered by subject). However, these differences become insignificant when the receivers earned zero from the risk-taking phase.

Table 2.5: Transfers Comparison between Controls and Treatments

	Transfer (%)			Transfer (ECU)		
	Treatment	Control	p-value	Treatment	Control	p-value
$\Delta = 5$	3.67	18.42	0.000	0.46	2.60	0.000
	N=130	N=73		N=130	N=73	
$\Delta = 10$	9.75	13.37	0.144	1.24	1.92	0.105
	N=181	N=91		N=181	N=91	
$\Delta>10$	15.43	15.13	0.905	3.09	3.02	0.905
	N=23	N=39		N=23	N=39	
PI=0	10.51	13.63	0.251	1.17	1.79	0.192
	N=108	N=62		N=108	N=62	
PI>0	6.47	16.38	0.001	1.02	2.64	0.002
	N=226	N=140		N=226	N=140	

Notes: Δ is income gap. PI is partner's income from the first phase. p-values are reported for Mann-Whitney U-tests clustered by subject.

A potential interpretation of the findings from our experiment is that most people evaluate fairness by multiple principles, one of which may dominate depending on the income gap and salience of related factors.

2.5.3 Econometric Analysis

In order to provide econometric evidence, we employ the model of fairness preferences proposed by Cappelen et al. (2007) that allows flexible forms of fairness views. We adapt their model in the context of our experiment as:

$$V = y - (y - F^t)^2/2\beta X^2$$

where the interior solution y^* is given by $y^* = F^t + \beta X^2$. V_i is a utility function of individual i; y_i is the post-distribution payoff of individual i; β measures tolerance for advantageous inequality; and X is the total earning in the group. We assume that the distributors endorse either the accountability principle (AP) or consequential egalitarianism (CE). Accordingly F_i^t is the fairness reference point for individual i of type t, either F^{AP} or F^{CE}. For CE preference, the fairness reference point is the average payoff in the group, i.e., the utility function is the same as Bolton and Ockenfels (2000). Since there is no room for option luck in the pure-luck control group, the disparity between AP and CE preference does not exist. AP preference is relatively complicated in the option-luck treatment group. While CE distributors' fairest transfer is such that makes equal earnings, AP distributors discount this amount by the relative weights of pure-luck and option-luck. To be specific, consider an example in the high-income inequality pure-luck control and option-luck treatment. Due to the random group assignment and pure luck in the high-income inequality control, participants have a 1/6 probability to earn 20 points, 1/3 probability to earn 10 points, and 1/2 probability to earn 0 points. Accordingly, the probability to see a pair of a distributor earning 20 points and a receiver earning 0 points is 1/12. F_i^{CE} is 10 points in this case. Rather, consider another pair of participants who also earned 20 points and 0 points in the first phase of the high-income inequality treatment. The distributor endogenously changed the probability of earning 20 points from 1/6 to 1/3 by not buying insurance, while the receiver changed the probability of earning nothing from 1/2 to 2/3. So despite of the same income distribution in the pure-luck control and option-luck treatment, the probability of this distribution is 1/12 in the pure-luck control and 2/9 in the option luck treatment. Hence AP distributors' fairest transfer should be discounted by the relative weight of pure luck, 3/8 (1/12 out of 2/9) in this treatment. Similarly, if the receiver bought insurance, then the discount factor is 3/4 (1/12 out of 1/9). We summarize the decomposition of pure-luck and option-luck in Table 2.6. Then the calculation of F^{CE} and F^{AP} is straightforward. For brevity, we only show the decomposition in the high-income inequality control and treatment.

Table 2.6: Decomposition of pure luck and option luck (HII)

Person 1/2's earning		Person 2 insurance	Prob. in the pure luck control	Prob. in the option luck treatment	Discount Factor
20	0	Yes	1/12=1/6*1/2	1/9=1/3*1/3	3/4
20	0	No	1/12=1/6*1/2	2/9=1/3*2/3	3/8
20	10	Yes	1/18=1/6*1/3	2/9=1/3*2/3	1/4
10	0	Yes	1/6=1/3*1/2	2/9=2/3*1/3	3/4
10	0	No	1/6=1/3*1/2	4/9=2/3*2/3	3/8

The model was estimated as:

$$y^* = F^t + \beta X^2 + \varepsilon$$

In the econometric model, there are two key parameters of interest that are needed for identification, β (the measure of tolerance for unfairness) and λ (the likelihood of an individual to have a fairness view of accountability principle). To capture the heterogeneity of β, we assume that β is normally distributed, s.t. $\beta \sim N(\overline{\beta}, \sigma_{\mu}^2)$. And the idiosyncratic error term is also assumed to be independently normally distributed, i.e, $\varepsilon \sim N(0, \sigma_{\varepsilon}^2)$. The likelihood contribution of a distributor is calculated by the combination of the likelihood of two views of fairness principles.

$$L_i = \lambda L_i^{AP} + (1-\lambda) L_i^{CE}$$

Table 2.7 reports the results. When the income gap is 5 points, the low value of β implies less tolerance for unfair allocation, and the likelihood of a distributor being classified to have a view of accountability principle is 74%. If the income gap is 10 points, the distributors care less about fairness ($\beta = 0.017$). Meanwhile, the likelihood of a distributor having a view of accountability principle reduces to 63%. Further, this likelihood is weakly significant if the income gap is over 10 points. Similarly, if the earning of the receiver is positive in the first phase, then β is very low at 0.002, while the likelihood of the distributor having an AP view is very high (82%). But when the distributor faced a counterpart earning nothing in the first phase, he/she is only 32% likely to be classified to hold AP view of fairness.

2.5.4 Robustness

To check whether our estimates are robust to the utility functional form, we also estimate the model with the utility functional form described in Cappelen et al. (2013)'s main specification:

$$V = y - (y - F^t)^2 / 2\beta X$$

Table 2.7: Estimates of β and λ (main specification)

	(1)	(2)	(3)	(4)	(5)
Parameter	$\Delta = 5$	$\Delta = 10$	$\Delta > 10$	PI>0	PI = 0
β	0.001**	0.017***	0.010***	0.002***	0.032***
	(0.001)	(0.002)	(0.002)	(0.000)	(0.003)
λ	0.744***	0.634***	0.092	0.816***	0.323**
	(0.205)	(0.141)	(0.243)	(0.127)	(0.130)
-LL	394.179	601.333	147.959	824.764	304.239
Obs	203	271	62	366	170

Notes: Standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

We report the results in Table 8. Although the estimates are different in magnitude from our main specification, they show the same direction and are consistent with our conclusions.

Table 2.8: Estimates of β and λ (Robustness Check)

	(1)	(2)	(3)	(4)	(5)
Parameter	$\Delta = 5$	$\Delta = 10$	$\Delta > 10$	PI>0	PI = 0
β	0.042***	0.217***	0.230***	0.063***	0.358***
	(0.013)	(0.023)	(0.037)	(0.012)	(0.024)
λ	0.483***	0.454***	0.070	0.570***	0.215*
	(0.213)	(0.134)	(0.237)	(0.133)	(0.127)
-LL	385.761	564.231	145.612	802.550	296.536
Obs	203	271	62	366	170

Notes: Standard errors in parenthesis. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

2.6 Concluding Remarks

This chapter has reported the results of a laboratory experiment investigating how people respond to different sources of risks in consideration of distributive justice. Our findings confirm some results from previous experimental studies that individuals trade off self-interest and fairness allocation and tend to understand fairness based on the accountability principle sometimes. By comparing the distributive decisions with respect to exogenously and endogenously chosen risks (e.g., pure-luck versus option-luck), our experiment also sheds light on the conflict between consequential egalitarianism and the accountability principle in explaining redistributive behaviors. Rather than completely supporting consequential egalitarianism or accountability principle, our results suggest reconciliation between them. People show a strong tendency for the accountability principle in case of relatively lower inequality but exhibit a tendency to consequential egalitarianism in case of large income inequality or salience of extreme low payoff. Therefore, fairness preferences and justice judgments should be understood in specific contexts varying in the choice, luck, income gap, and so on.

Returning to the normative literature, our experimental results show that people support an integration of Rawls' difference principle and libertarian egalitarianism in consideration of fairness. Regarding the policy implications, the importance of helping the people in need is supported, even though the needy people's misfortune is largely determined by their own faults. As discussed in the introduction AIDS and lung cancer patients should also be helped and respected even though the misfortune came out of bad life habits. In contrast, the redistribution from high income people to medium income people depends on the context richness. In these situations, the distributive policy making needs to consider the accountability principle for fairness. Consider the obesity example where most of the chronic diseases associated with obesity are not lethal. The public is reluctant to share the health insurance costs with obese people who have unhealthy life habits.

There are a few open questions yet answered in this paper. This experiment was conducted with college students in the U.S. As discussed in Alesina and Angeletos

(2005), European countries have a stronger preference for redistribution than North-American societies due to their different beliefs regarding the role of luck and effort in determining wealth. Thus it would be interesting to examine whether there are different fairness preferences in other societies.

In addition, although our findings are convincing, the underlying motivation of redistributive decisions is subject to other interpretations. An alternative explanation is moral wiggle room (self-serving bias) that hypothesizes that people may exploit the veil of ambiguity in the distributional situations as moral excuses to endorse their own self-interest by sacrificing fairness even though they do not really believe the moral excuses (Bolton, Brandts, and Ockenfels, 2005; Dana, Weber, and Kuang, 2007). An investigation of distributive decisions made by spectators would be meaningful to answer this question. Moellerstroem, Reme, and Sorensen (2015) report the result from a similar laboratory experiment with only redistributions made by spectators. Indeed, in absence of personal stakes, the impartial spectators are much more willing to make equal post-distribution earnings.

2.7 Appendix: Experimental Instruction

2.7.1 General Instruction

This part is the same for every subject.

Welcome to this experiment! Before the session begins, you will carefully read the basic instruction in 20 minutes. Please feel free to ask questions if you are confused. You will have to answer some questions to check that you understand the instructions. During the experiment you are not allowed to communicate with other participants. If you have a question, raise your hand. We will come to answer your questions. Sometimes you may have to wait a short while before the experiment continues. Please be patient. Thanks for your patience and cooperation.

Your earnings in the experiment will be calculated in points. Points will be converted to US dollars at the exchange rate shown on the screen. Upon the completion of the experiment, you will also receive a participation fee of $10. At the end of the experiment your total earnings will be paid out to you in cash.

Following the main choice tasks, we will ask you some questions about your socio-demographic characteristics. Your responses are helpful in that they can be used to explain some of the decisions you make in the experimental exercises. Please note that your responses will not be linked to your name, nor made available to anyone outside the research team. Your ID number is used to match your responses so that they are not confused with anyone else's, and will be used to determine your earnings from the experiment.

We ask that you not talk with anyone else today except for the designated researchers conducting this experiment.

We expect that the entire session will take less than one hour. Your participation is completely voluntary. You may ask questions at any time during the experiment.

2.7.2 Instruction for the First Phase of HII Treatment[6]

The instructions were handed out to the participants and also were shown on the computer screen.

Please read the instructions on your desk and screen. To make sure you completely understand the procedures, you will answer some questions. Once you are ready, please click on the red button below and answer the questions on the screen.

The three following situations will be realized with equal probability. Before the

[6]For sake of brevity, we do not provide the instructions for MII- and LII-treatment and control, which are quite similar with HII treatment and control.

outcome is unfolded, you are asked to decide whether or not to buy an insurance that costs 10 points.

A: You will receive 20 points minus the insurance cost (if you buy it).

B: You will have an avoidable loss contingent on whether the subject buys insurance. If you buy the insurance, you will receive 20 points minus the insurance cost. Otherwise, you will receive 0 point.

C: You will receive 0 point regardless of whether or not you buy insurance.

Exchange rate: 2 points=$1

	A	B	C
Having insurance	10	10	0
Not having insurance	20	0	0

Once the participants clicked ready button, they will answer some simple questions in z-Tree before going to the next step.

Q1. Suppose event A happened. You did not purchase insurance. How many points do you earn?

Q2. Suppose event B happened. You bought insurance. How many points do you earn?

Q3. Suppose event C happened. You did not insurance. How many points do you earn?

2.7.3 Instruction for the Second Phase of HII Treatment

Participants did not see this instruction until the end of the first phase.

Welcome to the second phase of this experiment. In this phase, you may have the opportunity to redistribute earnings between you and your counterpart. During the second phase of the experiment, you will be anonymously and randomly matched with a sequence of eight other participants. In each scenario, you will be asked to make redistribution decisions only if your earning in the first phase is higher than your counterpart. Therefore, please be patient if you are not the decision maker. Your counterpart will not see who make the decision. Your counterpart will not know the redistribution results until the end of this experiment. Only one scenario will be randomly chosen to distribute payment.

The distributors received the following information on screen while making distributive decisions. The following is an example.

Remember that (You bought the insurance.) Event B happened to you.
Now you are matched with a counterpart. (Your counterpart did not buy the insurance.) Event B happened to your counterpart.
Your income before redistribution is 10 points. Your counterpart's income before redistribution is 0 points.

Please input the amount you will give to your counterpart. You can input any integer number between 0 and your earnings inclusive.

2.7.4 Instruction for the First Phase of HII Control

One half of participants in this room will be randomly chosen to face scenario one, while the other half will face scenario two.

In case of scenario one, you will face three possible outcomes with equal probability. The table shows the payoff of each outcome. Exchange rate: 2 points=$1.

A	B	C
10	10	0

In case of scenario two, you will face three possible outcomes with equal probability. The table shows the payoff of each outcome. Exchange rate: 2 points=$1.

A	B	C
20	0	0

2.7.5 Instruction for the Second Phase of HII Control

Participants did not see this instruction until the end of the first phase.

Welcome to the second phase of this experiment. In this phase, you may have the opportunity to redistribute earnings between you and your counterpart. During the second phase of the experiment, you will be anonymously and randomly matched with a sequence of eight other participants. In each scenario, you will be asked to make redistribution decisions only if your earning in the first phase is higher than your counterpart. Therefore, please be patient if you are not the decision maker. Your counterpart will not see who make the decision. Your counterpart will not know the redistribution results until the end of this experiment. Only one scenario will be randomly chosen to distribute payment.

The distributors received the following information on screen while making distributive decisions. The following is an example.

In the first phase, your counterpart had 2/3 chance to receive 10 points, and 1/3 chance to receive 0 point.

Your income before redistribution is 20 points. Your counterpart's income before redistribution is 10 points.

Please input the amount you will give to your counterpart. You can input any integer number between 0 and your earnings inclusive.

3. AN EXPERIMENTAL STUDY ON COEVOLUTION OF RISK PREFERENCES AND FAIRNESS PREFERENCES: A PANEL VAR ANALYSIS

We explore the dynamics of a laboratory economy in which risk preferences, fairness preferences, and income inequality are jointly determined under different rules. In each of twenty periods of the Partner treatment, subjects choose between a safe and a risky option and then make redistributions within the fixed paired subjects, while in the Spectator treatment stakeholders make the same choice tasks but a third party makes the redistributions. The different dynamic interplays between the two treatments are examined based on a panel vector auto-regression model. We find that in the Spectator treatment, stakeholders do not affect each other, but make decision relying on the spectators' decisions. By contrast, in the Partner treatment, stakeholders show a stronger interplay in reciprocity and choices under risk.

3.1 Introduction

Justice and fairness have been of essential concern in the fields of moral philosophy and political science and began to evoke growing interests from mainstream economics starting in the early 1990s (Konow 2003, Rabin 1993). The distribution of income and wealth can significantly affect the stability and potential for development in a society, due to disagreements about what constitutes a 'fair' allocation and appropriate redistribution of income and wealth (Alesina and Angeletos 2005, Alesina and Glaeser 2004, Alesina and Rodrik 1994, Benabou and Tirole 2006). Social preferences are a central part of human culture. Guiso, Sapienza, and Zingales (2006) define social preferences as 'those customary beliefs and values that ethnic, religious, and social groups transmit fairly unchanged from generation to generation.' Culture means beliefs about the consequences of one's actions, or values and preferences. There is a large body of literature related to the interplay between institutions, culture, and economic outcomes. Institutional views assert strong causal effects of institutional quality on economic performance (Acemoglu, Johnson, and Robinson, 2001, 2005; North, 1981). Further, institutions can also affect economic inequality and redistribution (Alesina and Rodrik, 1994; Besley and Burgess, 2002). But the same institutions could function very differently in different cultures. Recent literature documents the role of culture in economic performance (Guiso, Sapienza, and Zingales, 2006; Greif and Tabellini, 2010) and institutional formation (Tabellini, 2010; Gorodnichenko and Roland, 2015; Greif and Tabellini, 2010). On the contrary, some studies show evidence that institutional arrangements can shape culture (Alesina and Fuchs-Schündeln, 2007). A recent set of experiments also suggest that institutions affect behavior and beliefs (Dal Bó, Foster, and Putterman, 2010; Sutter, Haigner, and Kocher, 2010; Kamei, Putterman, and Tyran, 2015).

This study contributes to the literature by implementing an experimental investigation of the dynamic evolution of an economy in which fairness preferences, redistributive policies, and income inequality are jointly determined under different institutional arrangements. In each of twenty periods of the '*Partner treatment*', a fixed pair of partners chooses between a safe and a risky option in the first phase and then make redistributive decisions in the second phase. A second '*Spectator treatment*' consists of three subjects in each group; two stakeholders make the same choice tasks during the first phase, while a *spectator* makes redistributive decisions in the second phase. The theoretical and empirical implications can be reexamined in a laboratory experiment where the confounding factors can be more easily removed. However, in a dynamic economy, people's social preferences and redistributive behavior can be mutually determined, and they are also affected by social preferences and redistribution made by others. Therefore, we investigate the coevolution of fairness preferences and risk preferences by estimating dynamic parameters through a panel VAR approach (pVAR), which treats all the variables in the system as endogenous and interdependent, providing a deeper understanding of the direction of causality. Directed acyclic graphs (DAG) are used to summarize the relationship among the variables in the pVAR estimation.

We find different patterns of interplay between subjects under the two redistributive institutions. On the one hand, in the *Spectator treatment*, the spectators can use redistribution as a tool to affect choices made by the stakeholders. In particular, greater redistribution leads to higher likelihood of stakeholders choosing the safe option. But the spectators' fairness preferences are not affected by shocks of the choices made by the two stakeholders. A positive shock of a stakeholder' income escalates the redistributive behaviors of spectators. Further, the stakeholders are not affected by how their counterparts make choices. On the other hand, in the *Partner treatment*, paired stakeholders have a much stronger tendency to choose the same option over time. Further, they show some reciprocity. Partners also adjust their choices according to the observed inequality aversion of their counterparts. We also find evidence that partners' fairness preferences can be partially changed by their counterparts' choices but not their incomes.

The rest of the paper proceeds as follows. Section 2 describes the design and procedures of our experiment. Section 3 provides a simple theoretical model. Based on this model, we introduce the methodology of panel VAR and estimate the dynamics of key parameters in section 4. We also deliver the causality direction by DAGs and impulse responses from pVAR estimates. Section 5 concludes.

3.2 Experimental Design and Procedures

Our experiment includes four treatments, namely Partner ONE, Partner TWO, Spectator ONE and Spectator TWO, distinguished by the payment amount of the safe alternative and the assignment of redistributive power. Consider the Partner One condition as an illustrative example first. At the beginning of the Partner ONE treatment, each participant is told that they will be anonymously and randomly matched with another participant for the whole session that last 20 periods. Each period includes two phases, a phase of risk choice followed by the distribution phase. In the first phase of each period, each participant is asked to choose either a safe alternative (S) that pays 4 points (Experimental Currency Unit, each point=$0.20) or a risky alternative (R) that will deliver either nothing or 10 points with equal probability. In the second phase of each period, after revealing all choices, realized outcomes and cumulative wealth for both counterparts in the previous periods, the subject is asked to decide how many points he/she would distribute to his/her counterpart if his/her earnings in the current period are higher than his/her counterpart. The redistribution procedure is skipped in case of equal earnings. The partner TWO treatment is all the same except that the payment of the safe alternative is 5 points instead of 4 points.

In the Spectator ONE treatment, three participants are anonymously and randomly assigned into a group for the whole session of 20 periods. Two group members are randomly chosen to be stakeholders who only participate in the first phase of each period, while the third group member is assigned to be a spectator only participating in the second phase of each period. They remain in the same role for the whole session of the experiment. In the first stage of each period, the two stakeholders face the same decisions in the Partner One treatment. In the second phase of each period, the spectator makes redistributive decisions from the high income stakeholder to the low income stakeholder. The redistribution procedure is skipped in case of equal earnings. The redistributive decisions are then shown to the stakeholders afterward. Spectator TWO treatment uses the same design only except that the payment of the safe alternative is 5 points. The screenshots of redistribution phase in zTree are shown in the appendix.

The experiment was conducted in the Economics Research Laboratory (ERL) at Texas A&M University during October and November in 2014. The experiment was computerized with the software 'z-Tree' (Fischbacher 2007). The recruitment was conducted with the Online Recruitment System for Economic Experiments (ORSEE) (Greiner 2004). A total of 228 participants are Texas A&M University students of both genders (85 females) and various majors. In a between-subject design, each subject only participated in one session. Each session lasted about 60 minutes. Before entering the experimental laboratory, participants were told that they would receive a show-up fee of $10 upon completion of the tasks and they would also have the potential of extra payoffs based on their randomly assigned role, luck and performance. But they were not provided with the details of the experiment. The average payment for stakeholders was $29.6, including the show-up fee. The spectators received $30 (including the show-up fee) for the whole session regardless of their decisions.

After being seated at separate computer terminals, subjects received written instructions that were also read aloud by the experimenters. To ensure complete understanding by all subjects, a set of test questions computerized in z-Tree needs be correctly answered before the experiments began.

3.3 Model and Measurement

In order to explore the joint dynamics of fairness preferences and risk preferences under different redistributive institutions in our experimental context, we introduce a simple model that defines key parameters used for further analysis.

3.3.1 Partner Treatment

We first describe the decision making in the *Partner treatment*. In the second phase of each period, an individual is assumed to be motivated by his own earning and fairness preferences when redistributing the total income earned in the first stage with his counterpart. Bolton and Ockenfels (2000) provide a utility function based on the assumption of inequality aversion. In a two-person game, e.g., *Partner treatment* in our experiment, the utility of person i in each period is

$$U_i(y_i, X) = y_i - (1/2 - y_i/X)^2/\delta_i \tag{3.1}$$

where y_i is the post-redistribution monetary payoff for the high income stakeholder i, X is the total payoff in the group, y_i/X is player i's relative payoff. The second term of the utility function captures the psychological loss from the payoff deviating from equal share within the group. The parameter δ_i measures the tolerance for inequality. By assumption, if δ_i is very close to zero, the function is maximized when $y_i/X = 1/2$, i.e., equal split of group earnings. Note that the function is concave, implying risk aversion. The interior solution of y_i^* is then given by

$$y_i^* = y_i' - t_i = X/2 + \delta_i X^2/2 \tag{3.2}$$

Then we can identify δ_i according to the stakeholder i's initial earning y_i', transfer amount t_i and total group earnings X. For example, if stakeholder 1 earned 10 points in the first phase, while his counterpart stakeholder 2 earned 0 points in the first phase. That is, y_1'=10, X=10. Stakeholder 1 transferred 3 points to stakeholder 2, i.e., t_1=3, y_1^*=7. We obtain δ_1=0.04.

In the first phase of each period, the stakeholders make choices between the safe and risky alternative. In order for the stakeholders to maximize expected payoff, their strategy on choosing S or R alternative depends on their prior and updated belief of other group members' fairness preferences.

In the Partner One treatment, the payoff structure is described by the utility functions below. Denote by c_1 and c_2 the choices of a stakeholder and his/her counterpart in the same group, either S or R. $U(c_1, c_2)$ is the utility function of the stakeholder. t_1 and t_2 denote transfers of the stakeholder and his/her counterpart in the same group.

$$U(S,S) = 4 \tag{3.3}$$

$$U(S,R) = 0.5[4-t_1-[1/2-(4-t_1)/4]^2/\delta_1]+0.5[4+t_2-[1/2-(4+t_2)/14]^2/\delta_1] \tag{3.4}$$

$$U(R,S) = 0.5[10-t_1-[1/2-(10-t_1)/14]^2/\delta_1]+0.5[t_2-[1/2-t_2/4]^2/\delta_1] \tag{3.5}$$

$$U(R,R) = 2.5+0.25[10-t_1-[1/2-(10-t_1)/10]^2/\delta_1]+0.25[t_2-(1/2-t_2/10)^2/\delta_1] \tag{3.6}$$

Assuming that both stakeholders maximize their utility, we obtain that

$$t_i(4,0) = 2 - 8\delta_i \tag{3.7}$$

$$t_i(10,4) = 3 - 98\delta_i \tag{3.8}$$

$$t_i(10,0) = 5 - 50\delta_i \tag{3.9}$$

And then

$$U(S,S) = 4 \tag{3.10}$$

$$U(S,R) = 4.5 + 2\delta_1 - 49\delta_2 - 24.5\delta_2^2/\delta_1 \tag{3.11}$$

$$U(R,S) = 4.5 + 24.5\delta_1 - 4\delta_2 - 2\delta_2^2/\delta_1 \tag{3.12}$$

$$U(R,R) = 5 + 6.25\delta_1 - 12.5\delta_2 - 6.25\delta_2^2/\delta_1 \tag{3.13}$$

Similarly, in the Partner Two treatment, we obtain

$$U(S,S) = 5 \tag{3.14}$$

$$U(S,R) = 5 + 3.125\delta_1 - 56.25\delta_2 - 28.125\delta_2^2/\delta_1 \tag{3.15}$$

$$U(R,S) = 5 + 28.125\delta_1 - 6.25\delta_2 - 3.125\delta_2^2/\delta_1 \tag{3.16}$$

$$U(R,R) = 5 + 6.25\delta_1 - 12.5\delta_2 - 6.25\delta_2^2/\delta_1 \tag{3.17}$$

So far we have completed the description of the payoff structure of the Partner treatment in a single period. The equilibrium is jointly determined by two stakeholders' own fairness preferences and their expectation of their counterparts' fairness preferences. We can obtain their fairness preference parameter according to their decisions. In each period, partners adjust their belief based on their counterpart's alternative choices and redistributive decisions over the previous periods. We assume that their own fairness preferences are also dynamically updated based on historical observations over the previous periods.

3.3.2 Spectator Treatment

A disadvantage of the model above is that it is unable to identify fairness preferences of the spectators, who do not trade off their own payoff and payoff distribution of two stakeholders. Thus we need a second measure of inequality aversion: $\beta = 2t/\Delta y$, where t is the transfer amount and Δy is the income gap before redistribution, hence β is between 0 and 1. β equals 0 if the distributor is not inequality averse, and 1 if the distributor prefers an equal share between the two stakeholders. This measure is suitable for inequality aversion of distributors in both the Partner- and Spectator- treatments.

The payoff structure of the Spectator treatment is described as follows. Take Spectator One as an example. If the utility function of stakeholders is assumed to be

$$U_i(y_i, \sigma_i) = y_i - (1/2 - y_i/X)^2/\delta_i$$

then by maximizing utility we obtain:

$$U(S,S) = 4 \tag{3.18}$$

$$U(S,R) = 4 + \beta/2 - 29(1-\beta)^2/196\delta_1 \tag{3.19}$$

$$U(R,S) = 5 - \beta/2 - 29(1-\beta)^2/196\delta_1 \tag{3.20}$$

$$U(R,R) = 5 - 0.125(1-\beta)^2/\delta_1 \tag{3.21}$$

Similarly, we obtain the utility function of stakeholders in the Spectator Two treatment

$$U(S,S) = 5 \tag{3.22}$$

$$U(S,R) = 5 - 0.14(1-\beta)^2/\delta_1 \tag{3.23}$$

$$U(S,R) = 5 - 0.14(1-\beta)^2/\delta_1 \tag{3.24}$$

$$U(R,R) = 5 - 0.125(1-\beta)^2/\delta_1 \tag{3.25}$$

On the one hand, spectators can use redistribution to affect the choices made by the stakeholders over the next periods. On the other hand, spectators adjust redistributive decisions according to the choices made by the stakeholders in the previous periods.

3.3.3 General Comparison

Before the main panel VAR analysis, we first deliver some simple statistics from general comparison between treatments. Figure 1 displays the risk-taking behaviors of stakeholders in four treatments over 20 periods. In the Partner One- and Spectator One- treatment, the payment of the safe alternative is relatively lower. Panel A contains the ratio of both stakeholders choosing the risky option in four treatments over 20 periods. Again, more pairs of subjects choose the risky alternative compared to the Partner Two- and Spectator Two-treatments (54% vs. 27%, $p < 0.001$, two-way clustered by subject and period). Depicted in Panel B of Figure 3.1, subjects show significantly higher tendency to choose the risky alternative compared to Partner Two- and Spectator Two-treatments (72% vs. 49%, $p < 0.001$, two-way clustered by subject and period). However, subjects do not show significantly different risk-taking behaviors between the two redistributive institutions.

Figure 3.2 draws the trends of fairness preferences measured by β and δ. Panel A shows that the average β over 20 periods in the four treatments are: $\beta_{partnerone} = 0.43$, $\beta_{leviathanone} = 0.43$, $\beta_{leviathantwo} = 0.39$, $\beta_{partnertwo} = 0.17$. Only inequality aversion in the Partner Two treatment is significantly lower ($p < 0.05$ for three comparisons,

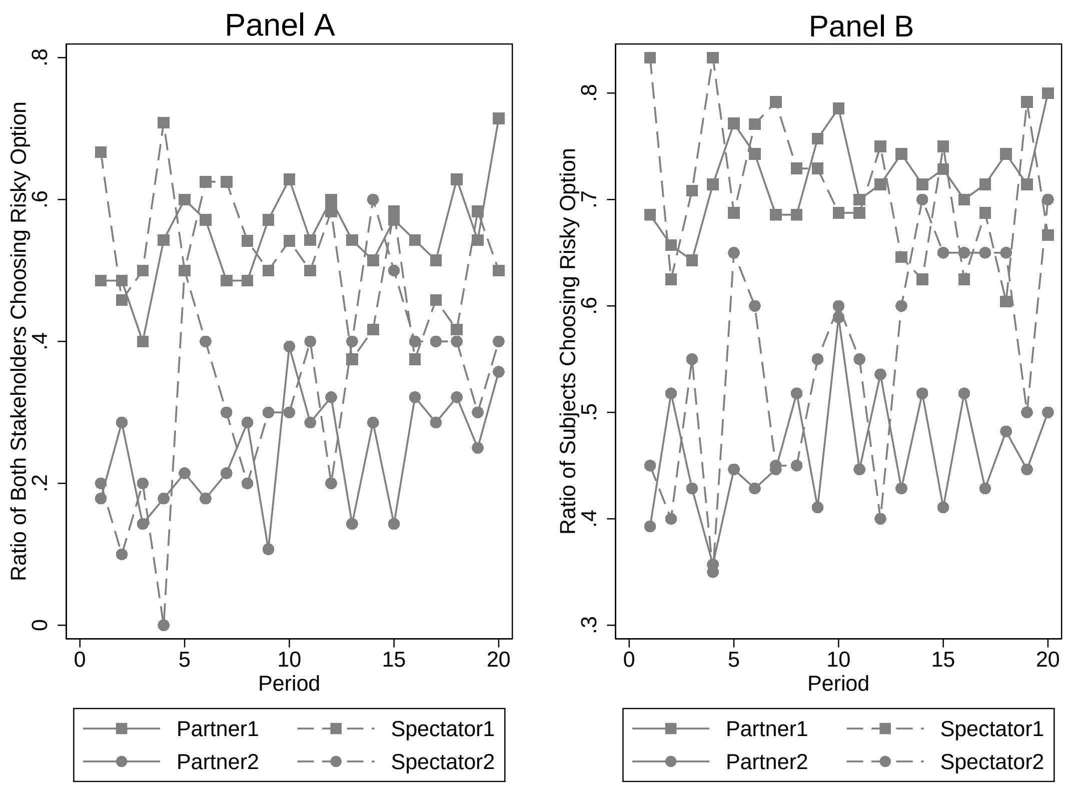

Figure 3.1: Trend of Choice Decisions

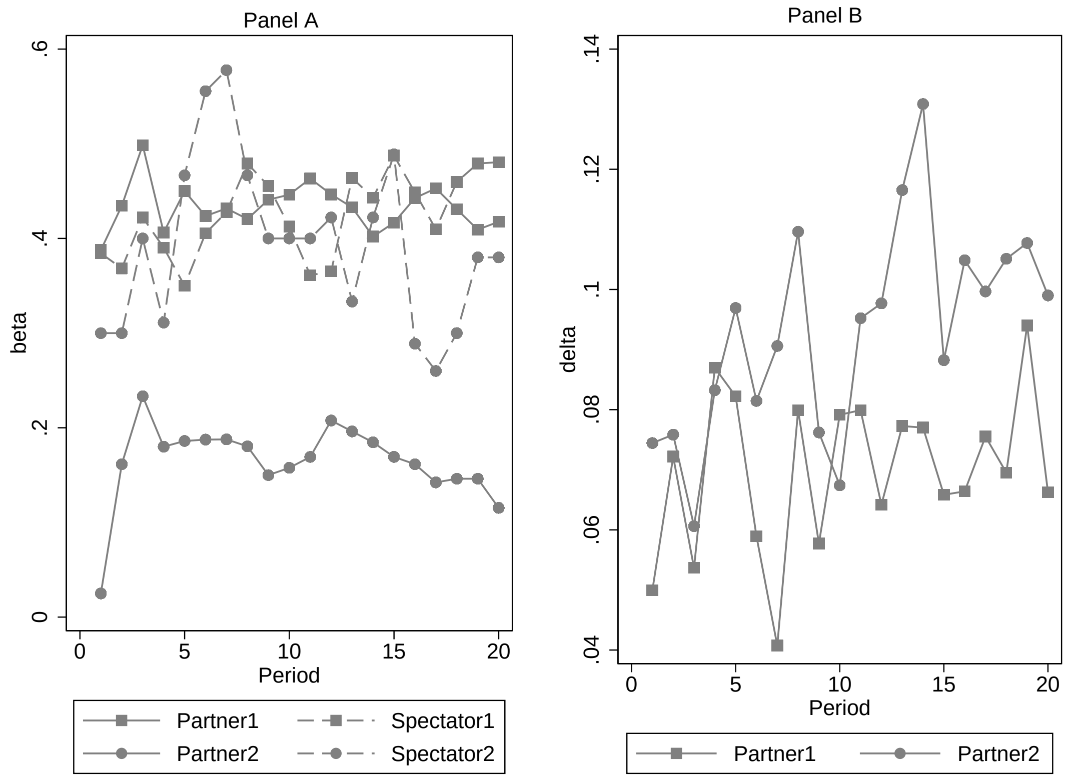

Figure 3.2: Inequality Aversion Comparison

two way clustered by subject and period). The reason is that reciprocity is less attractive for subjects in this treatment. The payment of the safe option is higher, thus subjects are less willing to rely on reciprocity to ensure their payoffs. This finding is also supported by comparing δ. Panel B displays δ in equation (3.1): $\delta_{partnerone} = 0.07$ and $\delta_{partnertwo} = 0.09$ ($p < 0.01$, two way clustered by subject and period).

3.4 Panel VAR Analysis

3.4.1 Estimation Strategy: Panel VAR Model

The model in the previous section provides the key measurements that were used for the econometric analysis. In order to establish the co-evolutionary relationship between subjects' risk preferences and fairness preferences, we employ the panel vector autoregression (PVAR) approach (Holtz-Eakin, Newey, and Rosen, 1988), which extends the traditional vector autoregression (VAR) proposed by (Sims, 1980) with a panel structure over time and cross-sectional units, thereby yielding better estimates. A VAR model is ideal for estimating contemporaneous relationships among a set of possibly endogenous variables. In a VAR, all the variables in the system are endogenous and interdependent, without worrying about causality direction. Each variable is determined by its own lags, and lagged values of the other variables in the system. As a combination of the time-series VAR approach and panel data estimation procedures, the panel VAR provides a powerful tool to analyze the dynamic relationships among variables in the system in our experiment.

The primary econometric model takes the following unrestricted reduced form

$$X_{it} = \Gamma_1 X_{i,t-1} + \Gamma_2 X_{i,t-2} + ... + \Gamma_p X_{i,t-p} + \mu_i + e_{it} \tag{3.26}$$

where X_{it} is a vector of variables, including $Choice_1$, $Choice_2$, $Income_1$, $Income_2$, β in the Spectator treatment and $Choice_1$, $Choice_2$, $Income_1$, $Income_2$, δ_1, δ_2 in the Partner treatment. Choice is a binary variable, which equals 1 if R is chose, or 0 if S is chosen. Γ_1, Γ_2 ... Γ_p indicate the matrix coefficients on the lagged variables. The optimal lag length p is determined by the Akaike information criterion (AIC) (Akaike, 1969), Schwarz's information criterion (SBC) (Schwarz, 1978) or H-Q information criterion (HQIC) (Hannan and Quinn, 1979). We adopt the selection criterion proposed by Andrews and Lu (2001) that resembles all of them. A first-order panel VAR is chosen for both the Spectator and Partner treatment. μ_i is a vector of unobserved fixed effects, representing individual-specific characteristics in our model; e_{it} is a vector of iid idiosyncratic errors.

The variables in the panel VAR model ought to be stationary. So we assess the assumption of stationarity by several different unit root tests generally used for panel data. Based on the Fisher principle, Maddala and Wu (1999)'s test assumes heterogeneity in the autoregressive coefficient of the Dickey-Fuller regression but ignores cross-sectional interdependence across different panel units. The assumption of independence across panel units is unrealistic in many cases. However, it may fit our study since the panel units (groups) made independent decisions in our experiment. We also check stationarity using Breitung (2000) and Choi (2001). Pesaran (2007) proposed the cross-sectional augmented Dickey-Fuller (CADF) test for unit roots that allows cross-sectional dependence across different panel units. The CADF test combined the classical augmented Dickey-Fuller with the approximately lagged cross-sectional mean and its first difference in order to capture the cross-sectional dependence. The results from the panel unit tests are reported in Table 3.1. Due to limited space, we only report results from panel unit root tests proposed by Maddala and Wu (1999) and Breitung (2000).

The standard first-differencing procedure may result in biased coefficients, since fixed effects are possibly correlated with the explanatory variables in the panel model . General method of moments (GMM) estimates can be obtained by using the original variables as instruments for Helmert-transformed variables (Arellano and

Table 3.1: Panel Unit Root Test Results

Variables	Method	
	Maddala and Wu (1999)	Breitung (2000)
$\beta_{partner}$	0.314	0.015
$\beta_{leviathan}$	0.000	0.000
δ	0.000	0.000
Choice (Partner)	0.000	0.500
Choice (Spectator)	0.000	0.000
Income(Partner)	0.000	0.000
Income(Spectator)	0.000	0.000

Bover, 1995; Blundell and Bond, 1998). In particular,

$$X_{it}^{*} = \delta_t[X_{it} - \frac{1}{T-t}(X_{i,t+1} + ... + X_{iT})], t = 1, , T-1 \tag{3.27}$$

and

$$e_{it}^{*} = \delta_t[e_{it} - \frac{1}{T-t}(e_{i,t+1} + ... + e_{iT})], t = 1, , T-1 \tag{3.28}$$

where $\delta_t = \sqrt{(T-t))/(T-t+1)}$. That is, variables in each of the first (T-1) periods are transformed into deviations from their forward means. The weighting δ_t guarantees equalized variance and preserves orthogonality in the transformed model. The final panel VAR model is then:

$$X_{it}^{*} = \Gamma_1 X_{i,t-1}^{*} + \Gamma_2 X_{i,t-2}^{*} + ... + \Gamma_p X_{i,t-p}^{*} + e_{it}^{*} \tag{3.29}$$

By this strategy, group fixed effects are removed. And time fixed effects are removed by ‘Time-Demeaning’ all variables in the system.

3.4.2 DAG Analysis: Causality Direction and Panel VAR Variables Ordering

Our objective is to investigate the dynamic interactions among variables, i.e., how one variable of interest reacts to a one-time shock of another variable, while holding all other shocks constant. The approach we applied to orthogonalize shocks is the Cholesky Decomposition, which places some restrictions on the variables ordering. It requires variables that come earlier in the ordering to be weakly exogenous with respect to the variables that appear later. For instance, if variable A is listed earlier than variable B, then A would affect B contemporaneously, but not vice versa. However, variables' lagged impacts are not restricted by the Cholesky Decomposition.

In order to establish causality direction and panel VAR variables order, we employ a directed acyclic graphs (DAG) approach from machine learning (Pearl, 2009). A DAG is a directed graph with no directed cycles. That is, it is formed by a set of vertices connected by directed edges, such that it is impossible to start from some vertex and eventually loop back again by a sequence of edges. It reveals qualitative causal directions through the directed graphs analysis of the covariance matrix of e^*_{it}. DAGs could be interpreted as nonparametric structural equation models (NPSEM)(Robins and Richardson, 2011).

In a DAG, directed arrows are used to represent contemporaneous causal flows. If variables are not connected by arrows, then it implies no direct contemporaneous causal effect. The LiNGAM (Linear, Non-Gaussian, Acyclic Causal Models) algorithm developed by Shimizu et al. (2006) is applied to obtain DAGs.

Figure 3.3 displays the causality directions using the software TETRAD (Tetrad, 2006). In the Spectator treatment (panel A), the spectator's inequality aversion (β) affected stakeholders' choices over safe or risky option ($Choice_1$,$Choice_2$), which then determined their incomes. In the Partner treatment (panel B), a stakeholder's

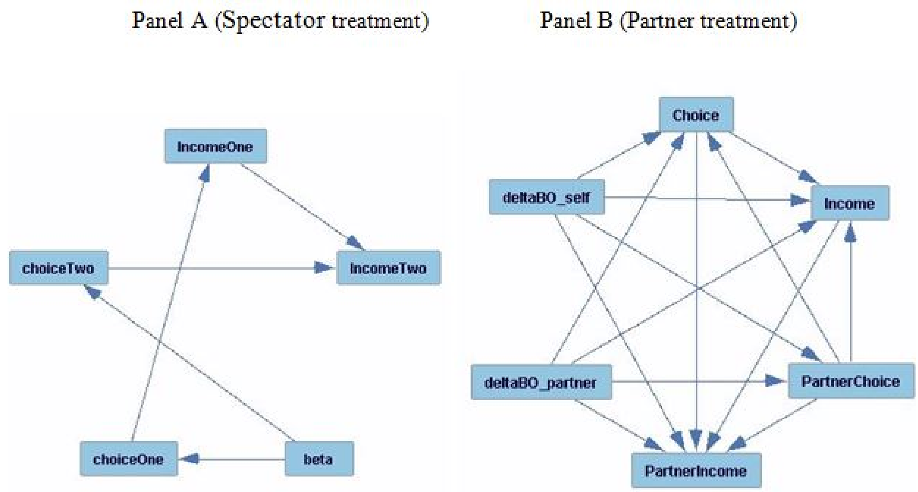

Figure 3.3: DAG in the Spectator Treatment and the Partner Treatment

δ affected his/her choice, and determined earnings along with his/her own choice and his/her counterpart's δ, choice and income. Hence the order of variables is (β,$Choice_1$,$Choice_2$,$Income_1$,$Income_2$) in the Spectator treatment, and (δ_1,δ_2,$Choice_1$, $Choice_2$,$Income_1$,$Income_2$) in the Partner treatment.

3.4.3 Impulse Response

The scale and direction of dynamic relationships among the variables are described by the impulse response functions (IRFs). Figure 3.4 shows the dynamic response of choices made in the first phase of each period to one standard deviation shocks of other variables in the *Spectator treatment*. Panel A shows the persistent effect of risk-taking decisions. Panel B delivers the interplay between two stakeholders' choices over the safe or risky option. We find a weak and transient trend that stakeholders tried to avoid making the same choice. The interplay between stakeholders' choice and the spectators' fairness preferences suggests that more redistribution leads to higher likelihood of stakeholders choosing the safe option. Further, higher income from previous periods leads stakeholders to be more willing to take risk in

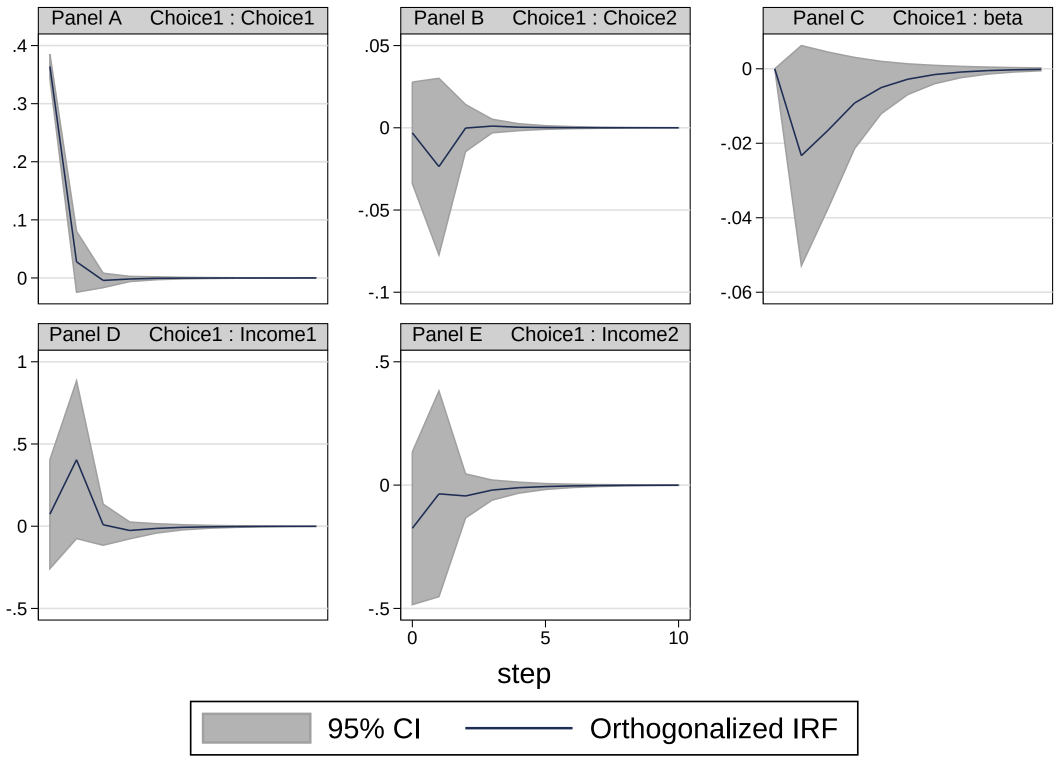

Figure 3.4: Impulse Response of Choices in the Spectator Treatment

later periods, while the counterparts' earnings do not have a significant effect.

The dynamic responses of spectators' inequality aversion to stakeholders' choices and periodly earnings are depicted in Figure 3.5. As shown in the Panel A, the spectators' inequality aversion will persist for several periods. Further, the spectators' fairness preferences are not affected by shocks of the choices made by the two stakeholders. A positive shock of a stakeholder' income escalates the redistributive behaviors of spectators.

Figure 3.6 and 3.7 summarize the dynamic responses of risk-taking behaviors and inequality aversion to the shocks of other variables in the *Partner treatment*. In Figure 3.6, the stakeholders have a transient tendency to make the same choice

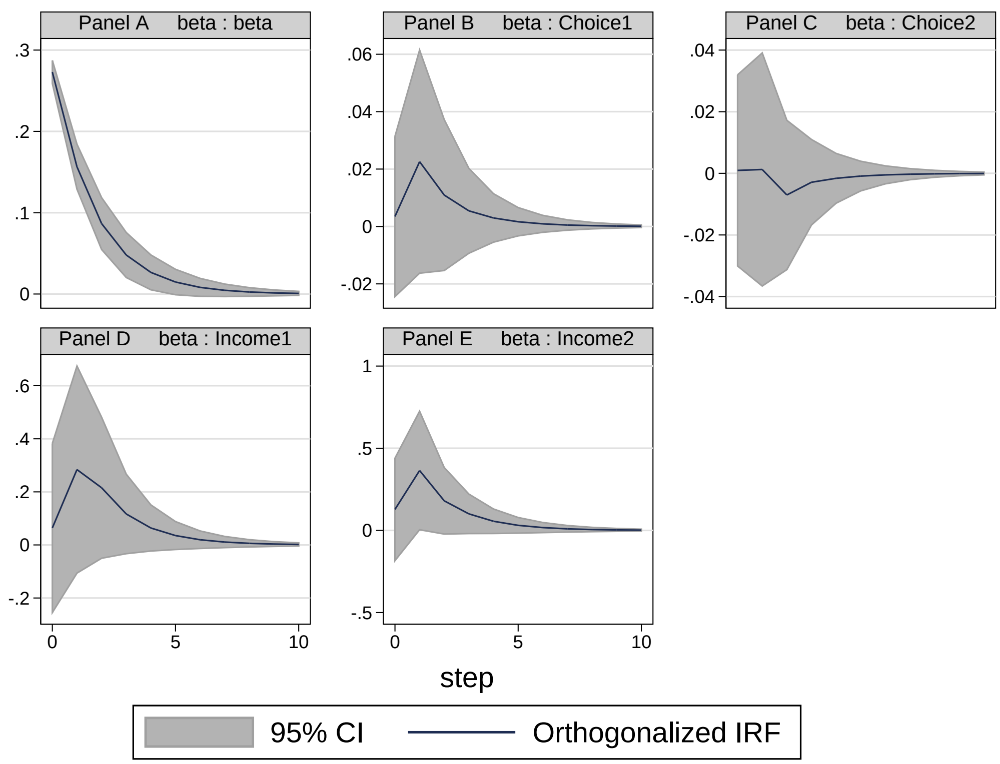

Figure 3.5: Impulse Response of Inequality Aversion in the Spectator Treatment

as in the previous period, and they tend to imitate their counterpart' choice in the previous period. An income shock has a vague effect on choice making. As shown in Panel C and F of Figure 3.6, the shock of a stakeholder's own δ is dissolved by other factors so that it does not significantly affect one's own choice making. In contrast, if the counterpart stakeholder is more generous, i.e., smaller δ, this stakeholder will be more willing to choose the safe option.

Figure 3.7 displays the dynamic response of δ to the shocks of other variables in the system. A higher likelihood of a counterpart choosing the risky option increased δ in the long term. That is, stakeholders are reluctant to be generous to a counterpart willing to take risk. Stakeholders also adjust their inequality aversion according to his/her own income shocks. Higher income in the previous period leads to more generosity. Panel C and F in the right reflect the persistence of δ and the reciprocity between two stakeholders in the same group. The reciprocity can last for a long time.

3.4.4 What Accounts for the Variance of Fairness Preferences and Choices under Risk? A Cholesky Forecast-Error Variance Decomposition

While the above IRF graphs show the detailed scale and direction of the dynamic interactions among fairness preferences and choices under risk. Forecast-error variance decomposition (FEVD) precisely measures the importance of shocks in one variable in explaining fluctuations of other variables, by verifying how much of a k-step ahead forecast-error variance for each variable can be explained by fluctuations of all explanatory variables. We decompose the relative importance of each variable in the Spectator- and Partner- treatment in Table 3.2 and Table 3.3 respectively.

Choices in the Spectator treatment clearly show an extremely strong auto regressive pattern according to Table 3.2. A stakeholder's choice was dominantly determined by itself (99.99% in the one forecast period, 97.17% in the long run).

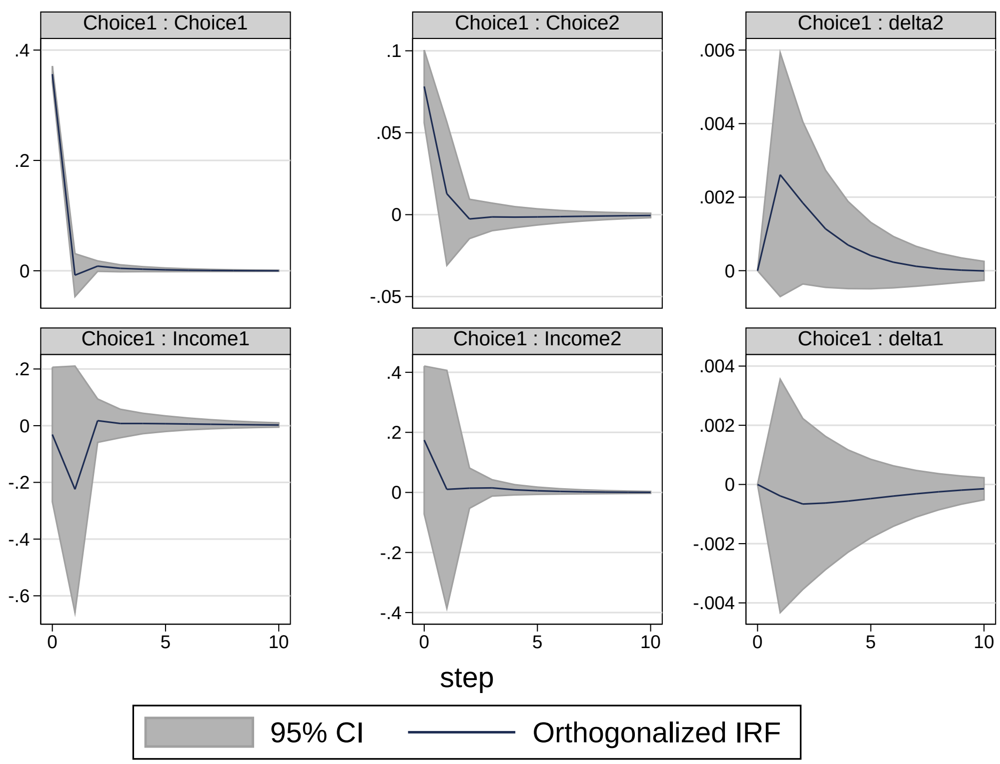

Figure 3.6: Impulse Response of Choices in the Partner Treatment

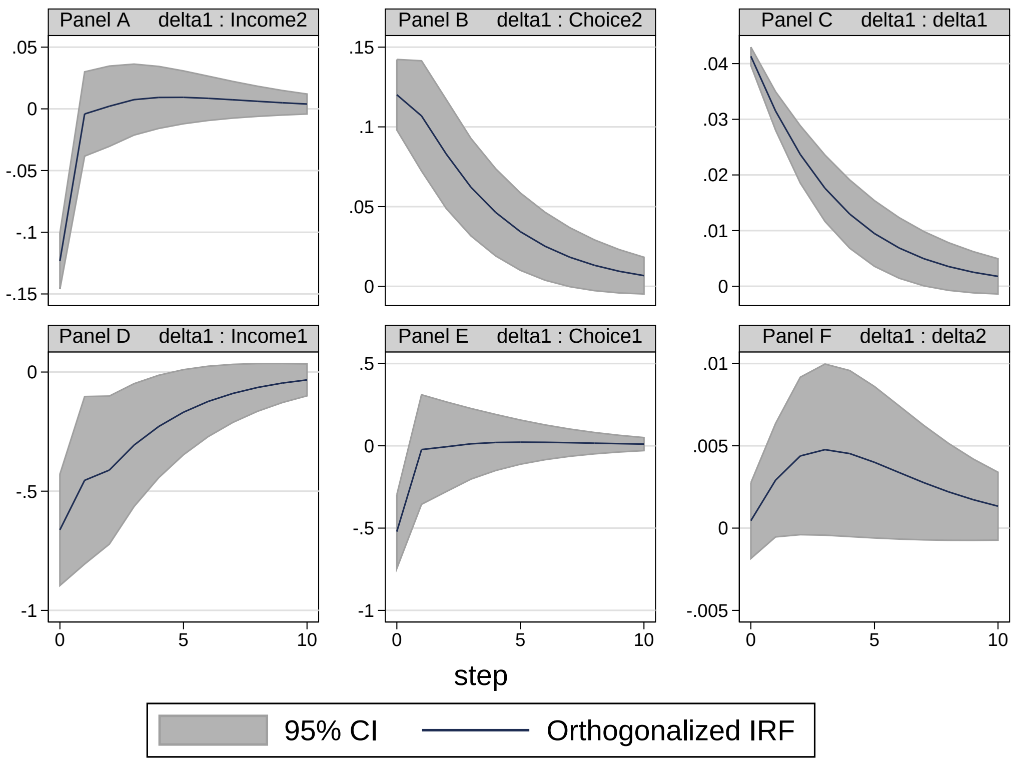

Figure 3.7: Impulse Response of Inequality Aversion in the Partner Treatment

Table 3.2: FEVD in the Spectator Treatment

Dep. var.	Forecast periods	Explanatory variables				
		β	$Choice_1$	$Choice_2$	$Income_1$	$Income_2$
$Choice_1$	1	0.01%	99.99%	0	0	0
	5	0.50%	97.17%	0.10%	2.20%	0.02%
	10	0.50%	97.17%	0.10%	2.20%	0.02%
β	1	100%	0	0	0	0
	5	98.69%	0.83%	0.05%	0.37%	0.08%
	10	98.66%	0.84%	0.05%	0.37%	0.08%

A 2.20% of variations in choice making can be explained by his/her own previous earnings. Higher redistribution leads to higher likelihood of stakeholders choosing the safe option, but its importance is very weak. Spectators' fairness preferences also show dominance in auto correlation. They also take the two stakeholders' incomes and choices into account, although the importance is not significant.

Choices in the Partner treatment also appear to have an auto regressive pattern as shown in Table 3.3. But the self-dominance pattern is much weaker than in the Spectator treatment. About three quarters of variations in choice making can be explained by itself. The stakeholders take their own inequality tolerance into account, while they make choices based on their estimates of counterparts' inequality aversion. From 20% (one forecast period) to 27% (long term) of the variations in stakeholders' choices can be explained by the variations in counterparts' fairness preferences. In contrast, the importance of their earnings is less.

The stakeholders tend to have a persistence for inequality tolerance. Nearly 97% of variations in the inequality aversion measure can be explained by itself. The rest of variations are mostly explained by reciprocity. Their earnings can also affect their inequality aversion, although the impact is not very important.

Table 3.3: FEVD in the Partner Treatment

Dep. var.	Periods	Explanatory variables					
		δ_1	δ_2	$Choice_1$	$Choice_2$	$Income_1$	$Income_2$
$Choice_1$	1	9.51%	11.11%	79.39%	0	0	0
	5	8.76%	18.22%	72.42%	0.09%	0.38%	0.13%
	10	8.88%	18.37%	72.15%	0.10%	0.38%	0.13%
δ_1	1	100%	0	0	0	0	0
	5	97.79%	1.44%	0.03%	0.03%	0.62%	0.09%
	10	97.05%	2.11%	0.05%	0.04%	0.67%	0.09%

3.5 Concluding Remarks

This paper reported the results of a laboratory experiment investigating how people's fairness preferences and risk preferences evolve. The experimental results are complementary with theoretical and empirical findings in understanding the evolution of income inequality and redistribution preferences. Since lagged effects and endogeneity problems cause difficulty in estimating causal effects in dynamic games even in a laboratory experiment, we employ a panel VAR model that introduces a methodology to analyze long term panel data in experimental economics.

Generally speaking, we find different interplay patterns of stakeholders between in *Partner treatment* and in *Spectator treatment*. In the Partner treatment, stakeholders show stronger interplay between fairness preferences and counterparts' choices. However, in the Spectator treatment, stakeholders do not affect each other's choices, but rely on the spectators' decisions. Spectators adjust their fairness preferences according to income inequality between stakeholders but not their risk choices.

3.6 Appendix: Experimental Instruction

3.6.1 General Instruction

Welcome to this experiment!

Before the session begins, you will carefully read the basic instruction in 20 minutes. Please feel free to ask questions if you are confused. You will have to answer some questions to check that you understand the instructions. During the experiment you are not allowed to communicate with other participants. If you have a question, raise your hand. We will come to answer your questions. Sometimes you may have to wait a short while before the experiment continues. Please be patient. Thanks for your patience and cooperation.

Your earnings in the experiment will be calculated in points. Points will be converted to US dollars at the exchange rate shown on the screen.

Upon the completion of the experiment, you will also receive a participation fee of $10. At the end of the experiment your total earnings will be paid out to you in cash.

Following the main choice tasks, we will ask you some questions about your socio-demographic characteristics. Your responses are helpful in that they can be used to explain some of the decisions you make in the experimental exercises. Please note that your responses will not be linked to your name, nor made available to anyone outside the research team. Your ID number is used to match your responses so that they are not confused with anyone else's, and will be used to determine your earnings from the experiment.

We ask that you not talk with anyone else today except for the designated researchers conducting this experiment.

We expect that the entire session will take about one hour. Your participation is completely voluntary. You may ask questions at any time during the experiment.

3.6.2 Instruction in Partner Treatment

In this experiment, you will be randomly matched with another participant for the rest of the session that will last 20 periods.

Each period includes two stages. In the first stage, you and your counterpart will independently make a choice between receiving 4 points (option A) and a risky alternative (option B) that pays 10 or 0 points with equal probability (50/50).

In the second stage of each period, after observing all choices and realized outcomes for both of you in previous periods, you will decide how many points you will give to your counterpart if your earning in this current period is higher than your counterpart.

The redistribution procedure will be skipped in case of equal earnings.

The exchange rate: 5 points= $1

Figure 3.8 is an example of screenshot that subjects see in the Partner Treatment during the experiment.

3.6.3 Instruction in Spectator Treatment

Welcome to the second experiment!

In this experiment, you will be randomly matched with the other two participants for the rest of the session that will last 20 periods. In each group, you will be randomly assigned to be a spectator or one of two stakeholders, and remained the same role for the duration of the experiment.

Each period includes two stages. In the first stage, two stakeholders will independently make a choice between receiving 4 points (option A) and a risky alternative (option B) that pays 10 or 0 points with equal probability (50/50).

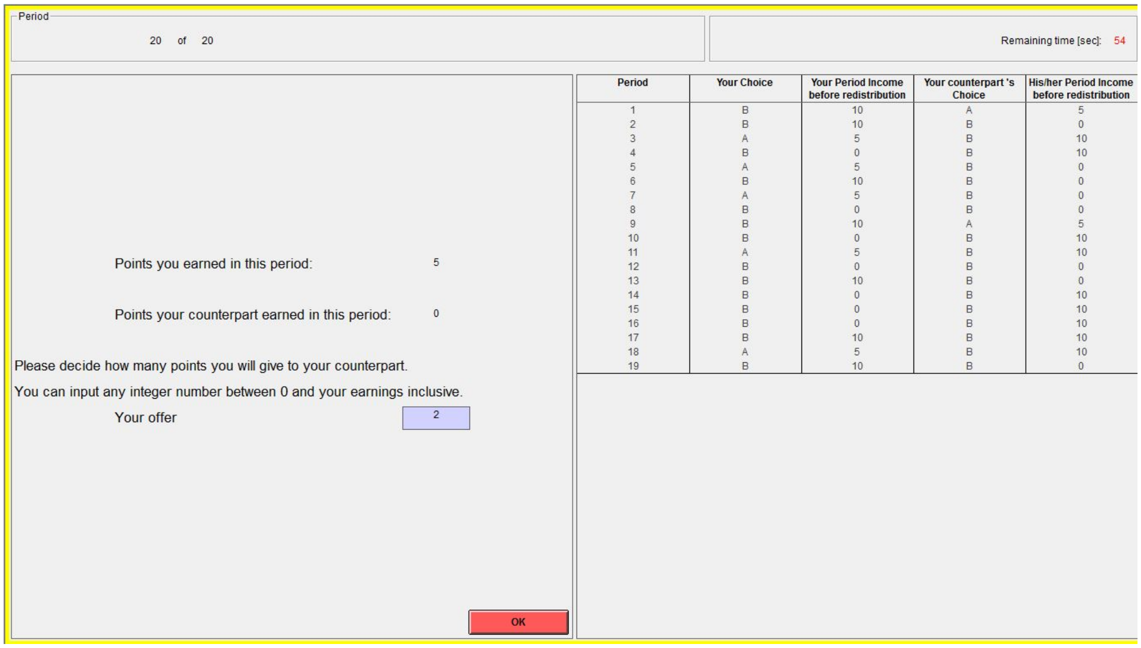

Figure 3.8: Screenshot: Distributive Decision in Partner Treatment

In the second stage of each period, the spectator will observe all choices and realized outcomes for both stakeholders in previous periods. Then the spectator will make redistribution decisions between the two stakeholders.

For example, stakeholder 1 chose to receive 4 points, whereas stakeholder 2 chose risk alternative and received 10 points due to good luck. After observing their choices and outcomes, spectator decided to give stakeholder 1 two points from stakeholder 2's account. Then the two stakeholders' net earnings after redistribution were 6 and 8 points.

The exchange rate: 5 points= $1

The spectators will receive $20 for the whole session regardless of their decisions.

Figure 3.9 is an example of screenshot that subjects see in the Leviathan Treatment during the experiment.

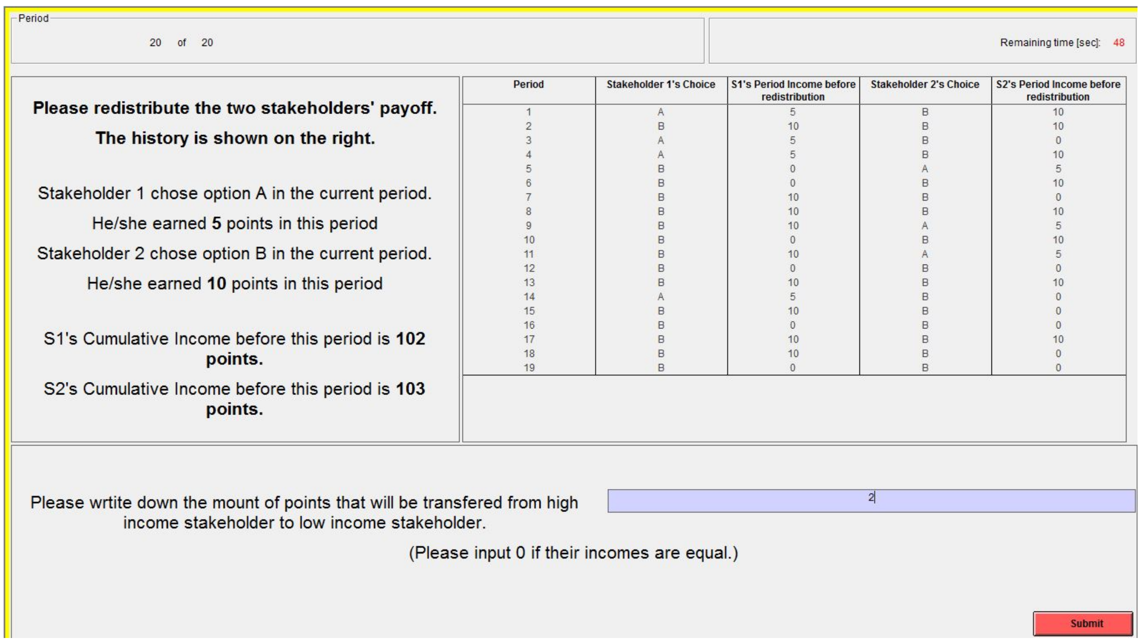

Figure 3.9: Screenshot: Distributive Decision in Leviathan Treatment

4. SHADOW OF A DOUBT: MORAL EXCUSE IN CHARITABLE GIVING

Decisions on charitable giving are often influenced by the performance of charitable organizations and associated organization costs. The veiled cost may come from corruption or legitimate administration and advertisement costs. Therefore, under the shadow, donors are confronted with a tradeoff between helping people in need and the possibility of being cheated. Individuals may justify not giving while being able to excuse their selfishness with a self-serving biased belief that the fundraisers are corrupt. In a laboratory experiment, we find evidence that participants are more likely to exploit the shadow of fundraising cost to bias their belief and contribute less when the incentive for selfishness is greater. Moreover, the charitable contribution significantly increases when the moral excuse is removed by excluding the manipulation of costs by the fundraisers.

4.1 Introduction

Charity plays an important role around the world from ancient to modern society. The premodern societies have practiced a variety of charitable organizations, such as Islamic Waqf, medieval Christian church, Israel Kibbutzim, and hospitals in Greece and Roman Empire. Today, the volume of charity continues to increase. In the United States, total charitable giving was \$358.4 billion in 2014, accounting for 2.1% of gross domestic product. The largest proporton of charitable giving came from private donation totally \$258.5 billion, or 72% of total giving. About 98% of high net worth households[1] give to charity. In May 2015, there were over 1.5 million charitable

[1] These individuals typically are defined as having investable finance (financial assets, excluding primary residence) in excess of \$1 million.

organizations in the United States.[2] A great share of charities have contributed to health and education in the United States.

Despite the importance of charity, efficient and fair provision has been challenging. Since charitable donation often takes the form of public goods, charitable institutions and governments have been seeking effective ways to overcome the free riding problem. For example, governments make charity related policies, such as the tax treatment of individual and corporate donations. Making efficient regulation and policy requires a deep understanding of the intrinsic and extrinsic motives behind charitable donation, most of which are subject to debate. Charitable giving is driven by a complex mixture of pure altruism and self-interest.[3] A well-established literature suggests that motives beyond pure altruism should be included to explain prosocial behaviors such as charitable giving, e.g., tax avoidance[4] , warm glow (Andreoni, 1989, 1990), inequality aversion (Bolton and Ockenfels, 2000; Fehr and Schmidt, 1999), and procedure and intention (Charness and Rabin, 2002; Cox, 2004; Engelmann and Strobel, 2004). Therefore, understanding the motives driving donation is not only important for economic theory, but also for improving the efficiency and fairness of charitable foundations.

This paper studies the motive for charitable giving by investigating the role of charitable organization costs under the shadow. Although legal regulations prohibit charitable organizations from engaging in a number of profitable activities[5], in real-

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[2]Giving USA 2014.
Access at http://www.nptrust.org/philanthropic-resources/charitable-giving-statistics/

[3]See overviews in Andreoni and Payne (2013) and Vesterlund (2006).

[4]Many governments allow tax deductions for the contributions to charitable organizations. In the United States, the Revenue Act of 1917 imposed that charitable contribution is tax deductible. The Obama budget proposal deductions face a lower marginal tax rate. According to the calculation by the Center on Philanthropy (2009), this resulted in a $1.63 billion reduction in charitable giving for the first year.

[5]These activities include but are not limited to: participating in political campaigns at the local, state, and federal levels; substantial lobbying; benefiting a private shareholder or individual with its earnings; pursuing private interests; participating in activities unrelated to tax exempt purposes;

ity, charity foundations charge the 'commission fee' with a wide variety and range of costs. The commission fee may reflect administrative costs, advertisement expenditures[6], as well as possible corruption. And also, the requests for charitable donations may sometimes come from dishonest individual recipients or cheating charitable organizations. Sometimes, it is possible to track how donations are spent. However, due to economic and institutional reasons, in many cases detailed spending information remains unclear to the public. As a result, donors are confronted with a tradeoff between contributing to charities for helping others and in some cases being cheated. Consequently, it raises an important concern when people make decision on charitable donation, especially when contributing to charitable organizations with a large amount of staff and targeted recipients. However, anecdotal evidence suggests that concerns about being cheated may not be the whole cause that discourages charitable giving. People may instead take advantage of the ambiguity of the use of commission fees (e.g., necessary administrative cost versus corruption) as a moral excuse to be less generous to the charity while sustaining a positive self-image. Information about charitable institutions' performance and costs are not always easily accessible, especially under unbinding institutions. If donors are unwilling to make an effort to obtain information about charitable organizations, then they might donate less than they would if they were more informed, or may not give at all.

Consider, for example, the scandal of the Red Cross China in 2011. A Chinese girl, Guo Meimei forged her identity by proclaiming to be a fundraising manager of the Red Cross China. Her display of a lavish lifestyle raised skepticism from the public towards the Red Cross China. Guo Meimei's notoriety ruined the reputation of the Red Cross in China, and people questioned if Red Cross China misused charitable

and acting or intending to act illegally (List, 2011).

[6] Charity organizations spend an average of nearly $100,000 per year on fundraising, with only 12% going to donations (Andreoni and Payne, 2011).

donations. Although this scandal is one of the main causes of the sharp decline in charitable donations in China, the reduction in charitable giving may not be purely driven by the skepticism against Red Cross China. Even though Red Cross China denied any relationship with Guo Meimei shortly after the incident, the public still doubted and donations sharply declined.

Although the anecdotal evidence seems widespread, few quantitative studies have been conducted to examine the potential effects of charitable reputation and self-serving bias on donations. In a laboratory experiment exploring the interplay between charitable organization and donors, we provide a direct test of the hypothesis that the ambiguity of charitable organization's performance can be used as a self-biased moral excuse to contribute less. In the baseline game, participants were randomly chosen to play the role of Participant 1 (henceforth the 'donor'), or the role of Participant 2 (henceforth the 'charity manager').[7] After being randomly paired, the donor received 20 dollars and was asked to distribute it between him/herself and the paired charity manager. At the same time, the charity manager was asked to determine the rate of commission fee, either 20% or 50%, at which each dollar he/she would charge on the donation from the paired donor and leave the rest to the Texas Disabled Veterans Association.[8] To elicit the self-serving bias, we also asked the donors to guess the number of charity managers in the room who chose 50% as the rate of commission fee. The task was incentivized and worth a $20 bonus if answered correctly.

[7]During the experiment, we avoid the use of words such as 'donor' and 'manager'. We use 'Participant 1' and 'Participant 2' instead.

[8]The Texas Disabled Veteran Association is a well-known charitable organization that provides charitable and volunteer outreach and service to veterans seeking the benefits acquired through military service, provided by the Department of Veterans Affairs and other local government and community agencies. Their aim is to maximize the quality of life and opportunities for retired, active duty, wounded and disabled veterans and their immediate family members as well as the disabled community.

To test our major hypothesis, we extend the baseline game in two ways. First, in a high private return (HPR) game, we manipulate the incentive for being selfish and the self-serving bias by a higher return rate of private investment. While the other settings are unchanged, every dollar a donor kept in his own account increased his earnings by $1.5 in the HPR game. Therefore, compared to the baseline game, the HPR game answers the following questions: if people have a higher level of contradictory interest between self-interest and altruism, are they more likely to use the ambiguity of the commission fee to excuse their selfishness? Or equivalently, are donors more likely to guess a higher number of charity managers in the room who chose the higher commission fee (50%) while being less generous to the charity?

Second, in an exogenous commission fee (ECF) game, the commission fee rate is randomly determined, rather than by the managers. That is, the exogenously determined commission fee rate excludes the responsibility of the managers, and prohibits the possibility for donors to maintain a self-serving belief that the charity managers would choose the higher commission fee. If our hypothesis holds, then donors would contribute more in the ECF game than in the baseline game.

The experimental results support our hypothesis. First, donors appear more selfish in the HPR game than in the baseline game. Second, they report a significantly higher proportion of managers who would choose the higher 50% commission fee, implying a greater self-serving bias while the incentive for selfishness is larger. Third, when the possibility for charity managers to manipulate the commission fee is eliminated in ECF game, donors are less selfish. Our estimates are robust to the inclusion of controls for subjects' gender, race, religious, political and ideological preferences.

The remainder of the paper proceeds as follows. We discuss the related literature in Section 2. Section 3 describes the design and procedures of the experiment. Section 4 presents the results. Section 5 concludes and discusses implications.

4.2 Related Literature

Our research is related to several strands of literature. First, there is a fair amount of evidence for the impact of social image on giving (Garicano, Palacios-Huerta, and Prendergast, 2005; Gerber, Green, and Larimer, 2008; Mas and Moretti, 2009; Meer, 2011; DellaVigna, List, and Malmendier, 2012). For example, in a natural field experiment with door-to-door fundraising, DellaVigna, List, and Malmendier (2012) find that many individuals do not want to give but dislike saying no. If they received a flyer with the exact time of solicitation, many of them will not open the door, in order to avoid direct interaction with the funder-raisers. A closely related literature underscores the role of social image. Donors take into account not only the outcomes but also the social image and the revealed intentions behind the outcomes. Givers in the dictator game show their generosity in order to signal the social image of their fairness to the audience (Andreoni and Bernheim, 2009). Dana, Cain, and Dawes (2006) allow the dictators to choose if they play a \$10 dictator game or 'opt-out' the game with \$9 leaving the recipient unaware of the existence of the allocation possibility. The opt-out option substantially reduces generosity, since it avoids the revelation of selfishness. Shang and Croson (2009) provide suggestive evidence in a field experiment that publically announcing donations of others significantly increased contribution to public goods. Lazear, Malmendier, and Weber (2012) also found similar evidence in laboratory experimental games. Taken together, the literature suggests that giving does not only reflect a pure concern for the welfare of others, but also a desire to avoid psychological costs of hurting their social image built on the recipient's expectation. Our study builds on the social image literature but emphasizes more on self-serving bias. People believe what they want to believe that is consistent with their own interest.

Our paper is also related to a vast literature on how information about the recipients affect charitable giving. Eckel and Grossman (1996) show that subjects in laboratory dictator games contribute more to the American Red Cross than an anonymous recipient. Andreoni and Rao (2011) show that two-way communication can substantially increase transfers in a dictator game. Fong and Oberholzer-Gee (2011) investigate the impact of endogenous information in a dictator game setting with different types of poor recipients.[9] While the dictators can purchase the signals about the source of poverty, one third of dictators pay to learn about their recipients, and mostly exploit the information to reduce their giving. Excessive appeals may reduce charitable giving because it is considered as a signal of adverse selection (Van Diepen, Donkers, and Franses, 2009). In an empirical study, Diamond and Noble (2001) provide evidence that donors can develop defense mechanisms by throwing out mail solicitations as a response to frequent request. Conversely, seed money, as a signal of the health of the charitable organizations, can increase charitable giving (List and Lucking Reiley, 2002; Eckel and Grossman, 2003; Karlan and List, 2007). However, as shown in our experiment, information may not only affect charitable giving as a signal, but also through building self-serving bias.

A recent experimental study close to ours is Kandul (2016). In a dictator game with *ex-ante* uncertainty about the recipient's endowment, he found that donors made no significant difference in the transfers with or without *ex-post* information revelation. But if the donors were given the opportunity to choose if they would like to receive such information, nearly one-third of them chose to ignore the *ex-post*

[9]There is a substantial literature in experimental economics that discusses the entitlement of earnings. Earnings from work effort are considered as more legitimate than from pure luck (Cherry, Frykblom, and Shogren, 2002; Durante, Putterman, and van der Weele, 2014; Fershtman, Gneezy, and List, 2012; Hoffman et al., 1994; Oxoby and Spraggon, 2008). Following a similar logic, people are more willing to help people suffering from an accident, but often refuse to support others who need help because of lacking self-control (e.g., drug abuse) (Fong, 2001).

information, while others gave significantly more. The *ex-post* ignorance serves to build a self-biased belief. Our experiment differs from Kandul (2016) in an important way, the self-serving bias is built on *ex-ante* and uncertainty is exogenously determined. Di Tella et al. (2015) is another close study, in which allocators avoid altruism by distorting their belief about the selfishness of others. Our experiment eliminates reciprocity in two-party games and focuses on altruism and self-serving bias in a charitable giving environment.

4.3 Experimental Design and Procedures

This experiment includes three games in total. We start the illustration of the experimental design with the baseline game.

4.3.1 Baseline Game

In each session, one half of the participants were randomly chosen to play the role of Participant 1 (the 'donor'), and the other half played the role of Participant 2 (the 'charity manager'). They were anonymously and randomly paired. Participants were not able to find out the identity of the person they were paired with.

In each pair, the donor received $20 and was asked to distribute it between him/herself and the charity manager. At the same time, the charity manager was asked to set the commission fee rate to keep in his/her own account. The commission fee rate was either 20% or 50%. The rest of the transfer from the donor not kept by the charity manager went to the Texas Disabled Veterans Association. The donation transaction was conducted at the end of the experiment and observed by all subjects. For example, consider the scenario in which a donor kept $10 and delivered $10 to the paired charity manager who set the commission fee to be 50%. In this case, the charity manager kept $5 and the charitable organization received $5 from this pair of participants. It is worthy to note that in contrast to many existing experiments with one side uncertainty, the participants in our experiment made decisions simultaneously. Hence, in the baseline game both sides of our experiment faced *ex-ante* uncertainty about the choices made by their counterpart. In other words, although $20 of the total endowment and the possible commission fee rate are common knowledge, the donor did not know the commission fee rate chosen by the manager until the distributive decision had been made, and the manager was unaware of the amount they will receive from the donor when choosing the commission fee. Before revealing the outcomes, the donors were asked to guess the number of managers in the room who chose the higher 50% commission fee. The correct answer was worth a $20 bonus, which is a significant incentive for donors to report their truthful belief.

4.3.2 High Private Return Game

The high private return (HPR) game is almost identical to the baseline game. The variation lies on the return rate of money to the donor's private account. In the HPR game, every dollar a donor keeps in his/her own account increases private earnings by $1.5. The higher return to private investment induces a greater incentive for donors to be less generous based on a self-biased belief that the managers would be more likely to choose the higher commission fee rate. We use the bonus question to elicit donors' belief on the decisions made by the managers.

4.3.3 Exogenous Commission Fee Game

The exogenous commission fee (ECF) game is exactly the same as the baseline game, except that the commission fee rate is randomly determined with equal probability, rather than chosen by the charity managers, which is known to all subjects. In this game, the donors received information about the commission fee rate before making the distributive decision. This setting discharges the responsibility of the charity managers, and prohibits the possibility for the donors to maintain a self-serving belief that the managers will choose the higher commission rate.

At the end of the experiment subjects filled out a questionnaire, including demographic questions such as gender, race, religion and ideology, as well as attitudes about the experimental procedures and payoff.

The experiment was computerized with the software 'z-Tree' (Fischbacher, 2007) and conducted at Texas A&M University. We used a between-subjects design. 204 subjects participated in the experiment, 78 subjects in the baseline game, 76 subjects the HPR game, and 50 subjects in the ECF game. Each subject participated in only one session . Each session lasted about 60 minutes, including sign-up, consent, decision-making, and payment delivery. Before entering the laboratory, participants were told that they would receive a show-up fee of $10 upon completion of the tasks and they would also receive potential of extra payoffs based on their role, luck and decisions. But they were unaware of details of the experiment. To ensure understanding of the experimental procedures by all subjects, a set of test questions need to be correctly answered before the experiment began. The average payment was $25 for the donors and $13.4 for the charity managers respectively. The Texas Disabled Veteran Association received $557 in total from this experiment, or equivalently $5.5 per pair of subjects on average.

4.4 Results

We begin the analysis with a simple comparison of the selfishness of donors between games. We define *Selfishness* as the gap between the amount of money a donor kept and the amount he/she contributed. Furthermore, to measure the self-serving bias of donors, we define *Guess* as the stated proportion of charity managers in the room a donor believed to choose 50% as the rate of commission fee. The main hypothesis of our paper can be tested by comparing the mean values of *Selfishness* and

Guess of donors across the three games (Table 4.1). In the baseline game, *Selfishness* is \$3.23 on average, less than 50% of that of the donors in the HPR game (\$7.96). The difference is considerable and significant on the edge (p=0.09). Donors reveal more generosity in the ECF game with *Selfishness* being only \$0.88, although the large standard deviation lowers the statistical significance. Regarding the self-serving bias measured by *Guess*, compared to the baseline game, the greater incentive for selfishness in the HPR game increases *Guess* from 53% to 69%, with a p-value of 0.03.

Table 4.1: Selfishness and Self-Serving Bias

	Baseline	HPR	ECF	H_0: Baseline=HPR	H_0: Baseline=ECF
Selfishness	3.23	7.96	0.88	p =0.09	p =0.44
	(11.78)	(12.06)	(11.63)		
Guess	0.53	0.69		p=0.03	
	(0.31)	(0.30)			

Notes: Standard deviations are reported in the parentheses. p-values are reported for two-side t-tests. Mann-Whitney tests report similar results.

One potential concern of a simple comparison is that subjects were not randomly treated. Hence in Table 4.2 we present regression-adjusted tests of the mean differences in self-serving bias between the baseline and HPR games. In every column we regress *Guess* on the dummy indicator of HPR game under different specifications. Column 1 does not include any control variables. The treatment effect is estimated to be 16%, which is identical to the result in Table 4.1. In column 2, we control for gender and race differences in the regression. Column 3 further adds other control variables including self-reported recent donation history, ideology, religion, and political affiliation. Compared to column 3, column 4 excludes the recent donation history and religion, both of which have many missing values. All the estimates provide a similar magnitude of self-serving bias.

Table 4.2: Comparing Self-Serving Bias between Games by Regressions

	(1)	(2)	(3)	(4)
	Guess	*Guess*	*Guess*	*Guess*
Game=HPR	0.16**	0.16**	0.19**	0.16**
	(0.07)	(0.08)	(0.09)	(0.08)
Controls	No	Gender & Race	†	§
R^2	0.07	0.22	0.47	0.26
Observations	77	77	63	74

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
†: Controls variables include gender, race, the recent donation history, ideology, religion, and political affiliation. We allow empty entry for most questions but not gender and race. Since donation history and religion are more private, some subjects refused to respond.
§: Controls include the control variables in †but exclude the recent donation history and religion.

In Table 4.3 we further provide regression-adjusted mean differences in *Selfishness* between the baseline and HPR games. The results in Table 4.3 suggest varying quantitative evidence of different selfishness between the two games. Donors are more selfish in the HPR game with greater incentive to be less generous. The statistical significance has a plausible trend toward significance.

It is interesting to explore the extent to which the donors utilize self-serving bias to excuse their selfishness. Hence we investigate the correlation between *Selfishness* and *Guess* by simple regressions in Table 4.4. In column 1 by pooling donors in the baseline and HPR games, the correlation suggests that a ten percentage point increase in *Guess* is associated with about \$0.7 additional *Selfishness*. More interestingly, we find different correlations between selfishness and self-serving bias by separating the two games in columns 2 and 3. In the baseline game with relatively

Table 4.3: Comparing *Selfishness* between Games by Regressions

	(1) *Selfishness*	(2) *Selfishness*	(3) *Selfishness*	(4) *Selfishness*
Game=HPR	4.73*	6.41**	6.28	5.90*
	(2.72)	(2.94)	(3.99)	(3.09)
R^2	0.04	0.18	0.34	0.20
Controls	No	Gender & Race	†	§
Observations	77	77	63	74

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
† and § are the same as before.

low incentives to excuse selfishness by self-serving bias, the correlation is attenuated and insignificant. In contrast, as seen in column 3, donors in the HPR game show a strong tendency to exploit the self-serving bias to justify their lack of generosity. This provides additional evidence in support of our findings.

Finally, we compare the payoff and self-reported satisfaction of donors across games. In our survey, subjects report their satisfaction on a scale from 'very disappointed' to 'very satisfied' (e.g., 1-5 in numerical measure). The null hypothesis is that payoff is positively correlated with the satisfaction of payoff. However, Table 4.5 shows that making higher profit does not satisfy donors, since they adjust their expectation of payoff by their biased beliefs of fairness and charity managers' choices.

Table 4.4: Correlation between Selfishness and Self-Serving Bias

Outcome Variable: *Selfishness*	(1) Baseline & HPR	(2) Baseline	(3) HPR
Guess	7.23***	3.78	9.58***
	(2.05)	(3.13)	(2.70)
R^2	0.14	0.04	0.25
Observations	77	39	38

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4.5: Payout and Satisfaction of Donors across Games

	Baseline	HPR	ECF	Baseline = HPR	Baseline=ECF
Payout	14.18	18.88	10.44	p =0.01	p =0.04
	(7.71)	(8.20)	(5.82)		
Satisfaction	4.15	4.21	4.04	p =0.75	p =0.64
	(0.84)	(0.70)	(1.06)		

4.5 Concluding Remarks

The results of this article speak to charitable fundraisers and related policy makers that seek more efficient financial donations, while also contributing to the behavioral economics literature. The findings of this laboratory experiment add to our understanding of the motives for charitable giving and provide policy relevance to make charitable fundraising more effective. We provide suggestive evidence that donors have strong propensity to excuse their selfishness by self-serving biased belief that fundraisers are corrupt. Participants in our experiment are more likely to exploit the shadow of fundraising costs to contribute less while the incentive for selfishness is higher. Eliminating the moral excuse by excluding the fundraisers' manipulation of commission fee significantly increases charitable giving.

Our experiment also sheds light on the policy implication for fundraising and charitable regulation. Legitimate fundraisers can differentiate themselves by providing transparent information about how donations are spent. Since it is likely that misinformation and bad public image of one charity may have spillover effects to other charitable institutions, the information about the performance of charitable organizations is a public good in nature. Accordingly, we suggest that governments and NGOs should spend resources on detecting and disclosing information about charitable organizations and recipients. Market design to improve the efficiency of charitable markets will provide promising avenues for future research.

A disadvantage of this study is that our result is limited to the laboratory environment. We believe that future research on philanthropic-related issues can benefit from naturally occurring field events. Furthermore, charitable giving is not limited to money; time effort makes up a large part of charitable contribution. In the United States, about 65 million adults volunteered 7.9 billion hours of public service, equivalently a estimated value of $175 billion.[10] Cross-country analyses suggests that well-being is positively correlated with monetary giving, but the relationship between time giving and well-being is weaker and more ambiguous (List and Price, 2011). Hence the heterogeneous effects of monetary contributions and time effort contributions should be explored in the future.

4.6 Appendix: Experimental Instructions

For sake of brevity, in the appendix, we only briefly describe the experimental instructions in the baseline game with screenshots in 'z-Tree'. The instructions in other games are similar.

Upon entering the laboratory, participants received and signed the consent form. All subjects first read a general instruction (see Figure 4.1). Then the subjects were randomly assigned as Participant 1 or Participant 2. Participant 1 read the instructions in Figure 4.2 and 4.3. At the same time, Participant 2 read the instruction as shown in Figure 4.3.

After reading the instruction, they were asked to answer the questions to ensure understanding (see Figure 4.4).

Once they correctly answered all the questions, they make their decisions simultaneously. But the subjects playing the role of Participant 1 had an opportunity to answer a bonus question that was used to elicit their self-serving bias (see Figure

[10] See http://www.nptrust.org/philanthropic-resources/charitable-giving-statistics/.

4.5).

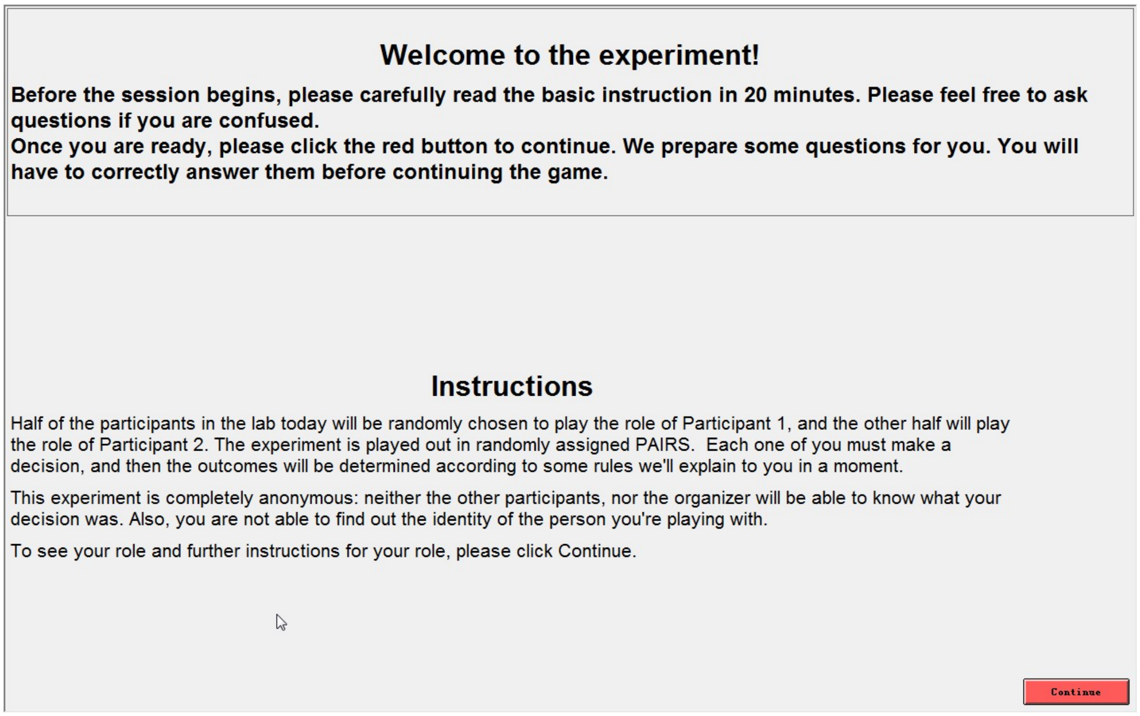

Figure 4.1: General Instruction

You are Participant 1. You received $20.

Another participant in the lab has been randomly assigned as Participant 2 paired with you.

You need to distribute those $20 dollars between you and Participant 2. In other words, you have to decide how many dollars out of the $20 you are going to keep, and how many you'll give to Participant 2. This is your only decision to make in the experiment.

The ONLY decision for Participant 2 to make is to set the rate of commission fee at which each dollar will be charged. The commission fee should be either 20% or 50% of the dollars you deliver to Participant 2. And the rest will go to the Texas Disabled Veterans Association. You can audit the process we donate to the Texas Disabled Veterans Association immediately after the experiment.

Important rules:

Note that you and Participant 2 will make choice simultaneously. In other words, you will not be able to know the commission fee until the distributive decision has been made, and Participant 2 will not know how you distributed the dollars until after having chosen the rate of commission fee.

Before you make your decision, we're going to explain the instructions received by Participant 2. What follows are the instructions that person receives (remember that you are Participant 1, not Participant 2). Please read them carefully, since they are important for you to understand how the experiment works and make decision.

Figure 4.2: Instruction for Participant 1, Part I

These following statements are NOT your instructions.
These are ALL the instructions that Participant 2 sees:

You are Participant 2.

Participant 1 will distribute $20 dollars between you and him/herself. In other words, he/she has to decide how many dollars out of the $20 to keep, and how many give to you.

The ONLY decision for you to make is to set the rate of commission fee at which each dollar will be charged. The commission fee should be either 20% or 50%. In other words, you will choose either keep 20% or 50% from the dollars the Participant 1will give to you. And the rest will go to the Texas Disabled Veterans Association. You can audit the process we donate to the Texas Disabled Veterans Association immediately after the experiment.

In the contract we sign we guarantee you that you REALLY are playing with another person, and accordingly your decisions will be executed. We guarantee the donation will be delivered. We encourage you audit the process we donate to the Texas Disabled Veterans Association with an IRB staff outside of our research team.

Important rules:

Note that you and your paired participant will make choice simultaneously. In other words, you will not be able to know how Participant 1 distributed the dollars until after having chosen the commission fee, and Participant 1will not know the commission fee you choose until the distributive decision has been made.

Go to questions

Figure 4.3: Instruction for Participant 1, Part II

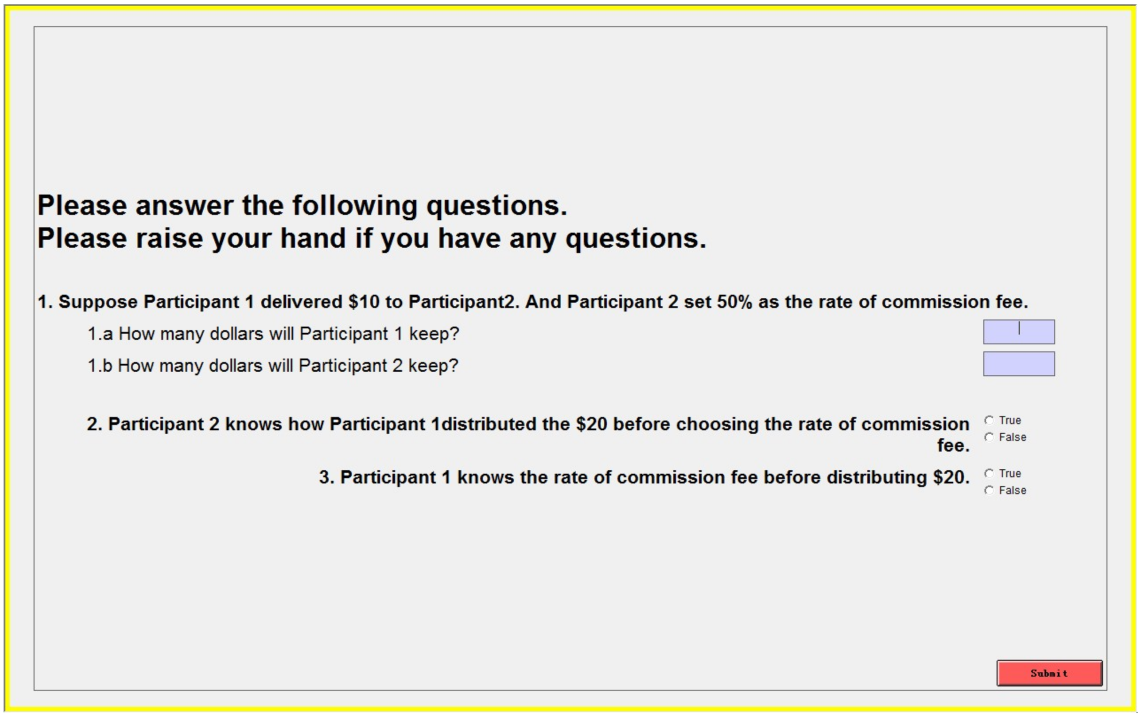

Figure 4.4: Test Questions

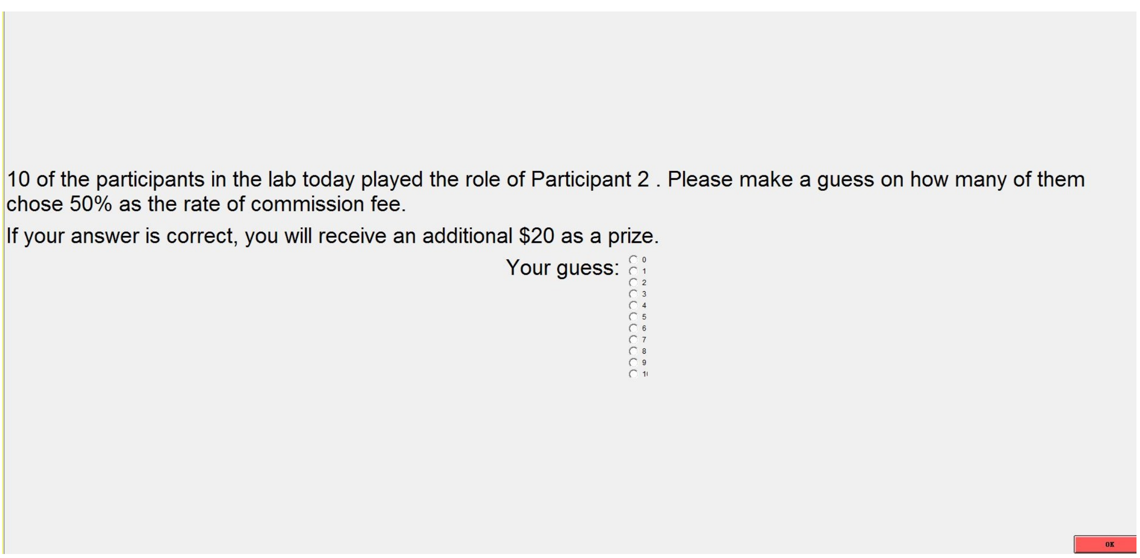

Figure 4.5: Bonus Question for Participant 1

5. CONCLUSION

A growing literature suggests that social preferences should be included in economic models to explain how people behave differently from pure self-interested and rationality. Policy makers and welfare program designers should also take fairness preferences into account while implementing and evaluating the effectiveness of policies.

In the three interrelated essays of this dissertation, experimental method is employed to elicit fairness preferences under risk and uncertainty. The first essay investigates fairness preferences and redistribution under different rooted risks, specifically whether the risk is exogenously assigned in a pure-luck control condition or endogenously chosen by subjects in an option-luck treatment. By comparing the redistributive decisions between the control condition and treatment, we reconcile the competing fairness views by suggesting that people tend to support the accountability principle in the presence of relatively lower income inequality but are more inclined to consequential egalitarianism in case of large income inequality or salience of extreme low payoff. We believe that the results are not only important for interpreting some prosocial behaviors but also shed light on social and economic policies, such as health insurance plans, job training programs, and so on.

In the second essay we explore the dynamic evolution fairness preferences, risk preferences in a laboratory economy in which and income inequality are jointly determined under different redistributive policies. This essay introduces a panel vector auto-regression model to compare different interactive patterns between the two treatments. We find that in the Spectator treatment, stakeholders tend to make decision independently, while they mainly rely on the spectators' decisions. By contrast, in the Partner treatment, stakeholders show a stronger interplay in reciprocity and choice making.

The third essay imitates the environment of charitable contribution, in which donors are confronted with a tradeoff between helping people in need and the possibility of being cheated. Donors excuse their selfishness by sustaining a self-serving biased belief that the fundraisers are corrupt. Donors believe what they want to believe that is consistent with their self-interest. The findings of this experiment deepen our understanding of the motives behind charitable giving and shed light on policy implications to make charitable fundraising more sufficient and effective.

Experimental economics has also some caveats, especially in laboratory experiments. The most important challenge is the concerns about 'external validity'. Field experiments, alike lab experiments, also randomize subjects into treated and control groups and infer causal effects by comparing outcomes between these groups. Field experiments have the advantage that behaviors and outcomes are observed in a naturally occuring environment rather than in a laboratory environment differently from the real world. Accordingly, field experiments are often seen as having higher external validity than laboratory experiments (Harrison and List, 2004; Levitt and List, 2009; List, 2009). Future work can be conducted by extending these laboratory experiment into the field.

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