EXPERIMENTS IN PUBLIC POLICY

A Dissertation

by

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ABSTRACT

This dissertation addresses three policy-relevant issues using experimental methodology. These studies illustrate the value of using experimental methods to study policy questions.

First is the blood donation game, which is related to health policy. In this essay I design an experiment to investigate two incentive treatments intended to increase donations, a waiver of blood transfusion fees and priority access to blood supplies. I find that both the waiver treatment and the priority treatment significantly raise donations, and combining the two incentives has the most impact. Second, I study private provision of public goods and examine how introducing the possibility that group members can punish each other affects provision, varying group sizes. Results indicate that introducing a punishment institution has no effect when group size is small. However, for large groups, introducing the punishment institution dramatically increases provision. Finally, I use a popular contest game, the non-constant-sum Colonel Blotto game, to study economic policy. This game mimics the R&D investment decisions of companies that compete with each other. I investigate two factors that might make it easier for firms to engage in a kind of tacit collusion, collectively lowering their investment in innovation: the stability of the relationship (pairs are either stable over time or re-matched each round), and the number of prizes (which proxy for inventions). I conclude that subjects are more successful in tacitly colluding when groups are stable, regardless of the number of prizes. However, when randomly re-matched every period, subjects

only collude when there are more prizes: that is, having more prizes facilitates tacit collusion. Both players are worse off when the number of prizes is small because they increase their bids to compete more aggressively against each other.

DEDICATION

This dissertation is dedicated to my parents for inspiring and supporting me throughout all of my years in school.

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I would like to express the deepest appreciation to my committee chair and mentor, Dr. Catherine Eckel, for her guidance, support and patience on both my research and life. This work would not have been completed without her help.

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1. INTRODUCTION

The three studies that comprise this dissertation use laboratory experiments to address three different public policy issues in the areas of health, social and economic policy.

The first study, contained in Section 2, focuses on health policy. The need for blood is increasing every year in many countries, according to the World Health Organization (WHO). A number of approaches have been taken to elicit more blood donations. Some institutions waive blood transfusion fees for recipients who have donated in the past. Others give priority access to former donors when the demand for blood exceeds supply. In this study, I design a new lab experiment to investigate the extent to which these two incentive treatments, a waiver of blood transfusion fees and priority access to blood supplies, are effective in increasing donations. I find that donations increase the most when subjects are exposed to the combination of both waiver and priority incentives. Both the waiver treatment and priority treatment individually increase contributions; however, the waiver treatment is more successful at increasing donations than the priority treatment.

Section 3 investigates social policy in the context of the private provision of public goods. The public goods game has been one of the most influential games applied to the study of social welfare. In this game each subject is given an endowment of resources which can be divided between their own earnings and a good that benefits their group: the public good. Introducing a punishment institution – that is, the possibility that group members can punish one another – has been shown to be effective at increasing public good contributions. I examine the effectiveness of punishment for groups of different sizes. One might expect that a punishment institution would have the most impact on small groups, because of a second-order free rider problem: group members might free ride on the punishment effort of others, and this would worsen with group size. However, I find that punishment has no effect on contribution levels when the group size is very small (2). However, for large groups (8 or 24), the provision of the public good is dramatically increased when punishment is introduced, and the cooperation that results can also be sustained.

Section 4 studies economic policy with respect to firm R&D investment. Suppose that firms compete to develop an innovation contest, and the firm that invests the most is more likely to be successful. To mimic this situation, I investigate people's behavior in a contest environment, the Colonel Blotto game. In this game, two players simultaneously allocate their endowments across multiple prizes (or inventions). The goal of each player is to maximize profit by using the minimum amount of resources to win the most prizes. Firms are better off if they can tacitly agree to reduce spending, because even if spending is very low, one firm will still win the contest. I vary two factors that might make it easier for firms to engage in tacit collusion, collectively lowering their investment in innovation: the stability of the relationship (pairs are either stable over time or re-matched each round), and the number of prizes (which proxy for inventions). Results show that subjects always succeed in reducing investment when groups are stable, regardless of the number of prizes. However, when firms are re-matched each period, firms only succeed in reducing spending when there is a large number of prizes. When the number of prizes is small, firms increase their bids to compete more aggressively against each other. Thus, both are worse off.

These three sections address different policy-relevant issues and suggest solutions to practical problems for policy makers to enhance efficiency and economic welfare.

2. HELP FOR THE HELPER: CAN BLOOD DONOR INSURANCE PLANS INSPIRE GREATER GIVING

2.1 Introduction

The need for blood and blood products is increasing every year all over the world. The critical shortage of blood is one of the biggest social issues worldwide. Blood banks in the U.S. face seasonal blood shortages. Especially in 2015, both the Red Cross and America's Blood Centers, which together represent virtually all U.S. blood banks, reported severe shortages during early fall when blood supplies are usually adequate.[1] Compared to the U.S., the availability of blood in developing countries is even scarcer. These blood shortage situations can be improved only if we maintain adequate supplies of blood.

The American Red Cross reports that an estimated 38% of the U.S. population is eligible to donate blood, but less than 10% actually do each year.[2] According to WHO Global Database on Blood Safety for the year 2012, the median blood donation rate is 36.8 donations per 1000 individuals in high-income countries, 11.7 donations in middle-income countries and just 3.9 donations in low-income countries.[3] Thus, the top priority of blood banks is to encourage individuals to donate blood in an effective way. WHO has adopted a resolution encouraging countries to promote the development of national

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[1] Abc News on September 19, 2015 (http://abcnews.go.com/Health/story?id=117954)

[2] Blood Facts and Statistics from American Red Cross (http://www.redcrossblood.org/learn-about-blood/blood-facts-and-statistics)

[3] Blood Safety and Availability from WHO (http://www.who.int/mediacentre/factsheets/fs279/en/)

blood services based on voluntary donations since 1975. However, purely altruistic incentives are not always enough to collect sufficient donations to fulfill the blood demand. There are still a few countries, like Russia, that have to pay blood donors to maintain an adequate supply. Although blood donation remains primarily voluntary in the developed world, different incentives have been offered to raise donations.

In the U.S., the Food and Drug Administration (FDA) requires that rewards for unpaid blood donors not be easily convertible to cash; however, the line between paying cash and offering monetary incentives is often a fine one. Most American blood banks recruit donors by using different forms of rewards including t-shirts, umbrellas, event tickets, or drawings for expensive prizes such as cars or home appliances. European countries, such as Italy and Germany, also offer similar incentives for blood donors. These incentives range from one or more days off work and tax reliefs to other material rewards.

Quite a few blood banks in the U.S. have recently started to offer insurance-like donor benefits to their donors. Blood centers, such as Our Lady of the Lake[4] and the Mayo Clinic[5], offer donors different insurance plans. Blood donors are rewarded with a supplement to their existing insurance that helps cover the charges for any blood they

[4] Our Lady of the Lake offers blood donors assurance plans. Through these plans, blood donors are rewarded by providing a supplement to existing insurance that helps cover the charges for blood use. The plans include unlimited replacement coverage for specific individuals on a yearly time frame and coverage for blood used at any hospital in the country. (https://fmolhs.org/ololrmc/Pages/Give-Support/Blood-Donor-Center/Benefits-of-Blood-Donation.aspx)

[5] Mayo Clinic offers donors the Recipient Benefits Plan which is designed to provide limited financial assistance to blood donors, their immediate families or specific designees who have received blood transfusions. It helps these recipients recover some of the blood processing fees. (http://www.mayoclinic.org/patient-visitor-guide/florida/blood-donor-program/recipient-benefits-program)

may use at any hospital in the country. These donor insurance plans have closely mimicked the blood donor benefit policies which have been in place in some Asian countries for decades. In China and Korea, people with prior donations are granted preferential treatments when they need to receive blood transfusions in the future. People who donated blood before might be exempted from blood transfusion fees, or even get priority during blood shortages. In Hunan province in China, donors who give a minimum of 900 mL of blood are guaranteed unlimited free use of blood for their lifetimes; those who give between 600 mL and 900 mL can receive three times the amount donated; those who give less than 600 mL can receive twice the amount. Spouses, parents and children of blood donors are also granted free use of blood up to the amount donated.[6]

Although most people are aware of the small rewards that are sometimes given to blood donors, they might not be aware that they could automatically be enrolled in blood-donor insurance plans and enjoy benefits if they need to receive blood transfusions in the future. In this paper, I design an experiment that captures all the key features of the process of making the decision to donate blood. Within this experimental environment I test the impact of two different insurance-related incentives for donating: waiving the fee for donors for future units of blood purchased, and giving priority to donors for future blood needs. Results show that, while compared to the no-incentive baseline, both incentives increase donations, the difference is statistically significant for

[6] This regulation was announced by the Standing Committee of the National People's Congress on September 30, 2006 in Hunan Province of China.

the waiver incentive and is only marginally significant for the priority incentive. But when these two incentives are combined, donations increase the most.

2.2 Previous Literature

Currently, a limited amount of research has investigated common gift-exchange rewarding mechanisms in charitable giving. Researchers choose different reward medias to incentivize people to donate blood. However, a conflict of opinions arises over the matter. Lacetera et al. (2012) conduct a field experiment involving nearly 14,000 American Red Cross blood drives and show that economic incentives have a positive effect on blood donations without attracting ineligible donors. The effect increases with the incentive's economic value. However, a substantial proportion of the increase in donations is explained by donors leaving neighboring drives without incentives to attend drives with incentives; this displacement can also increase with the economic value of the incentive. They conclude that extrinsic incentives stimulate prosocial behavior, but the effect may be overestimated if displacement effects are not considered. Furse and Stewart (1982) conduct another field experiment, in which both a monetary incentive and a charity incentive (a promised contribution) are given for responses to a mail survey. This experiment includes three treatments: a no-incentive control, a personal cash payments treatment and a promised contribution to a charity treatment. They find that the charity incentive does not produce a significantly greater survey return rate than was obtained with the no-incentive control. However, personal cash incentives produce a significantly greater response rate than either the no-incentive baseline condition or the charity-incentive condition.

Other studies provide contradictory opinions on the effect of common gift-exchange incentives. Titmuss (1971) argues that monetary compensation for donating blood might crowd out the supply of blood donors. To test this claim, Mellström and Johannesson (2008) carry out a field experiment with three different treatments. In the first treatment, subjects are given the opportunity to become blood donors without any compensation. In the second treatment, subjects receive a payment of $7 for becoming blood donors, and in the third treatment, subjects choose between a $7 payment and donating $7 to charity. The results differ markedly between men and women. For men, the supply of blood donors is not significantly different among the three experimental groups. For women, there is a significant crowding-out effect. The supply of blood donors decreases by almost half when a monetary payment is introduced. There is also a significant effect of allowing individuals to donate the payment to charity, and this effect fully counteracts the crowding-out effect. Existing literature also includes Heyman and Ariely (2004), and Kube et al. (2012) and provides evidence that cash payments appear to be ineffective or even counter-productive in stimulating extra effort. Similarly, Newman and Shen (2012) conduct six experiments to examine the effect of thank-you gifts on charitable giving. They found that thank-you gifts actually reduce charitable donations.

Lacetera and Macis (2010) argue that a number of experimental studies have documented that financial rewards undermine the performance of charitable activities. They conduct an experiment through a survey administered to 467 blood donors in an Italian town. They vary incentives by providing donors with either 10 euros in cash or vouchers for purchasing some goods of the same nominal value. They find that donors are not reluctant to receive compensation. Although a large number of donors stop donating if they are given the cash, such effects disappear when donors are offered vouchers. Women especially dislike direct cash payments.

Non-financial incentives have been investigated in several studies. Reich et al. (2006) conduct a field experiment in which t-shirts were offered to blood donors as a blood donor recruiting strategy. Authors conclude that a t-shirt incentive is not effective, which suggests that other common nonmonetary incentives may be less effective than expected. Glynn et al. (2003) conduct a survey asking previous blood donors about what type of incentives would encourage them to donate more. They find that the incentives that are related to health are chosen the most. Goette and Stutzer (2008) examine two types of incentives: a lottery ticket from the Swill State Lottery and a free cholesterol test. They conclude that lottery tickets significantly increase donations, especially among less motivated donors. However, the cholesterol test leads to no significant impact on usable blood donations.

Kessler and Roth (2012) investigate a special incentive in organ donations: donor priority. They design a laboratory experiment modeled on the decision to register as an organ donor and investigate how changes in the management of organ waiting lists might impact donations. They find that an organ allocation policy giving priority on waiting lists to those who previously registered as donors has a significant positive impact on registration.

This paper is similar to the previously mentioned studies, in that I also investigate incentives to donate. However, this paper is the first, to my knowledge, that uses donor insurance benefits as incentives to increase blood donations. The intuitions behind the insurance-like incentives in organ and blood donations are similar. However, it differs from the organ donation paper due to the essential distinctions between donating organs and donating blood. The supply and demand of transplanted organs is extremely unbalanced. Offering priority when the donor needs organ transplantations could be an adequate incentive to encourage registration. In the case of blood donations, the demand and supply is moderately unbalanced. Different from organ donations, there are fewer constraints in blood donations. People not only decide whether they want to donate or not, but also how often they want to donate. The priority incentive may not be sufficient to encourage people to donate blood. To fill the gap between using the insurance-like incentives in organ and blood donations, our paper will investigate the impacts of different blood-donor insurance incentives using experimental methods. If the impact of these insurance-like incentives are tested to be effective, policy makers may consider widely encouraging blood banks to adopt these mechanisms and highlighting these benefits in blood donor recruiting advertisement.

2.3 Experimental Design

I design four experimental treatments (including the baseline) to model different blood-donor benefit mechanisms. Instructions to subjects are stated in neutral language (i.e., not in terms of blood donation) to focus on the effect of the incentives. The experiment includes three phases, and is designed to capture key aspects of the decision to donate blood. In some countries, people donate blood and enroll in donor insurance programs automatically. Everyone faces the risk of losing blood. Donors can enjoy preferential treatments if they need blood in the future. However, they don't know whether they will need the blood or not before making their decisions. In my experiment, subjects decide whether to donate a "unit" to a group account, which can be thought of as a blood bank, in the first phase. In the second phase, subjects participate in a lottery, and the results determine which subjects need to purchase a unit. This phase mimics the process by which individuals come to need blood. Losers in the lottery are like those who become ill or experience an accident, and therefore require a unit of blood. In the third phase of the experiment, subjects decide whether to purchase a unit from the group account. Depending on the treatment, prior donors will receive benefits in this phase, as explained below. Health status carries over from period to period. If a subject who needs a unit fails to obtain one, he/she is then unable to donate in the next period. Subjects who are not healthy enough to donate generate a new unit between periods, which is then available for donation. Earnings are determined by health status at the end of each period, taking into account the cost (if any) of obtaining a unit.

Thus, subjects make three decisions in each period. First, each eligible subject is asked to make a donation decision of whether to donate one unit from the private account to the group account (Phase 1). Second, each subject plays a lottery game, in which the chance of winning is 1/2 and the chance of losing is 1/2 (Phase 2). Third, losers in the lottery game have a chance to purchase one unit from the group account (Phase 3).

At the beginning of each treatment, subjects are randomly assigned into groups of six. The composition of the groups remains the same throughout the treatment. Each group begins with an empty shared group account, and each subject receives a private account containing 2 units and 30 tokens. The number of units in the private account indicates a person's health condition in terms of being eligible to donate and being in need of blood (see Table 1). Two units indicate the person is very healthy and eligible to donate blood. One unit indicates that the person is just doing alright; there is no need to receive a blood transfusion, but he/she is not eligible to donate blood either. Zero units indicate that the person needs to receive a blood transfusion.

In Phase 1, subjects who are currently holding two units in their private accounts will be asked whether they would like to donate one unit to the group account. To play the lottery game in Phase 2, each subject needs to select a number from 1 through 6, and then the computer will randomly generate 3 numbers, also from 1 through 6. If the subject-chosen number is one of the computer-chosen numbers, the subject will win the game; otherwise the subject will lose. A winner in the lottery game is able to retain units currently held in his/her private account. In other words, a winner doesn't lose anything. On the other hand, a loser will lose all the units currently held in the private account. Phase 1 and Phase 2 are the same across all the treatments.

Table 1: Unites and Health Conditions

Units	2	1	0
Eligible to Donate	Yes	No	No
Need Blood	No	No	Yes

In Phase 3, losers may request to purchase one unit from the group account. If received, the unit is deposited into the private account and a 30-token fee may be charged. Differences across treatments arise in this phase:

Treatment 1: Baseline. Units in the group account are randomly assigned among unit-requesting losers, and losers who receive a unit pay a 30-token fee;

Treatment 2: Waiver. Units in the group account are still randomly allocated among losers. However, losers who made a donation in Phase 1 will receive a unit for free and losers who didn't make the donation need to pay a 30-token fee;

Treatment 3: Priority. Losers who donated in Phase 1 receive one unit from the group account with certainty. Then the remaining units in the group account will be randomly allocated among the rest of the losers who did not donate in Phase 1. Any subject who successfully purchases one unit needs to pay 30 tokens;

Treatment 4: Wavier & Priority. Losers who donated in Phase 1 will not only receive one unit from the group account with certainty, but will also get it for free. Then the remaining units in the group account will be randomly allocated among losers who did not donate in Phase 1 and a 30-token fee will also be charged.

Each treatment consists of 6 periods. At the end of each period (after Phase 3), subjects will receive corresponding payoffs based on how many units they are holding in their private accounts (shown in Table 2). Token payoffs cumulate across periods, and units in the private account (health status) carry over from period to period, but not from game to game. Since blood expires, units in the group account do not carry over to the next period in the game. To reflect the ability to replenish blood, subjects who have fewer than two units at the end of a period receive one bonus unit to start the next period.

Table 2: Period Payoffs

Number of Units	2	1	0
Tokens Payoffs	125	100	0

Each subject participates in two treatments, a baseline always followed by either another baseline or an incentive treatment. We call the first treatment (first 6 periods of the experiment) Game1 and the second treatment (last 6 periods of the experiment) Game 2. There are 4 different types of combinations of Game 1 and Game 2: a baseline followed by a baseline, a baseline followed by a waiver treatment, a baseline followed by a priority treatment and a baseline followed by a waiver & priority treatment. This order imitates the fact that, in a typical policy implementation, an incentive is adopted to improve the current performance of an institution. The composition of groups is different in the second game from the first game. Subjects are assigned to a new group before starting the second game. At the end of the experiment, one of the two games is randomly chosen for the payment.

2.4 Theoretical Predictions

2.4.1 Nash Equilibria

Rational people maximize their payoffs. The payoff function in the this game is as follows,

$$E\pi = p_{win} * Payoff_{win} + p_{lose} * Payoff_{lose},$$

where p_{win} and p_{lose} indicate the probabilities of winning and losing respectively. $Payoff_{win}$ and $Payoff_{lose}$ present the payoffs received as a winner or a loser respectively. In order to simplify the game, I only consider the case in which all six group members are eligible to donate. Therefore, each group has seven possible cases: all six group members keep the unit; five people keep it and only one person donates; four people keep it and two people donate; three people keep it and three people donate; two people keep it and four people donate; one person keeps it and five people donate; and all six group members donate. For each treatment, I calculate each case scenario's payoffs from either keeping or donating (see Table 3, 4, 5 and 6).

At a Nash equilibrium, no player can gain more by changing his/her own strategy. Therefore, the Nash equilibrium for the baseline is "everyone keeps"; for the waiver treatment it is "everyone donates"; for the priority treatment it is "three people donate and three people keep"; and for the waiver & priority treatment it is "everyone donates."

Table 3: Payoffs – Baseline

Cases	Payoffs from Keeping	Payoffs from Donating
6 people keep	62.5	NA
5 people keep, 1 person donates	73.98	61.48
4 people keep, 2 people donate	84.38	71.88
3 people keep, 3 people donate	92.03	79.53
2 people keep, 4 people donate	96.04	83.54
1 person keeps, 5 people donate	97.32	88.46
6 people donate	NA	85

Table 4: Payoffs – Waiver Treatment

Cases	Payoffs from Keeping	Payoffs from Donating
6 people keep	62.5	NA
5 people keep, 1 person donates	73.98	66.41
4 people keep, 2 people donate	84.38	81.25
3 people keep, 3 people donate	92.03	92.19
2 people keep, 4 people donate	96.04	97.91
1 person keeps, 5 people donate	97.32	99.74
6 people donate	NA	100

Table 5: Payoffs – Priority Treatment

Cases	Payoffs from Keeping	Payoffs from Donating
6 people keep	62.5	NA
5 people keep, 1 person donates	73.98	85
4 people keep, 2 people donate	84.38	85
3 people keep, 3 people donate	92.03	85
2 people keep, 4 people donate	96.04	85
1 person keeps, 5 people donate	97.32	85
6 people donate	NA	85

Table 6: Payoffs – Waiver & Priority Treatment

Cases	Payoff from Keeping	Payoffs from Donating
6 people keep	62.5	NA
5 people keep, 1 person donates	73.98	100
4 people keep, 2 people donate	84.38	100
3 people keep, 3 people donate	92.03	100
2 people keep, 4 people donate	96.04	100
1 person keeps, 5 people donate	97.32	100
6 people donate	NA	100

2.4.2 Treatment Effect

There are dominant strategies for the baseline, waiver treatment and waiver & priority treatment. A subject should always keep no matter what other group members do in the baseline. However, the strategy of donating is dominated in both the waiver treatment and waiver & priority treatment. There is a mixed strategy equilibrium in the priority treatment. The equilibrium probability of donating is 0.51 (see Appendix A.2 for detailed information). The hypothesis on the treatment effect is that the contribution level is raised higher by waiver or waiver & priority than by priority on its own.

2.5 Results

2.5.1 Descriptive Statistics

The experimental sessions were conducted at Rice University and Texas A&M University from April to June 2015. Each session included either 12 or 18 subjects. A total of 150 subjects participated in the experiment. Table 7 displays how many subjects are included in each of the combinations of the games. The duration of each session was around 90 minutes. The average earnings were $22.60, including a $5 show-up fee.

Table 7: Number of Groups (Subjects) in Each Combination of the Two Games

	Game 2			
Game 1	**Baseline**	**Waiver**	**Priority**	**Waiver & Priority**
Baseline	6 Groups (36 Ss)	6 Groups (36 Ss)	5 Groups (30 Ss)	8 Groups (48 Ss)

Recall that only the subjects with two units are eligible to make donation decisions in Phase 1 in each period of the game. Subjects with one or zero units at the beginning of each period were not eligible to donate. They were not able to make any donation decisions at all for that period. When investigating subjects' decisions, I focus on decisions made by subjects who had the chance to donate.

Table 8 is a summary table that shows the average donation rate in each game by different combinations of treatments. The donation rate is equal to the number of times a subject donated over the number of periods the person was eligible to donate. A paired t test is used to examine the difference on average contribution rate between Game 1 and Game 2. When subjects play the baseline twice, the average donation rate is significantly decreased from 0.6 in Game 1 to 0.48 in Game 2 (p = 0.009). For the combinations of baseline – waiver and baseline – waiver & priority, average donations rates under the incentive treatments are significantly increased compared to the baseline (p = 0.075 and p = 0.000, respectively). However, the priority treatment does not have a big influence on the average donation rate compared to the baseline (p = 0.882). Additionally, when comparing Game 1's different baselines, there is no significant difference across the different combinations (Kruskal-Wallis test, p = 0.185).

Table 8: Average Contribution Rates

Combination	**Game 1**	**Game 2**	**Wilcoxon matched-pair signed-rank test**
Baseline – Baseline (n=36)	0.60 (0.061)	0.48 (0.064)	p = 0.009
Baseline – Waiver (n=36)	0.47 (0.059)	0.60 (0.063)	p = 0.075
Baseline – Priority (n=30)	0.59 (0.071)	0.55 (0.078)	p = 0.882
Baseline – Waiver & Priority (n=48)	0.45 (0.056)	0.75 (0.046)	p = 0.000

2.5.2 Experimental Data vs. Theoretical Predictions

In this section, I am comparing the average donation rate and the Nash equilibrium. Table 9 reports that the average donation rate deviates from the Nash equilibrium in the baseline, waiver treatment and waiver & priority treatment (p = 0.000). In the priority treatment, there is no significant difference between the average donation rate and the Nash equilibrium (p = 0.909). Subjects are more generous in the baseline than the theory predicts. Although the full donation level is not achieved, waiver and waiver & priority help to increase their respective donation rates.

Table 9: Average Donation Rate Compared to the Nash Equilibrium

Treatment	**Average donation rate**	**Nash equilibrium**	**Wilcoxon signed-rank test**
Baseline (n=150)	0.52 (0.031)	Donation rate = 0	P = 0.000
Waiver (n=36)	0.60 (0.063)	Donation rate = 1	P = 0.000
Priority (n=30)	0.55 (0.078)	Donation rate = 0.51	P = 0.909
Waiver & Priority (n=48)	0.75 (0.046)	Donation rate = 1	P = 0.000

2.5.3 Within-subject Analysis

Figure 1, 2, 3 and 4 present the average donation rates over time for each of the four combinations of the two games, respectively. Figure 1 demonstrates average donation rates under the situation of the baseline – baseline combination. Since both Game 1 and Game 2 are the same baseline treatment, the two average donation rate lines have similar steady downward trends. For the baseline – waiver, baseline – priority and baseline – waiver & priority combinations, the average donation rate drops to a low level in the last period of Game 1, and then it is boosted to a high level right after being influenced by the shock of an incentive treatment. In Game 2, the average donation rate begins to decline or fluctuate after the first period under the baseline – waiver and baseline – priority combinations. The average donation rates are well sustained at a high level in the waiver & priority treatment under the baseline – waiver & priority combination.

Figure 1: Average Donation Rate: Baseline – Baseline Combination

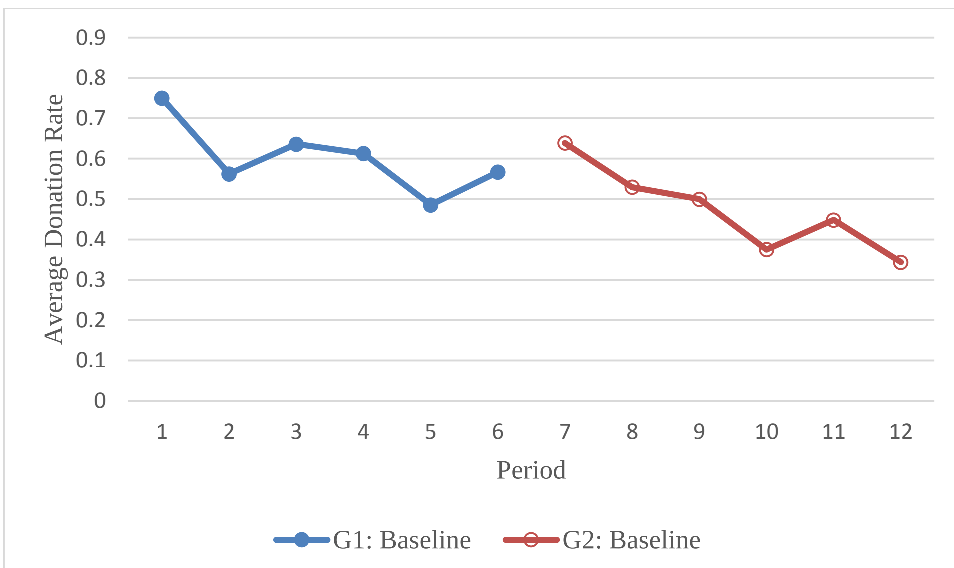


Figure 2: Average Donation Rate: Baseline – Waiver Combination

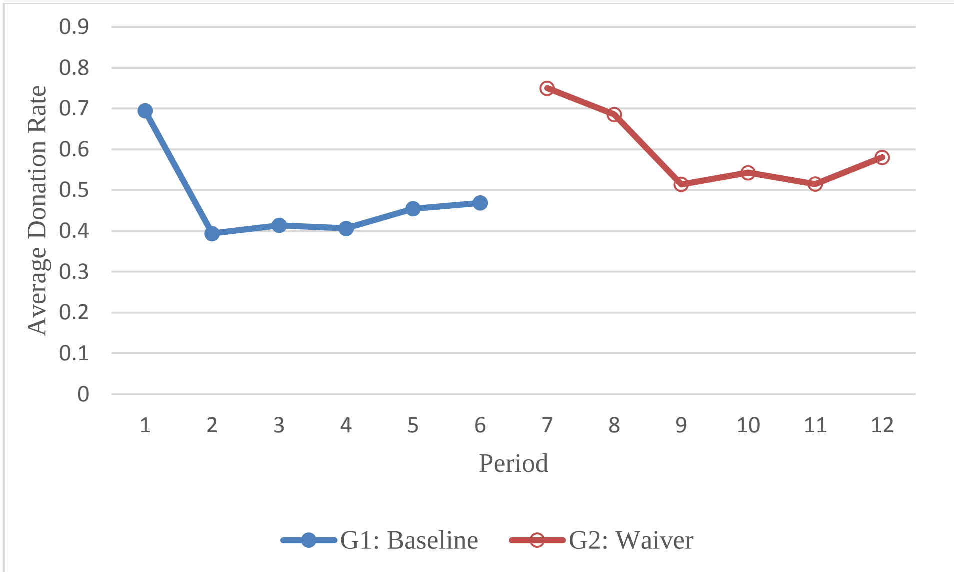


Figure 3: Average Donation Rate: Baseline – Priority Combination

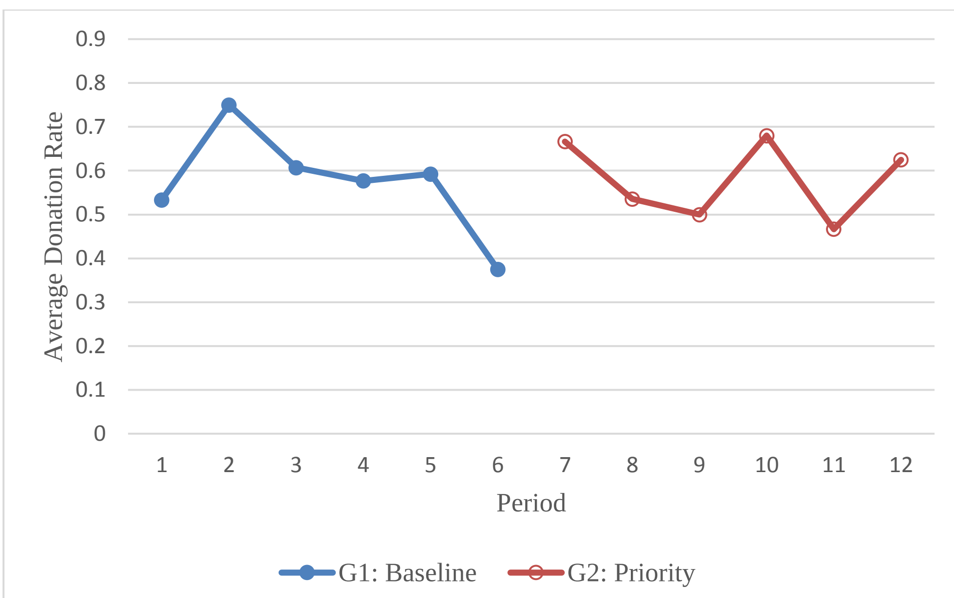
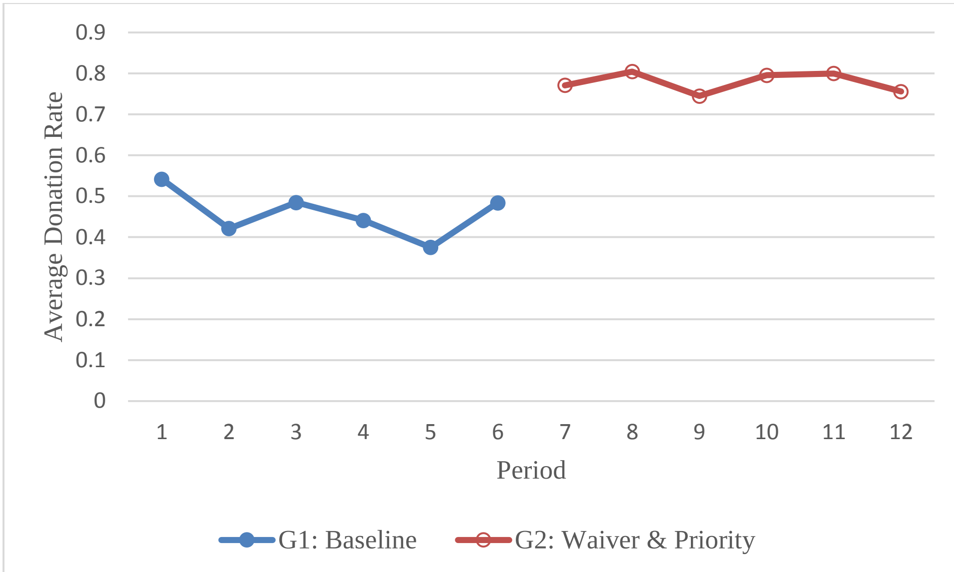


Figure 4: Average Donation Rate: Baseline – Waiver & Priority Combination



Regression analysis is shown in Table 10 and 11, which present results using panel data within each combination. The dependent variable, donation, is a dummy variable. Conditional on the subject being eligible to donate, if the subject decides to donate in Phase 1, the dependent variable is 1; otherwise, it is 0. Since donating or not is a binary decision, the logit model is chosen to analyze the data.

Independent variables are also dummy variables. Game 2 is 1 if it is the second game in the combination; otherwise it is 0. Thus, Game 2 could be the baseline, the waiver treatment, the priority treatment or the waiver & priority treatment. Before knowing the period earnings, subjects are informed about how many subjects in the group requested to purchase a unit in Phase 3, and how many subjects successfully make the purchase. The independent variable, $\text{Excess demand}_{t-1}$, represents whether everyone who requested to purchase successfully made the purchase or not in the preceding period. If there were not enough units in the group account for all of the subjects who requested to purchase units in the previous period, the Excess demand_{t-1} is 1; otherwise, it is 0. The independent variable, winner_{t-1}, is 1 if the subject was a winner in the lottery game in the preceding period; otherwise, it is 0. Regression 1a investigates the restart effect. Regression 2a, 3a and 4a focus on the incentive treatment effect. In order to control other factors that could possibly affect subjects' decision making, I add independent variables, Excess demand_{t-1} and winner_{t-1}, into regression 1b, 2b, 3b and 4b.

Table 10 and 11 present the results from running the logit regression and the marginal effects of its coefficients, respectively. If subjects play the baseline treatment again as Game 2, the probability of donating in Phase 1 of each period is significantly decreased by more than 0.2. Regression 3a and 3b show that compared to the baseline, adding the incentive treatment of priority does not make any significant difference on the likelihood of donating. However, the waiver treatment significantly increases the probability of donating by 0.21 in regression 2a and 0.26 in regression 2b, and the treatment combining both incentives, waiver & priority, greatly raises the probability of donation by 0.41 in regression 4a and 0.44 in regression 4b.

Table 10: Within-subject Analysis for Each Combination: Logit Model

Independent Variables	Dependent Variable: Donation							
	Baseline-Baseline		Baseline-Waiver		Baseline-Priority		Baseline-Waiver & Priority	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Game 2	-1.00***	-1.13***	0.84***	1.05***	-0.30	-0.36	2.00***	2.11***
	(0.292)	(0.340)	(0.257)	(0.309)	(0.337)	(0.411)	(0.266)	(0.399)
Excess demand$_{t-1}$		0.46		0.28		2.53***		-0.05
		(0.411)		(0.402)		(0.819)		(0.419)
Winner$_{t-1}$		0.26		-0.27		-0.75*		-0.64**
		(0.350)		(0.316)		(0.469)		(0.300)
Constant	0.79	0.34	-0.20	-0.46	0.64	0.85	-0.24	-0.04
	(0.496)	(0.583)	(0.360)	(0.469)	(0.569)	(0.753)	(0.309)	(0.455)
# of obs	392	320	400	328	324	264	491	395
# of subjects	36	36	36	36	30	30	48	48

Note: Standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5% and 1%, respectively.

Table 11: Within-subject Analysis for Each Combination: Marginal Effect of the Logit Model

Independent Variables	Dependent Variable: Donation							
	Baseline-Baseline		Baseline-Waiver		Baseline-Priority		Baseline-Waiver & Priority	
	(1a)	(1b)	(2a)	(2b)	(3a)	(3b)	(4a)	(4b)
Game 2	-0.24***	-0.28***	0.21***	0.26***	-0.07	-0.08	0.41***	0.44***
	(0.073)	(0.078)	(0.065)	(0.077)	(0.080)	(0.098)	(0.070)	(0.096)
Excess demand$_{t-1}$		0.11		0.07		0.60***		-0.01
		(0.103)		(0.101)		(0.213)		(0.088)
Winner$_{t-1}$		0.07		-0.07		-0.18		-0.14**
		(0.088)		(0.079)		(0.113)		(0.065)
# of obs	392	320	400	328	324	264	491	395
# of subjects	36	36	36	36	30	30	48	48

Note: Delta-method standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5% and 1%, respectively.

2.5.4 Between-subject Analysis

In order to compare the difference between treatments in Game 2, I pool the data together to investigate the treatment effects between subjects in different combinations of the two treatments. The Game 2 variable is used to control for the order effect. Game 2 is 1 if it is the second half of the experiment no matter what treatment it is; otherwise, it is 0. Waiver is 1 if it is in the waiver treatment and is 0 if it is not. Priority is 1 if it is in the priority treatment and 0 if it is not. Same with the waiver & priority variable, Waiver & priority is 1 if it is in the waiver & priority treatment and 0 if it is not.

Table 12 includes two models. Model 1 explores the different treatment effects by controlling the order effect. More controls are added into Model 2. Marginal effects of the coefficients are shown in Table 13. Results from Model 1 show that by playing the second game for another 6 periods, the probability of donating is decreased by 0.2 in general. This is mainly because subjects are more and more reluctant to make donations when there is no incentive offered. Preferential treatments are only conducted in Game 2. Compared to the baseline in Game 2, the waiver treatment, priority treatment and waiver & priority treatment significantly increase the probability of subjects donating. The waiver treatment, priority treatment and waiver & priority treatment increase the probability by 0.39, 0.16 and 0.67, respectively. Offering both incentives at the same time, the waiver & priority treatment, has the biggest effect. The priority treatment alone has the smallest impact on influencing the probability of donating. When adding the control of the information on excess demand and the winning result from the preceding period, the waiver treatment and waiver & priority treatment still significantly increase the probability of donating. However, the priority treatment is no longer significant. If subjects know that not all of the people who requested to purchase a unit have successfully made their purchase in the last period, the probability of them donating in the current period is increased by 0.1. Winning in the preceding period will decrease the probability of donating in the current period by 0.08.

Table 12: Between-subject Analysis for Pooled Panel Data: Logit Model

	Dependent Variable: Donation	
Independent Variables	Model 1	Model 2
Game 2	-0.84***	-0.93***
	(0.270)	(0.310)
Waiver	1.64***	1.94***
	(0.366)	(0.427)
Priority	0.67*	0.61
	(0.402)	(0.457)
Waiver & Priority	2.84***	3.34***
	(0.372)	(0.459)
Excess demand$_{t-1}$		0.40*
		(0.222)
Winner$_{t-1}$		-0.34
		(0.169)**
Constant	0.17	0.05
	(0.210)	(0.267)
# of obs	1607	1307
# of subjects	150	150

Note: Standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5% and 1%, respectively.

Table 13: Between-subject Analysis for Pooled Panel Data: Marginal Effect of The Logit Model

	Dependent Variable: Donation	
Independent Variables	Model 1	Model 2
Game 2	-0.20***	-0.23***
	(0.064)	(0.076)
Waiver	0.39***	0.47***
	(0.088)	(0.105)
Priority	0.16*	0.15
	(0.095)	(0.111)
Waiver & Priority	0.67***	0.81***
	(0.092)	(0.115)
Excess demand$_{t-1}$		0.10*
		(0.054)
Winner$_{t-1}$		-0.08**
		(0.041)
# of obs	1607	1307
# of subjects	150	150

Note: Delta-method standard errors are in parentheses. *, **, and *** denote significance at the 10%, 5% and 1%, respectively.

2.6 Conclusion

From the experiment, I observe that if there is no incentive offered to people for donating, people are less likely to donate as time passes as they learn that free riding on other people's donations is the best strategy. In this paper, two different insurance-like incentive treatments are investigated along with a combined treatment. In the waiver treatment, units are provided free for previous donors. In the priority treatment, donors who lose in the lottery game are guaranteed the chance to purchase units. In the combined treatment, both incentives are implemented. People behave more generously in the baseline than the theory predicts. However, consistent with the comparative-statistic prediction of the theory, results demonstrate that all three preferential treatments increase the probability of donating compared to the baseline (no-incentive) treatment. Results also indicate that the treatment that combines waiver and priority has the strongest impact on making a donating decision, while the treatment that includes only priority has the weakest impact among the three different treatments.

These results imply that offering blood donors insurance plans could be an effective way to encourage more contributions in the real world. For people who donated blood to save other people's lives, we should show our appreciation by giving them priority access when they encounter health issues that require them to have blood transfusions. Providing donors with both a transfusion fee waiver and priority access to blood is the most powerful policy. Although blood donor insurance has been adopted by a few blood banks, more blood institutes should take these special insurance-like incentives into consideration.

3. GROUP SIZE AND THE EFFECTIVENESS OF PUNISHMENT IN PUBLIC GOODS GAMES

3.1 Introduction

The public goods game has been one of the most influential games in economics. What especially interests people is that human behavior in the real world deviates from the theoretical prediction that people will not contribute to the public goods at all.

In order to separate how factors influence the contribution level individually, lab experiments have been adopted as a method to study public goods game for decades. Studies show that different factors that affect people's contribution levels in different ways. One of the issues that draws researchers' attention is the correlation between the contribution level and the group size. Olson (1968) theoretically predicts that the provision of the common good would decrease when the group size increases. Isaac et al. (1994) conduct a public goods game varying the group sizes from 4, 10, 40 to 100 for different Marginal Per Capita Returns (MPCR) respectively. Authors conclude that when the MPCR is lower, the contributions in the group size of 40 and 100 treatments are significantly greater than in the group size of 4 and 10 treatments. Isaac and Walker (1998) examine the relationship between variations in group size and provision of public goods. The results demonstrate that increasing group size increases contributions when the MPCR is low. However, this effect is weak when MPCR is high. Goeree et al. (2002) compare treatments of two-person group and four-person group. They do not find any clear effect that the group size has on public goods provision.

In order to increase public-good contributions, different mechanisms have been investigated, among which the punishment mechanism is approved to be one of the most effective methods to sustain cooperation. Fehr and Gächter (2000) study how costly punishment affects contribution level under both “stranger” and “partner” protocols. In the experiment, participants are randomly divided into groups of four. Using a within-subject design, subjects play a public goods game for a total of 20 periods, 10 periods with no punishment and the other 10 periods with punishment. In the punishment treatment, one more stage is added. After subjects play the normal linear public goods game, they are able to monitor other group members’ contributions and punish each other. The paper shows that although the punishment is costly, it significantly raises contribution levels compared to those in the treatment without punishment. Gürerk et al. (2006) analyze public goods provision where participants can decide either to stay in a sanctioning institution or a sanction-free institution. The findings show that the population migrates to the sanctioning institution with strong cooperating behavior.

The incentives for peer monitoring can be greatly affected by the group size since it becomes harder for individuals to monitor others when in a large group (Stiglitz 1993). Furthermore, the punishment mechanism would be less effective to elicit public goods contributions. There have been only a few experimental studies that examine the relationship between group size and punishment in public goods games. Following Fehr and Gächter (2000), Carpenter (2007) stimulate different punishment environment where each participant can punish all other group members, half of the group members, only one of the group members or cannot punish at all. Participants are assigned in either the high MPCR (0.75) or the low MPCR (0.375) treatment. Sessions are run with either small (five-person) groups or large (ten-person) groups. They find that since punishment does not fall in ten-people treatments, contributions in the large groups are no lower than those in small groups.

In order to directly address the question of how group size impacts the effectiveness of the punishment mechanism when it is introduced, we carefully pick three variations for the group size, 2, 8 and 24. 2 is the smallest group that can be formed to conduct a public goods game. Due to the capacity of our lab, we can only run a maximum of 24 subjects. 8 is the number we decide to pick between 2 and 24, which hasn't be invested with.

The detailed experiment design and procedure are detailed in the following section. In section 3, we discuss the results of data collected from the experiment. The conclusion is made at the end.

3.2 Experimental Design and Procedure

We use the public goods game design from Fehr and Gächter (2000) with a few modifications. Our game is different in the following two ways:

1. Investigating the effect of changing group size in the public goods game is tricky. Andreoni (2007) notes that people, especially those who are altruistic, care not only about the MPCR, but also the MGR (Marginal Group Return). As long as we want to study the effect of group size, it is impossible to control both MPCR and MGR since $MGR = MPCR \times Group\ Size$. If the MPCR stays constant, the total surplus increases with an increasing group size. If the MGR stays constant, the MPCR decreases with an

increasing group size. In order to balance both the effects of MPCR and MGR on the contribution level and produce an initial average contribution of 50% of the subjects' endowments, we calibrate MPCR based on prior experiment following the rule below,

$$MPCR = \frac{1}{n} + 0.1$$

Then we round the MPCR up to the nearest multiple of 0.025.

2. We use a simple linear punishment cost function. This cost function ensures that each participant can always afford to assign up to three punishment points. Therefore, in each period all the participants have equal punishment power.

Each participant can only distribute up to three punishment points in total among the other group members. Therefore, each subject receives an average maximum of three punishment points no matter what the group size is under the punishment condition.

Our design involves two treatment variables, group size and punishment. The group size varies from 2, 8 to 24. Punishment is a dummy variable, either with no punishment or with punishment. Six treatments ($group\ size\ of\ 2, 8\ or\ 24 \times with\ no\ punishemnt\ or\ with\ punishment$) are consist of all the combinations of two treatment variables. Calculated by MPCR rule in our experiment, the MPCR is 0.60, 0.25, or 0.15 for the group size of 2, 8 or 24 respectively. Treatment conditions are listed in Table 14.

Table 14: Treatments

Within – subject / Between – subject	**No Punishment (1-10 periods)**	**Punishment (11-20 periods)**
n=2 MPCR=0.60	Treatment 1A	Treatment 1B
n=8 MPCR=0.25	Treatment 2A	Treatment 2B
n=24 MPCR=0.15	Treatment 3A	Treatment 3B

We use the partner-matching protocol considering the difficulties of using a stranger-matching protocol for 24-peson group treatments. At the beginning of each session, subjects are randomly assigned in to groups. The group composition remains the same throughout the experimental session. Each session includes 20 periods. Participants are told to play a standard public goods game with no punishment for 10 periods and then they are informed to play another 10 periods with punishment. The no-punishment condition is always followed by the punishment condition. This is because in previous studies, it has been confirmed as a robust result that punishment can significantly raise contribution levels no matter what the order is when a within-subject design is used. We also try to imitate that in the real world, a mechanism is usually brought in later to improve the performance of the current institution.

Each period, each participant is endowed with 20 tokens. Under the "no punishment" condition, subjects need to decide either keep these tokens in the private account for himself or invest g_i tokens ($0 \leq g_i \leq 20$) into the group account. The period payoff for each subject i is the tokens in his private account and the total tokens in the group account multiplied by the MPCR. The payoff equation for subject i is given by

$$\pi_i^{np} = 20 - g_i + MPCR \times \sum_{j=1}^{n} g_j$$

where j is a subject in a group, n is the size of the group, and i is the subject.

Under the "punishment" condition, one more decision stage is added after all the subjects make their investment decisions. At the second stage, subjects are able to review each of the other group members' investment to the group account. Subjects are then granted an opportunity to punish each other.

Different from Fehr and Gaechter (2000), we use a linear punishment function that each punishment point assigned costs the punisher one token and deducts three tokens from the punishee's earnings in the first stage. Each subject can assign up to a total number of three punishment points among the other group members no matter what the group size is. The minimum payoff after stage two is zero. The period payoff function for subject i is written as,

$$\pi_i^{p} = 20 - g_i + MPCR \times \sum_{j=1}^{n} g_i - \sum_{j \neq i}^{n} p_{ij} - 3 \times \sum_{j \neq i}^{n} p_{ji} \qquad \pi_i^{p} \geq 0$$

Where p_{ij} is the number of punishment points subject i assigns to his group member j, and p_{ji} is the number of punishment points subject i receives from his group member j.

Period payoff is cumulative. The total payoff is the sum of the 20-period payoffs.

The experiment was conducted in the Economic Research Lab in Texas A&M University from April to June, 2015. Student participants were recruited through the Online Recruitment System (ORSEE) for Economic Experiments. 7 sessions were run with 168 subjects. Each session lasted about 70 minutes. The average earnings for each participant were $24.78 including a $5 show-up fee.

3.3 Results

Result 1: The punishment mechanism significantly raises the average contribution level with a group size of 8 or 24; however, the increase in contributions in the punishment treatment is very small compared to in the no-punishment treatment with a group size of 2.

Table 15 presents the data summary. In columns three and four, we show the average contribution over all ten periods of no punishment and with punishment treatment respectively. In column five, we list the average punishment points assigned/received by each participants when in the punishment treatment. This table reports that when group size is 8 or 24, introducing the punishment mechanism increases the contribution two times more than in the no punishment treatment. Nevertheless, when the group size is 2, the increase in the contribution is very small compared to the contribution in the no punishment treatment.

We also use the nonparametric method, Wilcoxon rank sum test, to confirm that the difference in contribution between the no-punishment condition and the punishment condition is not significant (p=0.796) when the group size is 2. Nevertheless, the contributions in the no-punishment condition and those in punishment condition are significantly different when the group size is either 8 or 24 (p=0.000 in both cases).

Table 15: Data Summary

	Sample Size	No Punishment Avg Contribution per Capita	Punishment Avg Contribution per Capita	Avg Punishment Points per Capita
N=2 MPCR=0.60	24 subjects 12 groups	9.525 (6.88)	10.321 (7.49)	0.417 (0.65)
N=8 MPCR=0.25	48 subjects 6 groups	8.979 (5.42)	15.673 (5.91)	0.571 (1.13)
N=24 MPCR=0.15	96 subjects 4 groups	8.499 (5.46)	14.898 (5.57)	1.063 (1.96)

Result 2: Under the group size of 2 condition, average contributions over time are relatively stable in both no-punishment punishment and treatments; In contrast, under both group size of 8 and 24 conditions, average contributions drop dramatically over time in the no punishment treatment. The punishment not only significantly increases the contributions, but also well sustains the cooperation.

Result 2 is supported by Figure 5, 6, and 7. These figures show the average contributions over time when group size is 2, 8 and 24 respectively. For group size of 2 (Figure 5), the average contributions over time fall within the boundary of 7 and 12 in both the no-punishment treatment and punishment treatment. The punishment mechanism raises the average contribution level just a little. Participants decrease their contributions in the last period in both no-punishment treatment and punishment treatment. The average contributions in the last round reach their lowest values under both no punishment and punishment conditions.

For group size of 8 and 24 (Figure 6 and 7), under the no punishment condition, participants begin to drop their contributions after the 3rd period. Introducing the punishment mechanism immediately increase participants' contributions and the cooperation is sustained till the last period.

Figure 5: Average Contribution – Group Size of 2

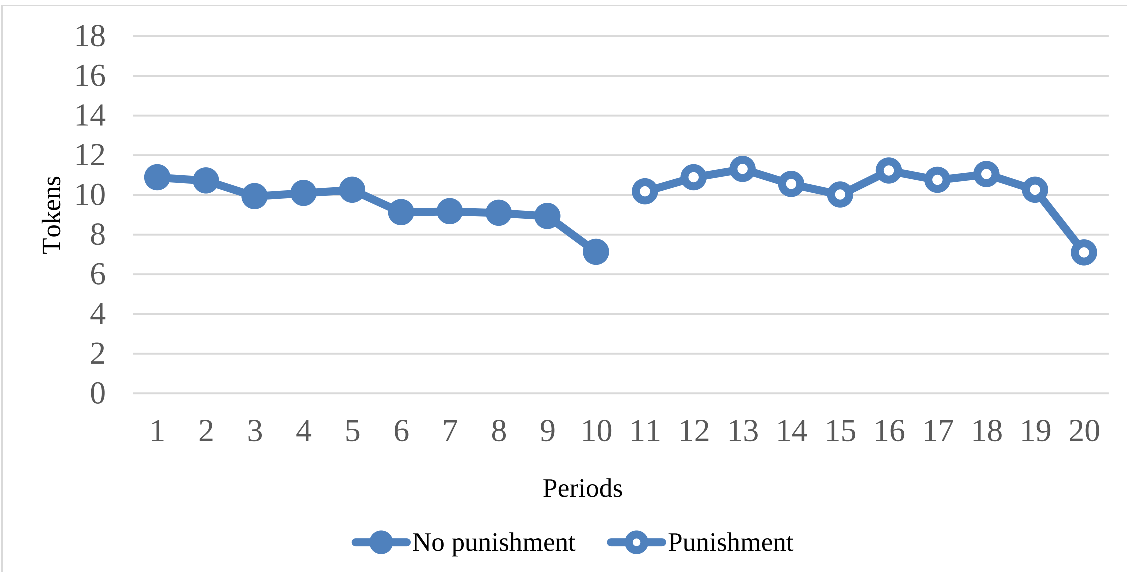


Figure 6: Average Contribution – Group Size of 8

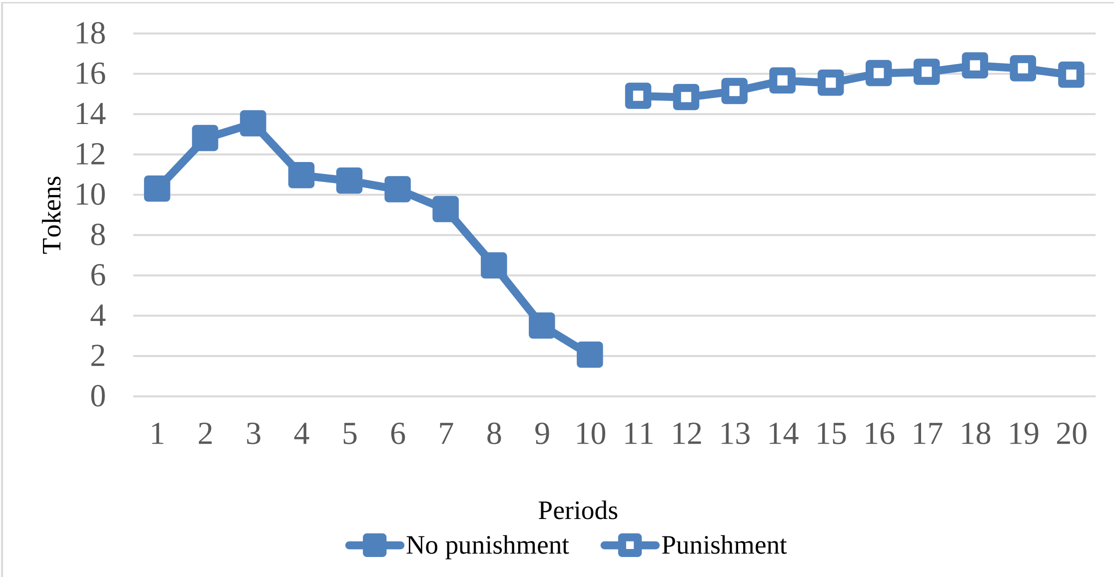
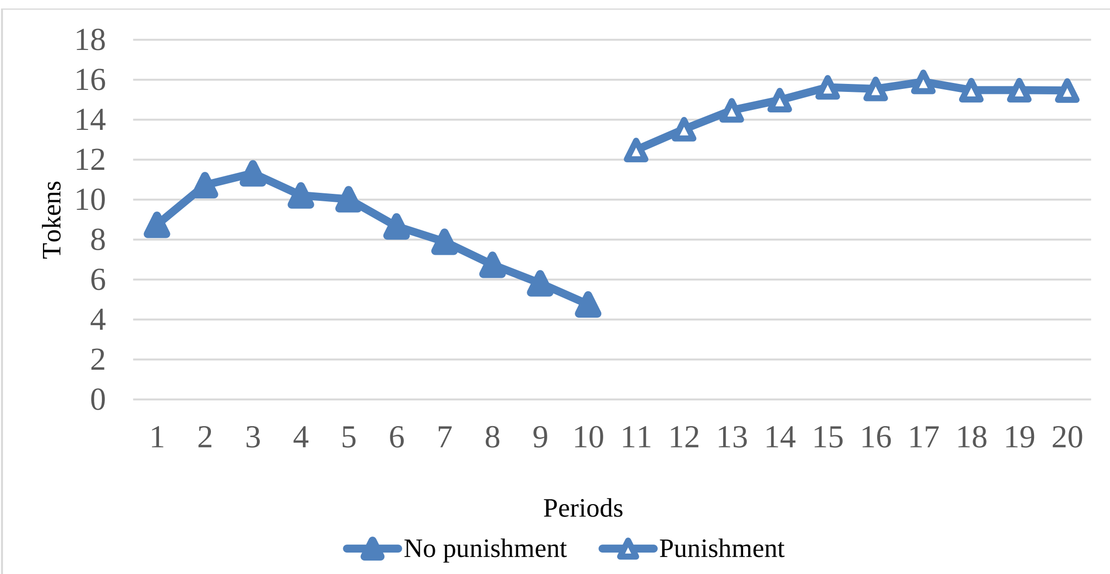


Figure 7: Average Contribution – Group Size of 24



Result 3: The situation of "free riding" is notably improved by the punishment mechanism when the group sizes are 8 and 24. This is not the case when the group size is 2.

Figure 8, 9 and 10 show the distributions for the pooled data for group size of 2, 8 and 24 treatments with and without punishment. In figure 8, 24% of individual choices are $g_i = 0$ and 29% of individual choices are $g_i = 20$ with group size of 2. After introducing the punishment, 27% of individual choices are $g_i = 0$ and 34% of individual choices are $g_i = 20$. The two distributions are not significantly different from each other (Kolmogorov-Smirnov test: p=0.440 in both cases).

Figure 9 and 10 present distributions for group size of 8 and 24, respectively. In figure 2b, the relative frequency of choosing $g_i = 0$ is 27% and it decreases to 3% in punishment treatment. Thirty-three percent of the choices are to make full contribution in the no punishment treatment. Adding the punishment mechanism almost doubles the number of full contribution decisions to 63%. The contribution distributions are significantly changed in punishment treatments in both figures (Kolmogorov-Smirnov test: p=0.000 in both cases). This provides the evidence that punishment can effectively solve the free riding problems when group size is relatively large.

Figure 8: Distribution of Contributions – Group Size of 2

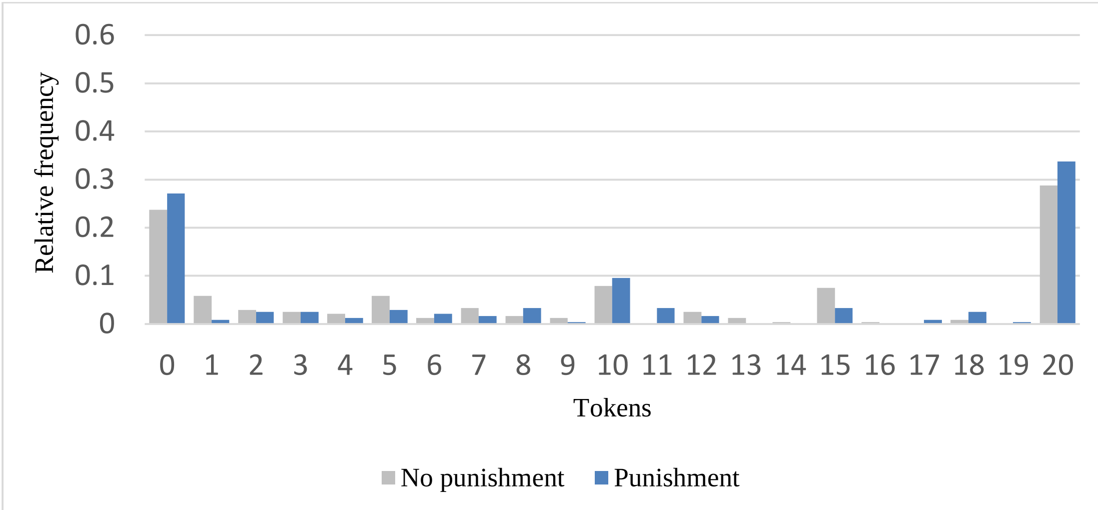


Figure 9: Distribution of Contributions – Group Size of 8

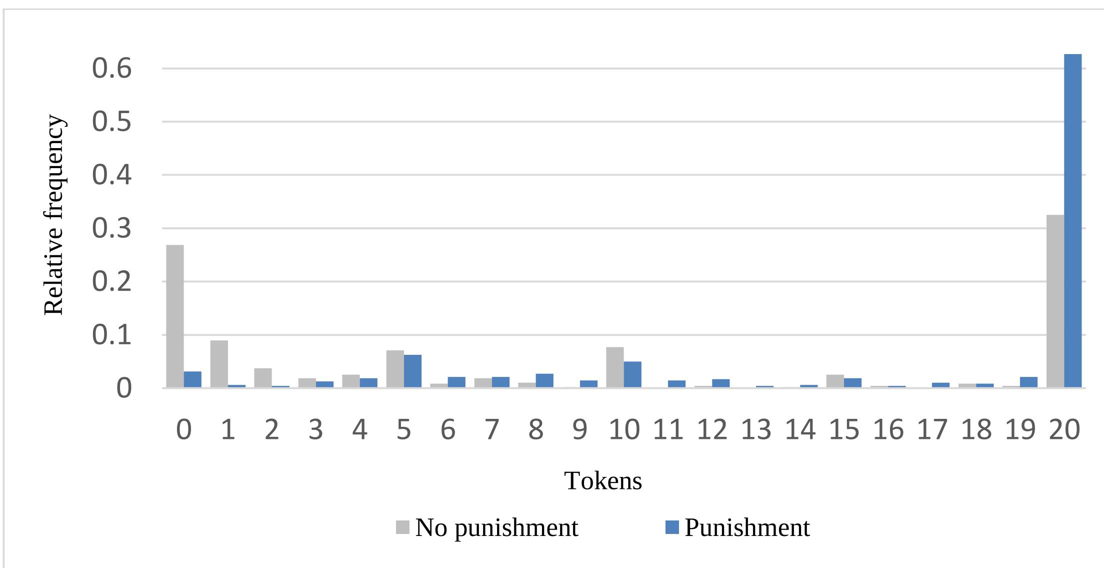
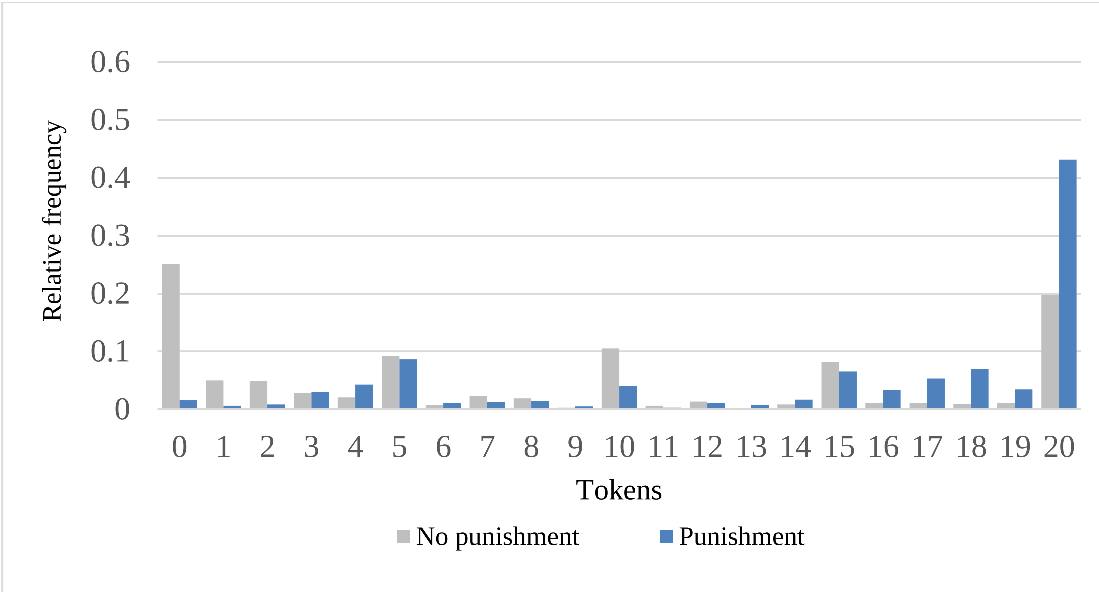


Figure 10: Distribution of Contributions – Group Size of 24



Result 4: Punishment is used more often when group is large.

Figure 11, 12 and 13 show the probability of people who assign punishment points to their group members over periods in a group size of 2, 8 or 24 treatment respectively. Punishment is used most often when the group size is 24, although there is a decreasing trend.

Figure 11: Proportion of People Who Punish – Group Size of 2

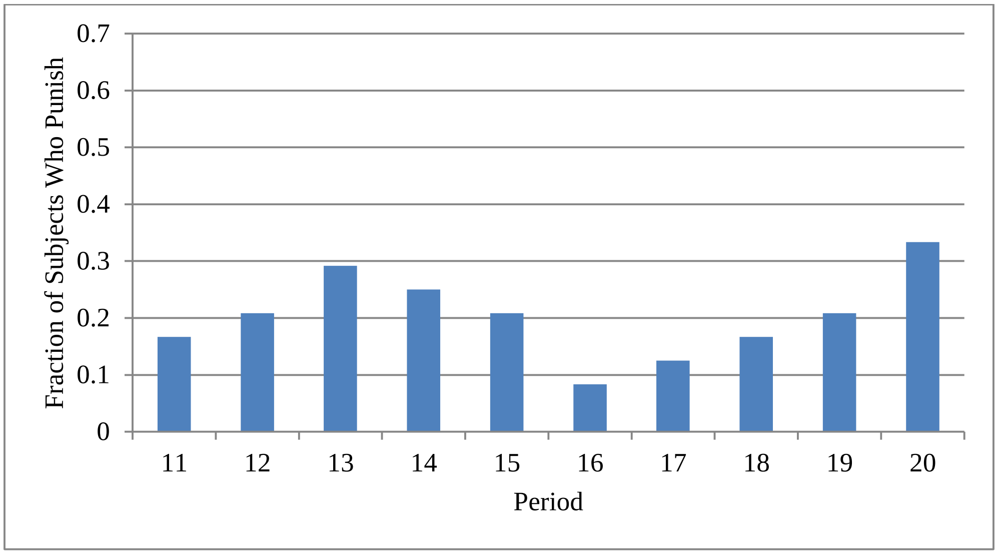


Figure 12: Proportion of People Who Punish – Group Size of 8

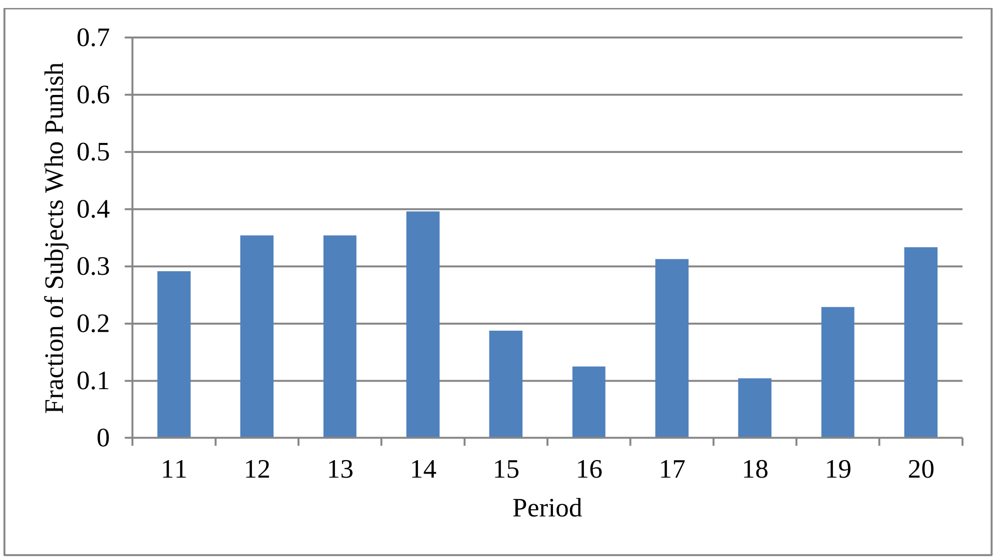
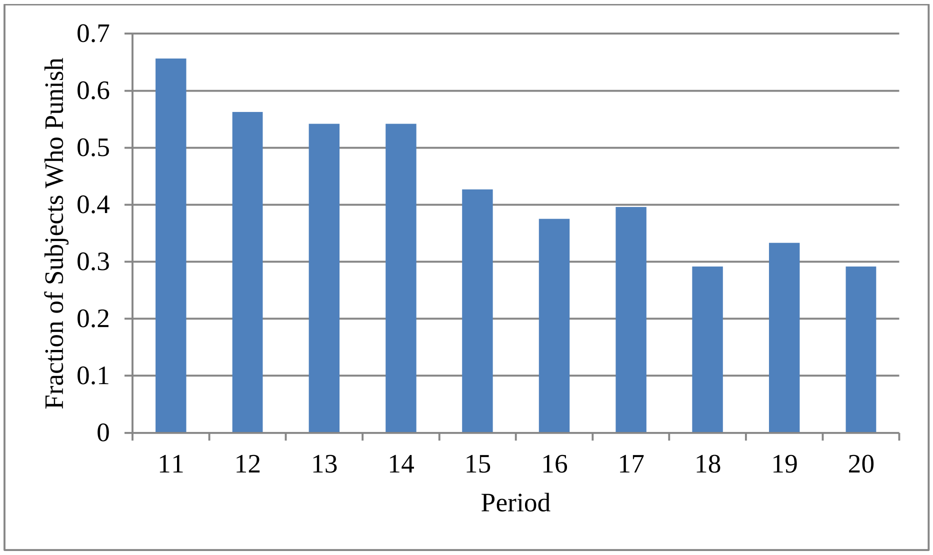


Figure 13: Proportion of People Who Punish – Group Size of 24



Result 5: Punishment is more responsible for low contributions when group size is large.

Table 16 contains three Tobit regressions for treatments with different group sizes, each with punishment. The dependent variable is a participant's "received punishment points". Independent variables are "other's average contribution" which is the average contribution of all the other group members, "positive deviation" which is the difference of a subject's contribution and the average contribution of his group members if his contribution is greater than that average contribution, and "absolute negative deviation" which is the absolute value of the difference of a subject's contribution and other group members' average contribution if his contribution is lower than the average. If a participant's own contribution is equal to the average contribution of his group members, the "positive deviation" is set to be 0.

Table 16 indicates a strong correlation between received punishment points and absolute negative deviation. The lesser a participant's contribution is than other group members' average contribution, the more punishment points this participant would receive. A participant would receive 0.5 punishment points for every token less than the average contribution when the group size is 2. The received punishment points is increased to 1 with group size of 8 and to 1.98 with group size of 24. The punishment is more responsible for negative deviations when the group size is larger.

Table 16: Determinants of Getting Punished: Random Effect Tobit Results

	Dependent Variable: Received Punishment Points		
Independent Variables	Group Size of 2	Group Size of 8	Group Size of 24
Other's Average Contribution	0.34***	0.20***	0.27***
	(0.119)	(0.065)	(0.074)
Absolute Negative Deviation	0.50***	1.00***	1.98***
	(0.135)	(0.077)	(0.090)
Positive Deviation	0.12	-1.02**	0.13
	(0.104)	(0.453)	(0.135)
Constant	-2.11	-1.40	-11.25***
	(-1.301)	(-1.014)	(-1.307)
N	24	48	96
Wald Chi²(3)	15.08***	179.54***	509.10***
Log Likelihood	-139.77	-248.63	-809.76

Note: Standard errors are in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% respectively. The regression is a Tobit with both upper and lower censoring and individual random effects.

Result 6: Contributions respond more to punishment when the group size is large.

Result 5 has shown that contributions lower than the other group members' average contribution are punished more often. Although distributing punishment points to others are costly, people still punish those who made low contributions, especially in a repeated game, hoping to teach them a lesson, hoping that low contribution makers would raise their contributions in future periods.

The MPCR and the group size change together. In order to isolate the effect of group size and rule out the effect of nonfixed MPCR on punishment, we run a regression on each separate data set of treatment with different group sizes respectively, then use Hausman test to compare the regression coefficients.

Table 17 includes three regressions to test whether punishment works in the way that people expect. First column shows punishment cannot significantly increases contributions with a group size of 2. Second column illustrates that punishment significantly increase people's contribution by 16.54 points with a group size of 8. The last column indicates that punishment raises the contribution by 10.80 with a group size of 24. The Hausman test demonstrates that the efficiency of punishment is significantly much higher to increase contributions levels when in large groups, group size of 8 or 24 (p=0.000 in both cases).

Table 17: Determinants of Making Contributions: Random Effect Tobit Results

	Dependent Variable: Contribution		
Independent Variables	Group Size of 2	Group Size of 8	Group Size of 24
Punishment	1.14	16.54***	10.80***
	(0.975)	(1.058)	(0.43)
Constant	10.18***	9.20***	8.07***
	(3.253)	(1.940)	(0.887)
N	24	48	96
Wald Chi²(1)	1.37	244.60***	641.11***
Log Likelihood	- 914.35	-1774.60	-4415.30

Note: Standard errors are in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% respectively. The regression is a Tobit with both upper and lower censoring and individual random effects.

Result 7: Efficiency is significantly increased by punishment when group size is large.

The ultimate goal of introducing punishment into public goods games is not just to raise contribution levels, but also to increase social surplus. If people make great contributions to the public good but abuse punishment to decrease each other's earnings in the second stage, the social efficiency is still low. Information in Table 18 displays how punishment affects efficiency with different group sizes respectively. The dependent variable is a subject's period payoffs. Punishment is the independent variable which is a dummy variable. When group size is 2, there is a negative relationship between punishment and efficiency. Punishment decreases the period payoffs by 1.54 tokens. When the group size is large, 8 or 24, this relationship becomes positive. Punishment increase the period payoffs by 4.41 and 12.47, respectively, which presents that punishment increase efficiency.

Table 18: Period Payoffs and Punishment: Random Effect Tobit Results

	Dependent Variable: Period Payoffs		
Independent Variables	Group Size of 2	Group Size of 8	Group Size of 24
Punishment	-1.54***	4.41***	12.47***
	(0.342)	(0.432)	(0.455)
Constant	21.94***	28.98***	42.10***
	(0.510)	(0.810)	(1.119)
N	24	48	96
Wald Chi²(3)	20.21***	104.50***	751.23***
Log Likelihood	-1331.07	-3247.06	-7277.64

Note: Standard errors are in parentheses. *, ** and *** denote significance at the 10%, 5% and 1% respectively. The regression is a Tobit with both upper and lower censoring and individual random effects.

3.4 Discussion and Conclusion

In this paper we tackle an important issue how group size and punishment interact. We examine this question from different aspects: 1) How does punishment influence contributions with different group sizes; 2) What are the determinants affect the usage of punishment with different group sizes; 3) What impacts does punishment have on social efficiency with different group sizes. We find that: 1) With a large group size, punishment significantly raises contribution levels; 2) Punishment is used more effectively when the group size is large; 3) Punishment can boost social efficiency with a large group size.

We conclude that the effectiveness of punishment is greatly affected by group size in public goods games. Punishment has no effect on contribution levels when the group size small. However, when the group size is large, the provision of the public goods is dramatically raised by the introduction of punishment, and the cooperation can also be well sustained.

We attribute this phenomena to two reasons: 1) the revenge punishment, which is an inefficient punishment, is used more often when the group size is 2, since subjects who receive punishment would know the punishment points are from the other group member. In the next round, people who received punishment points assign punishment points to their group members even if their contributions a smaller than their group members' contributions, just to revenge; 2) Although, on average, each person can receive up to 3 punishment points no matter what the group size is, when group size of 8 or 24, it's possible a person could receive up to 21 or 69 punishment points respective. Feeling the pressure of earn nothing is they make low contributions, subjects in large groups would rather make high contributions.

4. COMPETE OR COOPERATE? RESOURCE ALLOCATION IN THE NON-CONSTANT-SUM COLONEL BLOTTO GAME

4.1 Introduction

A laboratory experiment is used to investigate the non-constant Colonel Blotto game with symmetric resources. In this game, two players are asked to simultaneously allocate any portion of their identical endowments across several prizes. Each prize has an equal symmetric value for both players. Players' earnings consist of the prizes they win and their remaining endowments. The player who allocates the most resources to a prize wins it with certainty. I check on subjects' behavior under both repeated and random matching protocols.

I compare the experimental results with theoretical predictions of the Nash Equilibria proposed by Roberson and Kvasov (2012). It turns out that although most bids fall within the predicted boundaries, people significantly underbid compared to the Nash Equilibrium. I also find that bidders collude under both matching protocols.

The Colonel Blotto game is originally introduced by Borel in 1921. It is a fundamental model for multidimensional strategic resource allocation and later has been widely applied to many fields from warfare, terrorism, political election, to markets. In this standard version of the Colonel Blotto game two players receive the same amount of endowments and need to allocate these endowments across three battlefields. Each battlefield is won by the player who allocates a greater amount of resources to it. In other words, "auction" contest success function is used to decide who wins the prize. Since in this game the payoff for each player equals to the number of battlefields he or she wins, both players must spend all their resources to maximize the expected number of battlefields they win. As the property of the endowment is “use it or lose it,” it is called “constant-sum” Colonel Blotto game.

Throughout other theoretical literature, Friedman (1958) uses the “lottery” contest success function, in which the probability of winning a battlefield equals the ratio of a player’s resource allocation to the total resources allocated to that battlefield. He also investigates the Colonel Blotto game giving players asymmetric endowments and providing a strategic solution. Roberson (2006) finds the existence of unique equilibrium payoffs for the Colonel Blotto game using the “auction” contest success function. Players can be endowed with either symmetric or asymmetric amount of resources.

Chowdhury, Kovenock and Sheremeta (2013) experimentally examine the Colonel Blotto game with players receiving asymmetric resources. They argue that in the “lottery” contest success function treatment, players divided their resources equally across all battlefields. On the other hand, in the “auction” contest success function treatment, where the player with the largest force in a particular battle wins that battlefield with certainty, disadvantaged players (with less endowment) stochastically allocate zero resources to a subset of battlefields.

Cinar and Goksel (2012) research different Colonel Blotto Games giving players either symmetric resources or asymmetric resources. The number of battlefields is 6 and 10 respectively. In each treatment, subjects play the game for 12 periods against a computer program which has been coded to play the optimal strategy predicted by the theory. They confirm that players are more likely to win if they have more resources than their counterparts.

The literature above concerns constant-sum Colonel Blotto games, in which players' resources have no values for themselves and their earnings depend on how may battlefields they win. However, over the years, different variants of Colonel-Blotto-like games have been developed to address different problems. Kvasov (2007) introduces a non-constant-sum version of the Colonel Blotto game, in which unused resources are valuable for players and player also try to maximize the total value of objects. Games incorporating this insight have been defined as "non-constant-sum" contests. That article characterizes that mixed-strategy equilibria in the case of identical values and symmetric budgets and analyzes its connections with the standard version of the Colonel Blotto game. Roberson and Kvasov (2012) extend the analysis of the non-constant-sum version of the Colonel Blotto game to a more general case which allows players to receive asymmetric budgets. They derive the equilibrium for the non-constant-sum Colonel Blotto game and conclude that if the level of asymmetry is below a threshold, there exists a one-to-one mapping from the unique set of equilibrium univariate marginal distribution functions in the constant-sum game to those in the non-constant-sum game. However, this relationship can be broken down if players' budgets exceed this threshold. The authors also characterize the unique equilibrium, the total expected expenditure, and the unique equilibrium payoff.

There has been some experimental literature investigating contest games which are similar to the non-constant-sum version of the Colonel Blotto game. Mago and Sheremeta (2012) examine subjects' behavior in simultaneous non-constant-sum three-battlefield contest. In their game the payer expending the highest effort in a battlefield wins that battlefield and the player who wins two or three battlefields wins the contest. They discover that, contrary to theoretical predictions, most of the time subjects make positive bids in each battle, these bids fall within the predicted boundaries and they significantly overbid.

Irfanoglu et al. (2011) modify Mago and Sheremeta's game by using the lottery contest success function. Contrary to the theoretical prediction, most subjects vary their allocation between different battlefields in a specific period and within battlefields over time. Mago, Sheremeta and Yates (2013) design a non-constant-sum multi-battlefields experiment, in which two players simultaneously divide effort across three battlefields using the auction contest success function mechanism. They find over-expenditure of resources relative to the Nash Equilibrium benchmark. They speculate this result might come from the inexperienced subjects, a non-monetary utility of winning, or the possible impact of sunk costs in the preceding period.

There are a number of experimental studies on different non-constant-sum Colonel-Blotto-like contests. However, our paper focuses on the standard version of the non-constant-sum Colonel Blotto game, in which two players allocate resources across several prizes. A prize is won by the bidder offering the largest resources. The goal of a subject is to spend the least resources to win the most prizes. Furthermore, I explore the game under different matching protocols, and with different numbers of prizes. I find there is no big difference under the repeated matching protocol and random matching protocol. Subjects under the repeated matching protocol always underbid compared to the expected Nash Equilibrium. However, under the random protocol, subjects underbid only when there are 8 prizes and they bid around the expected Nash Equilibrium when there are either 3 or 5 prizes.

The remaining of this section is organized as follows. Section 4.2 describes the model and the theoretical solutions of the non-constant-sum Colonel Blotto game. In section 4.3, I detail the experimental design and procedure, and also raise theoretical predictions specifically for the setup of our game. Section 4.4 represents the results. A concluding section in the end summarizes.

4.2 Theoretical Background

Following Kvasov (2007), I assume a game in which two players, A and B, each have an equal, fixed amount of budget, X_i for $i = A, B$. Let $X_A = X_B$. Two players simultaneously compete in a series of battlefields, each of which has the same commonly known value v. The total number of battlefields is a finite $n \geq 3$. I use x_{ij} to denote player i's bid on the jth prize, where $j = 1, 2, \ldots, n$ and x_{ij} is nonnegative. Each battlefield can be treated as an independent all-pay auction. For the jth battlefield, the player who makes the highest bid wins its prize v with certainty. Otherwise, the player loses. So the net payoff of player i for a bid of x_{ij} is equal to the value of the battlefield minus the bid he/she has spent:

$$\pi_{ij}(x_{ij}, x_{-ij}) = \begin{cases} v - x_{ij} & if\ x_{ij} > x_{-ij} \\ -x_{-ij} & if\ x_{ij} < x_{-ij} \end{cases}$$

where if both player, i and $-i$ (player i's counterpart), offer the same bid to a battlefield, each player wins the battlefield with equal probability $\frac{1}{2}$.

For each player i, the budget constraint can be denoted as:

$$\sum_{j=1}^{n} x_{ij} \leq X_i$$

The solution to the non-constant-sum Colonel Blotto game described above has been derived by Roberson and Kvasov (2012). Although the class of multi-battlefield contest games has no pure strategy equilibria, the authors introduce the modified budgets for the game with three or more battlefields. The modified budgets are the equilibrium total expected expenditures which are unique.

If $X_A, X_B, v, and\ n \geq 3$ satisfy $\left(\frac{2}{n}\right)\min\{v, X_B\} < X_A \leq X_B$, the unique equilibrium total expected expenditure for player A is $M_{X_A}(X_A, X_B) = \min\{X_A, \left(\frac{nv}{2}\right)\}$ and the unique equilibrium total expected expenditure for player B is $M_{X_B}(X_A, X_B) = \min\{X_B, \left(\frac{nv}{2}\right), \left(nv\frac{X_A}{2}\right)^{\frac{1}{2}}\}$. The unique equilibrium expected payoff for player A is $\frac{nvM_{X_A}}{2M_{X_B}} - M_{X_A}$, and the unique equilibrium expected payoff for player B is $nv(1 - \frac{M_{X_A}}{2M_{X_B}}) - M_{X_B}$. Mixed strategy equilibrium on each battlefield for each player follows a marginal distribution function of resources. Player A's unique set of univariate marginal distribution functions is:

$$\forall j \in \{1, 2, \dots, n\}, \qquad F_{Aj}(x_j) = \left(1 - \frac{M_{X_A}}{M_{X_B}}\right) + \frac{x_j}{\left(\frac{2}{n}\right) M_{X_B}} \left(\frac{M_{X_A}}{M_{X_B}}\right) \qquad for \quad x_j \in [0, \frac{2}{n} M_{X_B}]$$

Player B's unique set of univariate marginal distribution functions is:

$$\forall j \in \{1, 2, \dots, n\}, \qquad F_{Bj}(x_j) = \frac{x_j}{\left(\frac{2}{n}\right) M_{X_B}} \qquad for \quad x_j \in [0, \frac{2}{n} M_{X_B}]$$

A more detailed description of the Nash equilibrium solution of our experimental game is provided in Section 4.3.2.

4.3 Experimental Environment

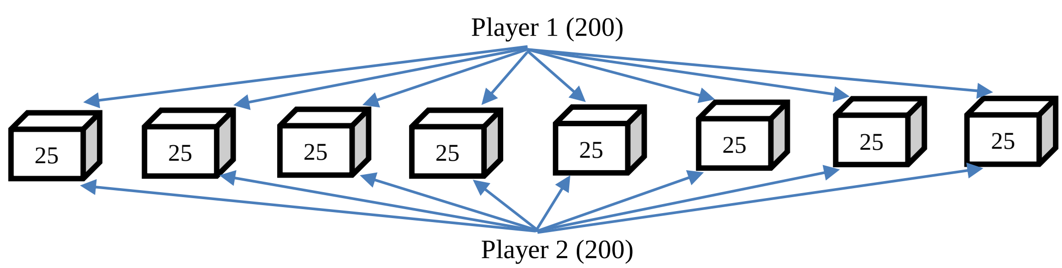
4.3.1 Experimental Design

This experimental design consists of six treatments in which matching protocols differ, and under each matching protocol, the number of prizes is varied from 8, to 5, to 3. Three components are included in each experimental session. A risk-preference elicitation task is conducted at the beginning of the experiment. According to Eckel and Grossman (2008), each subject is asked to make a choice among six 50/50 lotteries. The second part is the non-constant-sum Colonel Blotto game. Subjects play the game either with same counterparts or different counterparts for each period. I ask subjects to answers some survey questions at the end.

In the non-constant-sum Colonel Blotto game, subjects are placed in pairs and receive 200 tokens as endowments. Both players allocate their tokens across 8 prizes. Subjects may allocate any number of tokens between 0 and 200 to any prize. The total number of tokens they spend on prizes must be less than or equal to 200. Tokens not spent count towards their earnings. After all subjects in the session submit their allocation decisions for a period, the computer displays which prizes they have won.

Each prize is worth 25 tokens to the winner and 0 tokens to the loser. The winner of each prize is chosen by using the “auction” contest success function, in which the subject, who offers a higher bid on a prize than his/her counterpart, wins the particular prize with certainty. If both players allocate the same amount of tokens to a prize, the computer randomly chooses one of them as the winner. Figure 14 below displays the sample structure of the game for the 8-prize case as an example.

Figure 14: Sample Structure of the Experimental Design Illustration for 8 Prizes



After all subjects make their decisions, results for that period are revealed to the subjects. Subjects are informed how their counterparts allocated the 200 tokens across the 8 prizes, how many prizes they won, their own period earnings and cumulated earnings. Subjects play the game repeatedly for 15 periods.

In the treatments with 5 prizes, the only difference is that the endowment for each subject is reduced to 125. In the treatments with 3 prizes, each subjects only receives 75 tokens as the endowment. This ensures each subject’s budget always equals the total value of the prizes.

There are two treatment variables: matching protocol and the number of prizes. Matching protocol enables us to implement either a repeated matching treatment or a random matching treatment. Subjects are matched with the same counterparts for the entirety of the session of the repeated matching treatment. However, in each session of the random matching treatment, subjects are matched with different counterparts for each period. Number of prizes varies from 8, to 5 to 3 (see Table 19).

Table 19: Six Treatments

	8 Prizes (Endowment: 200 tokens 200 tokens = $1)	**5 Prizes** (Endowment: 125 tokens 125 tokens = $1)	**3 Prizes** (Endowment: 75 tokens 75 tokens = $1)
Repeated-matching	Treatment 1	Treatment 3	Treatment 5
Stranger-matching	Treatment 2	Treatment 4	Treatment 6

4.3.2 Theoretical Hypotheses

I have discussed the general equilibrium solution for the non-constant-sum Colonel Blotto game Section 4.2. Here I apply this solution to propose hypotheses for our present experimental design. In our game, both players receive symmetric endowments of 200 tokens, a prize is worth 25 tokens, and there are 8 prizes to distribute resources to. The mixed Nash equilibria require each player to distribute the resources following the univariate marginal distribution below.

$$\forall j \in \{1, 2, \dots, n\}, \qquad F_{ij}(x_j) = \frac{x_j}{25} \qquad for \quad x_j \in [0, 25]$$

Each player's equilibrium total expected expenditure is 100 tokens. The net payoffs of using the 100 tokens, in equilibrium, is 0 tokens. So for each player, his/her expected period earnings are 200 tokens.

Following the same procedure, I can get that each player's equilibrium total expected expenditure is 62.5 when the number of prizes is 5. Individual expected period earnings are 125 tokens. When there are 3 prized, each player's equilibrium total expected expenditure is 37.5 and the expected period earnings are 75.

4.3.3 Experimental Procedure

Three sessions of the repeated matching treatment with 8 prizes are conducted at the Center of Behavioral and Experimental Economic Science at University of Texas at Dallas during July and August of 2012. The rest of the sessions are conducted at the Economic Research Lab at Texas A&M University in 2014 and 2015. This is a computerized experiment, programmed using z-Tree (Fischbacher, 2007). A total of 216 subjects participated in 18 sessions (12 subjects in each session). Our subjects include both undergraduate students and graduate students who have never participated in this study before.

Instructions are paper-based and were read aloud by our experimenter. [7] In the first part of each session, I completed the risk-preference task. However, the result of this task was not disclosed until the end of the experiment. In the second part, subjects Ire informed that there would be 15 decision-making periods. I also collected subjects' demographic information through a questionnaire at the end of each session.

Only 1 of 12 subjects was randomly selected and paid for his/her decision making in the risk-preference elicitation task. All subjects Ire paid for all the 15 periods in the second part. And the earnings from this part needed to be converted into US dollars at the end of the experiment. The exchange rate was 200 tokens = $1 when the number of prizes is 8, 125 tokens to $1 when the number of prizes is 5 and 75 tokens = $1 when the number of prizes is 3. In addition to what they earned in the experiment, participants Ire also paid a $5 show-up fee. On average, subjects earned $21.75 each, which was paid in cash, in private. Each experimental session took around 80 minutes.

4.4 Results

4.4.1 Average Bids Compared to the Nash Equilibrium Benchmarks

There are 3240 observations from 216 subjects in total. Each subject participated in only one of the six sessions. Table 20, 21 and 22 summarize the average bid of each prize for each matching protocol treatment with 8, 5 and 3 prizes respectively.

[7] The instructions are included in Appendix C.

Table 20: Average Bid per Prize vs. Nash Equilibrium Bid per Prize – 8 Prizes

		Prize 1	Prize 2	Prize 3	Prize 4	Prize 5	Prize 6	Prize 7	Prize 8	Total
Nash Equilibrium Bid		12.5	12.5	12.5	12.5	12.5	12.5	12.5	12.5	100
Repeated Matching	Mean Bid (StDev)	8.76 (5.23)	9.79 (6.04)	9.67 (5.95)	9.36 (5.35)	8.94 (5.68)	9.30 (5.62)	8.71 (5.49)	8.85 (5.46)	73.37 (41.75)
	t-test	-4.29	-2.70	-2.86	-3.52	-3.77	-3.42	-4.14	-4.01	-3.83
Random Matching	Mean Bid (StDev)	10.71 (8.60)	8.37 (6.11)	8.82 (7.52)	7.92 (5.57)	8.08 (5.49)	8.61 (5.62)	7.46 (4.93)	7.95 (4.94)	67.93 (40.88)
	t-test	-1.25	-4.05	-2.94	-4.94	-4.82	-4.15	-6.13	-5.52	-4.70

Table 21: Average Bid per Prize vs. Nash Equilibrium Bid per Prize – 5 Prizes

		Prize 1	Prize 2	Prize 3	Prize 4	Prize 5	Total
Nash Equilibrium Bid		12.5	12.5	12.5	12.5	12.5	62.5
Repeated Matching	Mean Bid (StDev)	10.18 (5.66)	8.90 (3.69)	9.10 (3.78)	8.66 (3.39)	8.77 (3.02)	45.62 (17.80)
	t-test	- 2.46	- 5.85	- 5.39	- 6.78	- 7.42	- 5.69
Random Matching	Mean Bid (StDev)	11.81 (7.20)	11.51 (6.19)	11.43 (6.32)	11.04 (5.74)	11.14 (6.49)	56.94 (30.21)
	t-test	- 0.57	- 0.96	- 1.01	- 1.51	- 1.26	- 1.10

Table 22: Average Bid per Prize vs. Nash Equilibrium Bid per Prize – 3 Prizes

		Prize 1	Prize 2	Prize 3	Total
Nash Equilibrium Bid		12.5	12.5	12.5	37.5
Repeated Matching	Mean Bid (StDev)	10.30 (4.57)	10.10 (4.60)	9.53 (4.10)	29.94 (12.86)
	t-test	- 2.89	- 3.12	- 4.35	- 3.53
Random Matching	Mean Bid (StDev)	11.02 (4.25)	10.98 (4.30)	12.15 (4.24)	34.15 (10.94)
	t-test	- 2.08	- 2.11	- 0.49	- 1.83

Each table above presents the average bid for each of the prizes and the total bid across all prizes under both repeated matching protocol and random matching protocol. Each table also provides t values which indicate the differences between the mean bids and expected Nash equilibrium bids.

Result 1: On average, subjects always underbid in the repeated matching treatments regardless of the number of prizes.

Theoretical equilibrium strategies predict that each player should allocate a random number, between 0 and 25, to each prize, and the total number of token spent across 8, 5 and 3 prizes should be summed up to 100, 62.5, and 37.5 respectively. I would expect each player to allocate 12.5 tokens to each prize on average. However, I find average bids are less 12.5. Players significantly underbid compared to the expected allocation of Nash equilibrium for each prize.

I attribute this underbidding phenomenon to the collusion between the two players. The nature of the repeated non-constant sum Colonel Blotto game offers subjects with incentives to collude. A common solution to eliminate the collusion is to use random matching protocol instead of using repeated matching protocol. However, I find collusion happened under both protocols. Most subjects choose to use the minimal-bid strategy (Kwasnica and Sherstyuk, 2007), in which they allocate low prices across prizes. I impute subjects' collusive behavior to the complex and less competitive bidding environment. First, bidders are easily able to collude by perceiving counterparts' collusive signals when the number of players in the auction market is small. Van Huyck et al. (1990) conclude that groups with just two players can tacitly coordinate more easily than can groups with more players. Kwasnica and Sherstyuk (2007) find that coordination on payoff-superior collusive outcomes can always be achieved in ascending multiple-prize auctions, as long as there are only two bidders in the market. Second, our game is a multiple-prize auction. Subjects can simply split the market by tacitly colluding. Phillips et al (2003) and Kwasnica and Sherstyuk (2013) both find that quantity for sale is a key collusion-facilitating feature in the auction. Multiple prizes generate some strategic uncertainties, especially when there are more prizes, which may persuade subjects not to bid a large amount of tokens on each prize. Third, subjects participate in the game repeatedly for 15 periods. It is easy for experienced players to aware that they can increase their expected payoff if they coordinate on collusive bids.

4.4.2 Repeated Matching Protocol vs. Random Matching Protocol

4.4.2.1 Aggregate-level Results

The experiment is conducted by using both repeated matching protocol and random matching protocol to test whether there are any differences either on the level of total bids or distributions of bids between these two matching protocols. Table 23, 24 and 25 below summarize statistics of average individual bids over all prizes and periods for 8-prize, 5-prizes and 3-prize cases respectively.

Table 23: Descriptive Statistics: Overall Average Bids – 8 Prizes

Treatment	Number of Subjects	Average Bid (StDev)	Minimum Bid	Maximum Bid
Repeated Matching	36	9.17 (5.22)	0	50
Random Matching	36	8.49 (5.11)	0	60

Table 24: Descriptive Statistics: Overall Average Bids – 5 Prizes

Treatment	Number of Subjects	Average Bid (StDev)	Minimum Bid	Maximum Bid
Repeated Matching	36	9.12 (3.56)	0	55
Random Matching	36	11.39 (6.04)	0	125

Table 25: Descriptive Statistics: Overall Average Bids – 3 Prizes

Treatment	Number of Subjects	Average Bid (StDev)	Minimum Bid	Maximum Bid
Repeated Matching	36	9.98 (4.29)	0	45
Random Matching	36	11.38 (3.65)	0	35

Result 2: At the aggregate level, the difference between the repeated matching protocol and random matching is insignificant.

Table 23 shows that when subjects bid across 8 prizes, the average bid under the random matching protocol is even smaller than under the repeated matching protocol, which is in contradiction of previous experimental studies arguing that bids in the random matching treatment are usually slightly higher than in the repeated matching treatment in contests (Lugovskyy et al, 2010; Vandegrift and Yavas, 2010; Baik et al, 2013). Table 24 and 25 present that the average bids are a little higher when subjects need to bid across 5 prizes and 3 prizes. However, none of these differences between average bids of different protocols in our game is statistically significant.

To get an intuitive grasp of how frequently subjects use specific allotments, Figure 15, 16 and 17 display the distribution of pooled bids under each number of prizes condition. I find most bids still fall between the predicted boundaries, through 0 to 25. Subjects use 0-token bid more frequently in the repeated matching treatment than in the random matching treatment, which implies more attempts to achieve maximum payoff-superior collusive outcomes in the repeated matching treatment. Nevertheless, there are more bids exceeding the expected Nash Equilibria in the repeated matching treatment, which leads to a higher average bid compared to the random matching treatment.

Figure 15: Distribution of Pooled Bids – 8 Prizes

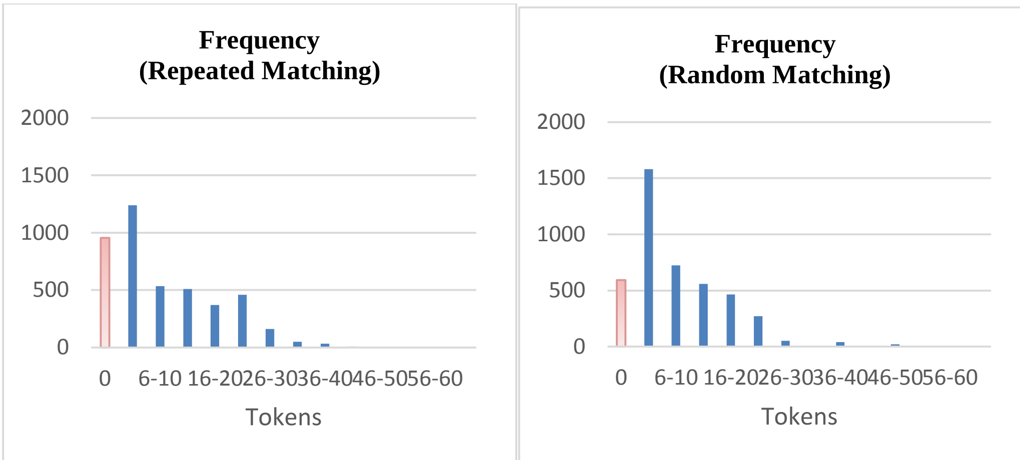


Figure 16: Distribution of Pooled Bids – 5 Prizes

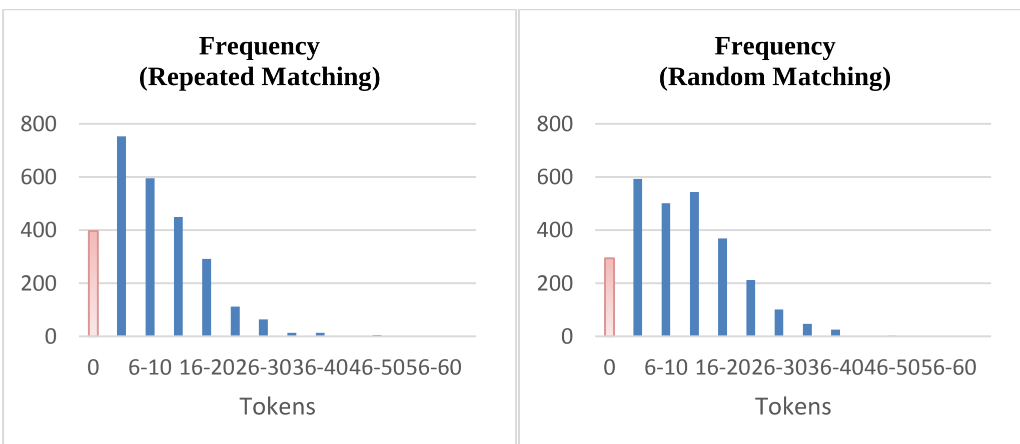
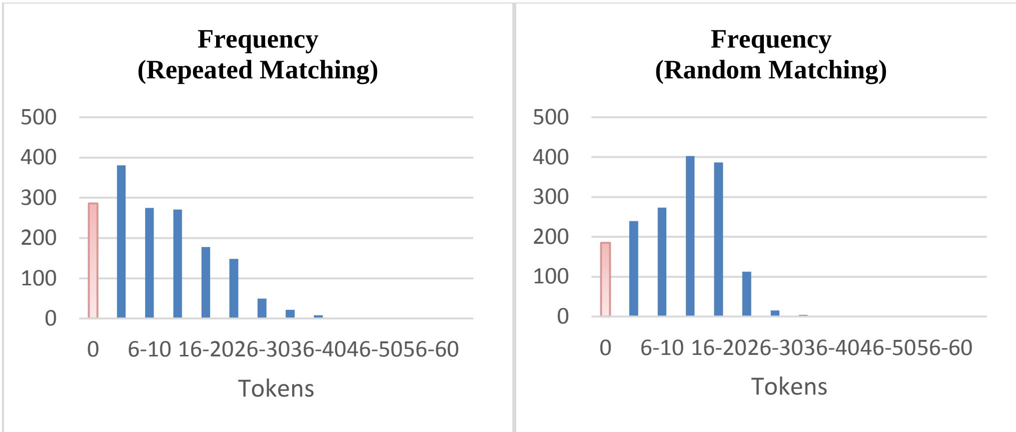


Figure 17: Distribution of Pooled Bids – 3 Prizes



4.4.2.2 Individual-level Results

Section 4.4.2.1 suggests that there are no significant differences across the repeated matching protocol and random matching protocol. In this section, I will focus on individual behavior. I run several random effect panel regressions of individual-level bids to check how it would be affected by control variables.

I choose bid_{itj} as the dependent variable, where bid_{itj} is player i's allocation of tokens to the jth prize in the tth period. Model 1 includes one dummy, stranger treatment, as the independent variable. More control variables are added to model 2. "Lag of own bid" denotes player i's allocation to the same prize in the previous period, "previous period win" is a dummy variable denoting whether player i won that specific prize in the previous period, and "lag of counterpart's bid" denotes the counterpart's distribution to that prize in the previous period. I also control for several demographic variables such as gender and risk preference. To be specific, both gender and risk preference are dummies, where “female” equals 1 if this subject is a female; “risk-seeking” equals 1 indicating that the subject has chosen option 4, 5, or 6 in the risk-preference elicitation task. Model 1 emphasizes the treatment effect that includes all the observations. Data are also divided for repeated-matching treatment and random-matching treatment separately to compare how these control variables affect individual decision-making within each treatment.

Result 3: At the individual level, there is no significant difference between repeated matching protocol and random matching protocol.

In Table 26, 27 and 28 below, the “stranger” dummy capturing the treatment effect turns out to be insignificant in either model 1 or model 2, although some control variables are significant in model 2. This suggests that there is no significant difference on the quantitative level of bids in the repeated matching or random matching protocol.

Table 26: Regressions on Determinants of Allocation to a Prize – 8 Prizes

Dependent Variable: Individual Bid on One Prize	Model 1 Overall	Model 2 Overall	Model 3 Repeated	Model 4 Random
Stranger	-0.681	-0.385		
	(1.21)	(0.62)		
Lag of Own Bid		0.485***	0.332***	0.612***
		(0.05)	(0.04)	(0.08)
Lag of Win Indicator		-0.652	0.270	-1.265*
		(0.51)	(0.58)	(0.69)
Lag of Counterpart's Bid		0.108***	0.241***	0.002
		(0.03)	(0.04)	(0.03)
Period		0.081**	0.088	0.072*
		(0.04)	(0.08)	(0.04)
Risk-seeking		0.820	1.685**	0.152
		(0.61)	(0.69)	(0.77)
Constant	9.171***	3.121***	2.449**	3.374***
	(0.86)	(0.71)	(1.19)	(0.76)
observations	8,640	8,064	4,032	4,032
number of subjects	72	72	36	36
R-sq	0.001	0.260	0.227	0.351

Where * and ** indicate significance at 5% and 1% levels. I use OLS regressions. Standard errors, provided in parentheses, are clustered at individual level for Model 1, 2 and 4, and at both individual level and group level for Model 3.

Table 27: Regressions on Determinants of Allocation to a Prize – 5 Prizes

Dependent Variable: Individual Bid on One Prize	Model 1 Overall	Model 2 Overall	Model 3 Repeated	Model 4 Random
Stranger	2.264	1.192		
	(1.16)	(0.73)		
Lag of Own Bid		0.395**	0.269**	0.438**
		(0.04)	(0.04)	(0.05)
Lag of Win Indicator		-0.372	-0.426	0.411
		(0.42)	(0.47)	(0.80)
Lag of Counterpart's Bid		0.064*	0.146**	0.051
		(0.03)	(0.04)	(0.04)
Period		0.035	0.006	0.059
		(0.05)	(0.09)	(0.06)
Risk-seeking		-0.186	-1.280*	0.560
		(0.72)	(0.60)	(1.11)
Constant	9.123**	5.027**	6.216**	4.898**
	(0.59)	(0.79)	(1.15)	(1.20)
Observations	5,400	5,040	2,520	2,520
Number of Subjects	72	72	36	36
R-sq	0.016	0.186	0.133	0.229

Where * and ** indicate significance at 5% and 1% levels. I use OLS regressions. Standard errors, provided in parentheses, are clustered at individual level for Model 1, 2 and 4, and at both individual level and group level for Model 3.

Table 28: Regressions on Determinants of Allocation to a Prize – 3 Prizes

Dependent Variable: Individual Bid on One Prize	Model 1 Overall	Model 2 Overall	Model 3 Repeated	Model 4 Random
Stranger	1.406	0.925		
	(0.93)	(0.65)		
Lag of Own Bid		0.336**	0.332**	0.335**
		(0.05)	(0.09)	(0.06)
Lag of Win Indicator		-0.156	-1.716	1.250
		(0.70)	(0.96)	(0.71)
Lag of Counterpart's Bid		0.045	0.024	0.073
		(0.05)	(0.08)	(0.04)
Period		0.060	0.008	0.107
		(0.05)	(0.08)	(0.07)
Risk-seeking		0.497	-1.764	0.456
		(0.63)	(1.13)	(0.75)
Constant	9.171**	6.147**	8.352**	5.160***
	(0.86)	(0.89)	(1.30)	(0.97)
Observations	3,240	3,024	1,512	1,512
Number of Subjects	72	72	36	36
R-sq	0.007	0.123	0.104	0.164

Where * and ** indicate significance at 5% and 1% levels. I use OLS regressions. Standard errors, provided in parentheses, are clustered at individual level for Model 1, 2 and 4, and at both individual level and group level for Model 3.

Result 4: Subjects in the repeated matching treatment use different strategies to make their decisions from those in the random matching treatment.

The "lag of own bid" coefficient is positive and significant in both the model 3 and model 4 in Table 26, 27 and 28, which implies that there is a strong serial correlation in each treatment. Subjects are inclined to bid more than in the previous period. Moreover, not only will the allotment of resources in the previous period affect a subject allocation decision for the current period, but the result from the previous period will also influence it. So I create an interaction term combining these two factors together to capture the joint effect. Although this interaction term in significant in both model 3 and

4, it imposes a positive effect on the bid under repeated matching protocol and a negative effect on the bid in the random matching treatment. This is an interesting finding. In the repeated matching treatment, a subject is always paired with the same counterpart. If a subject won a specific prize in the previous period because of a lager bid than his/her counterpart, this subject should have expected that the counterpart would spend more to win the prize in this current period. So he/she is motivated by winning the prize in the previous period and will allocate more resources to that prize to win it again. On the other hand, in the random matching treatment, a subject plays the game with a different counterpart in each period. Winning a prize in the previous period could possibly make the subject realize that it was actually not necessary to spend that amount of tokens, taking the counterpart's bid as reference. So this subject may reduce his/her bid in the current period. The counterpart's bid in the previous period can only positively and significantly affect a subject's decision making for the current period under the repeated matching protocol, whereas it is insignificant in the random matching treatment.

This result may be due to the fact that if a player repeatedly plays the game with the same counterpart, he/she would try to figure out the pattern of the counterpart's strategy when makes his/her own decision. A player allocates more resources to the prize in the current period than his/her counterpart's allocation in the last period. Yet, if a player meets a new counterpart every period, it's no longer necessary to take the last counterpart's decision making into account.

4.4.3 8 Prizes vs. 5 Prizes vs. 3 Prizes

Model 1 and 2 in Table 29 includes the data from all the sessions. No significant effect is found in either the matching protocol or number of prizes. In order to isolate the effect of how the number of prizes affects individual bids, the data are separated into two sets, under repeated matching condition and random matching condition. Model 5 and 6 show that the bid is significantly decreased by more than 2 tokens under the random matching protocol when there are 8 prizes.

Table 29: Regressions on Determinants of Individual Allocations to a Prize

Dependent Variable: Individual Bid on One Prize	Model 1 Overall	Model 2 Overall	Model 3 Repeated	Model 4 Repeated	Model 5 Random	Model 6 Random
Stranger	0.631	2.264*				
	(0.73)	(1.15)				
8 Prizes	-1.424	0.048	0.048	-0.952	-2.897*	-2.522*
	(0.84)	(1.04)	(1.39)	(1.02)	(1.31)	(1.16)
3 Prizes	0.426	0.855	0.855	0.241	-0.003	0.778
	(0.75)	(0.92)	(1.08)	(1.04)	(1.16)	(1.19)
Stranger*8 Prizes		-2.945				
		(1.67)				
Stranger*3 Prizes		-0.858				
		(1.48)				
Lag of Own Bid				0.271**		0.461**
				(0.04)		(0.05)
Lag of Win indicator				-0.319		-0.370
				(0.42)		(0.47)
Lag of Counterpart's Bid				0.176**		0.026
				(0.03)		(0.02)
Lag of Own Bid*8 Prizes				0.101		0.125
				(0.06)		(0.09)
Lag of Own Bid*3 Prizes				0.021		-0.060
				(0.09)		(0.07)
Period				0.047		0.074*
				(0.05)		(0.03)
Risk-seeking				0.079		0.378
				(0.50)		(0.53)
Constant	9.940**	9.123**	9.123**	4.887**	11.387**	6.067**
	(0.61)	(1.48)	(0.78)	(0.95)	(1.00)	(0.97)
Observations	17,280	17,280	8,640	8,064	8,640	8,064
Number of Subjects	216	216	108	108	108	108
R-sq	0.009	0.015	0.001	0.170	0.027	0.300

Where * and ** indicate significance at 5% and 1% levels. I use OLS regressions. Standard errors, provided in parentheses, are clustered at individual level for Model 1, 2, 5 and 6, and at both individual level and group level for Model 3 and 4.

4.5 Conclusion

This section investigates individual behavior in the non-constant-sum Colonel Blotto Game, where both leftover endowments and prizes are valuable for subjects. I ran this experiment using both repeated matching protocol and random matching protocol. I also vary the number of prizes. I find that most bids fall within the predicted boundaries provided by Roberson and Kvasov (2012). However, our data shows under the repeated matching protocol, subjects significantly underbid compared to theoretical expected equilibrium, which also contradicts previous experimental studies exhibiting individual overdissipation in either single-prize or multiple-prize all-pay auction contests. I suggest that bidder collusion explains this underbidding phenomenon.

I also compare and contrast two different matching protocols. I find that there is no significant difference in terms of the quantitative level and distribution of bids between repeated matching protocol and random matching protocol. However, the effects of decision-making strategies and demographic information are diverse under different matching protocols. I find that people bear both their own and their counterparts' previous strategies in mind when they make decisions in the repeated matching treatment. However, people rely only on their own previous strategies in the random matching treatment.

Varying the number of prizes has no effect on subjects' underbidding strategies under repeated matching protocol. However, subjects only underbid when they face a large number prizes, not when they face a small number of prizes. Instead, they just bid around the expected Nash Equilibrium.

5. SUMMARY

The experimental approach has been well recognized as an effective and efficient tool that provides us with better understanding when studying public policy. The three experiments that compromise this dissertation take different areas of public policy in account: health policy, social policy and economic policy. Experiments are carefully designed to capture the features of the real world. To examine the effects of different policies, changes in subjects' behavior are observed and analyzed as incentives or the institutions change.

For health policy, I investigate the extent to which two incentive treatments, a waiver of blood transfusion fees and priority access to blood supplies, are effective in increasing donations. I find that contributions increase the most when subjects are exposed to a combination of waiver and priority incentives. Both the waiver treatment and the priority treatment individually increase contributions; the waiver treatment, however, is more successful at increasing donations than the priority treatment.

To study social welfare, a public goods game is adopted to explore how group size influences the effectiveness of a punishment institution. In this experiment, there are three varying group sizes: 2, 8 and 24. Subjects play a standard public goods game without punishment for the first 10 periods followed by another 10 periods of a public goods game with a punishment institution that allows subjects to punish others group members. Results show that in groups of 2, the punishment institution has no effect on contribution levels. When the group size increases, however, the provision of public goods is raised dramatically by the introduction of punishment, and cooperation is sustained until the last round.

Economic policy is simulated in the non-constant-sum Colonel Blotto game, in which two players simultaneously allocate their endowments across several prizes. This game mimics the investment decisions of companies which compete in markets. Each player's objective is to maximize earnings by using a minimum amount of resources to win more prizes. The prize goes to the person who has allocated a larger amount to the prize. The experiment is conducted using both a repeated matching protocol and a random matching protocol. Under each matching protocol, I run different treatments varying the number of prizes. Experimental results show that although subjects use different strategies to make decisions for different matching protocols, there is no significant difference between the two matching protocols. Participants under the partner matching protocol always bid less than the theoretical Nash Equilibrium. Participants under the stranger matching protocol underbid, but only when they face a large number of prizes. Under repeated interaction, tacit collusion is relatively easy, but with random interaction, having fewer prizes makes the market more competitive and thus makes tacit collusion more difficult.

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APPENDIX A

THE BLOOD DONATION GAME

A.1 Instructions

You are now taking part in an economic experiment. Your earnings will depend on the decisions that you make, the decisions of other people, and chance. You will be paid these earnings privately in cash at the end of the session today. However, during the experiment, we shall not speak of “dollars” but rather of “tokens”. At the end of the experiment, the total amount of tokens will be converted to dollars at the following rate:

40 tokens = 1 dollar

This experiment consists of two games, Game One and Game Two. Each game includes six periods. At the beginning of each game, you will be randomly assigned into a six-person group. Your group composition will remain the same for the entirety of the game (six periods). Each game begins with each subject having a private account, which contains thirty tokens and two units, and each group having an empty group account. At the end of the session today, one of the two games will be randomly selected for payment. The experimenter will roll a die to select the random round.

Game One (Baseline Treatment)

What to do: In each period there are three phases that will involve decision-making. In the first phase, you will decide (if you are eligible, as explained later) whether to donate to the group account. In the second phase, you will play a lottery game, which determines whether you are a “winner” or a “loser”. In the third phase, if you are a loser, you will have a chance to purchase one unit from the group account as explained later. Your earnings depend on how many units you hold at the end of the period. We will now go over the details for each phase.

Donating to the group account (Phase 1): If you have enough units in your private account at the beginning of a period, you are eligible to donate a unit to the group account. At the beginning of each period, if you have two units in your private account, you will be asked to make the choice either to keep both units or to donate one unit from your private account to the group account. If you have fewer than two units, you are not eligible to donate. Later, donated units in group account will be distributed among your group members who lost in the lottery game (see next page). However, you will not know if you are a winner or loser before you make your decision.

Playing the lottery game (Phase 2): In the lottery game, you choose one integer from 1 through 6. Then the computer will randomly generate three lucky numbers, also from 1 through 6. If the number you choose is one of the lucky numbers, you win, and you are able to retain all the remaining units in your private account. If the number you choose is not one of the lucky numbers, you lose, and all the remaining units will be deducted from your private account. In other words, a loser in the lottery game will have zero units in the private account. However, losers might be able to purchase units from the group account.

Example 1: Suppose you chose number 4 and there are two units in your private account. If the computer generates number 1, 2 and 4 as lucky numbers, you are a winner and you can keep everything in your private account.

Example 2: Suppose you chose number 5 and there is one unit in your private account. If the computer generates number 1, 2 and 6 as the lucky numbers, you are a loser and you lose the unit in your private account. Therefore, you have zero units in your private account. (Note that if you are a loser, you will lose all units in your private account.)

Purchasing from the group account (Phase 3): If you are a loser, you will have a chance to purchase one unit from the group account and deposit it to your private account. If the total number of units requested by losers exceeds the number of units in the group account, units in the group account will be allocated randomly among those requesting units. Each unit purchased costs a fee of thirty tokens. (Remember, only those who successfully purchase units will pay the fee.) Winners do not have the opportunity to purchase units from the group account.

Example 1: Suppose there are two losers in your group, and both losers requested to purchase one unit. Suppose there are currently two donated units in the group account. Then each loser will successfully make the purchase, receiving one unit and paying thirty tokens.

Example 2: Suppose there are three losers in your group, and all of them requested to purchase one unit. However, there are just two donated units in the group account. Then these two units will be randomly assigned to two of the three losers respectively. Each loser will have an equal chance (2/3) of getting one unit from the group account. Each of the two losers who successfully makes the purchase pays thirty tokens. The loser who does not make the purchase does not need to pay.

Receiving payoffs: Note that units are valuable for you. After losers purchase units, you can earn tokens from units that you are currently holding in your private account. Numbers of units and their corresponding payoffs are shown in the table below.

Number of Units	2	1	0
Token-payoffs	125	100	0

You will receive payoffs in tokens from the units currently retained in your private account. Your earnings are cumulative. Both earning tokens and units carry over to the next period. (However, units in the group account do not carry over.)

Getting bonus units: Before the beginning of the next period, if the number of units in your private account at the end of a period is fewer than two, one bonus unit will be automatically added to your account. The maximum number of units you can hold is two, so if you already have two units in your current private account, you will not receive the bonus unit.

Example 1: If you lost in the lottery game and did not get to purchase a unit from the group account, you would have zero units at the end of that period. You will receive one bonus unit prior to the start of the next period.

Example 2: If you won in the lottery game and did not donate a unit to the group account, you have two units in your private account. You will not be able to receive one bonus unit. You will still have two units to begin next period.

Game Two (Waiver Treatment)

Compared to Game One, the only difference in Game Two is that each unit successfully purchased is free for lottery losers who were donors, while it costs thirty tokens for lottery losers who were not donors.

Example 1: Suppose there are two losers in your group. One of the loser donated one unit to the group account in Phase 1, but the other one did not. Both losers requested to purchase one unit. Suppose there are currently two donated units in the group account. Then both losers will successfully make the purchase. However, the loser who was not a donor pays thirty tokens for receiving the unit, while the loser who was a donor receives the unit for free. (Remember that if the total number of units requested by losers exceeds the number of units in the group account, units in the group account will be allocated randomly among those requesting units.)

Game Two (Priority Treatment)

Compared to Game One, the only difference in Game Two is that losers who were donors are guaranteed to make purchases if they make requests. The remaining units in the group account will be randomly allocated among requests submitted by losers who were not donors.

Example 1: Suppose there are three losers in your group. One of them donated one unit to the group account, but the other two did not. All the three losers requested to purchase one unit. However, suppose there are just two donated units in the group account. First, the loser who was a donor will be guaranteed to receive one unit form the group account. Then the remaining one unit will be randomly assigned to one of the other two losers who were not donors. Each loser will have an equal chance (1/2) of getting one unit from the group account. Each of the two losers who successfully makes the purchase pays thirty tokens. The loser who does not make the purchase does not need to pay.

Game Two (Waiver & Priority Treatment)

Compare to Game One, there are two differences in Game Two:

1. Losers who were donors are guaranteed to make purchases if they make requests, then the remaining units in the group account will be randomly allocated among requests submitted by losers who were not donors;

2. Each unit successfully purchased is free for lottery losers who were donors, while it costs thirty tokens for lottery losers who were not donors.

Example 1: Suppose there are three losers in your group. One of them donated one unit to the group account in Phase 1, but the other two did not. All of them requested to purchase one unit. However, suppose there are just two donated units in the group account. First, the loser who was a donor will get one unit for free. Then the remaining one unit in the group account will be randomly assigned to one of the other two losers who were not donors. In other words, each loser who was not donor will have an equal chance (1/2) of getting one unit from the group account. The unit received by the loser who was not a donor costs thirty tokens.

A.2 Mixed Strategy for the Priority Treatment

$$\mathrm{p} * (0.5 * 100 + 0.5 * 70) = (1 - \mathrm{p})(0.5 * 125 + 0.5 * ((1 - p)^5 * 0 + C_1^5 * \mathrm{p} * (1 - p)^4 * \left(C_5^5 * 0.5^5 * 70 + C_4^5 * 0.5^5 * \left(\frac{1}{2}\right) * 70 + C_3^5 * 0.5^5 * \left(\frac{1}{3}\right) * 70 + C_2^5 * 0.5^5 * \left(\frac{1}{4}\right) * 70 + C_1^5 * 0.5^5 * \left(\frac{1}{5}\right) * 70 + C_0^5 * 0.5^5 * \left(\frac{1}{6}\right) * 70\right) + C_2^5 * p^2 * (1 - p)^3 * \left(C_5^5 * 0.5^5 * 70 + C_4^5 * 0.5^5 * 70 + C_3^5 * 0.5^5 * \left(\frac{2}{3}\right) * 70 + C_2^5 * 0.5^5 * \left(\frac{2}{4}\right) * 70 + C_1^5 * 0.5^5 * \left(\frac{2}{5}\right) * 70 + C_0^5 * 0.5^5 * \left(\frac{2}{6}\right) * 70\right) + C_3^5 * p^3 * (1 - p)^2 * \left(C_5^5 * 0.5^5 * 70 + C_4^5 * 0.5^5 * 70 + C_3^5 * 0.5^5 * 70 + C_2^5 * 0.5^5 * \left(\frac{3}{4}\right) * 70 + C_1^5 * 0.5^5 * \left(\frac{3}{5}\right) * 70 + C_0^5 * 0.5^5 * \left(\frac{3}{6}\right) * 70\right) + C_4^5 * p^4 * (1 - p)^1 * \left(C_5^5 * 0.5^5 * 70 + C_4^5 * 0.5^5 * 70 + C_3^5 * 0.5^5 * 70 + C_2^5 * 0.5^5 * 70 + C_1^5 * 0.5^5 * \left(\frac{4}{5}\right) * 70 + C_0^5 * 0.5^5 * \left(\frac{4}{6}\right) * 70\right) + C_5^5 * p^5 * \left(C_5^5 * 0.5^5 * 70 + C_4^5 * 0.5^5 * 70 + C_3^5 * 0.5^5 * 70 + C_2^5 * 0.5^5 * 70 + C_1^5 * 0.5^5 * 70 + C_0^5 * 0.5^5 * \left(\frac{5}{6}\right) * 70\right))) ,$$

where p denotes the equilibrium probability of donating.

The solution for p is 0.51.

APPENDIX B

THE PUBLIC GOODS GAME

B.1 Sample Instructions (Group Size of 8)

Phase I Instructions

You are now taking part in an economic experiment. If you read the following instructions carefully, you can, depending on your decisions, earn a considerable amount of money. It is therefore very important that you read these instructions with care.

The instructions which have been distributed to you are solely for your private information. It is prohibited to communicate with the other participants during the experiment. If you have any questions please ask us. If you violate this rule, we shall have to exclude you from the experiment and from all payments.

During the experiment we shall not speak of Dollars but rather of tokens. During the experiment your earnings will be calculated in tokens. At the end of the experiment the total amount of tokens would have earned will be converted to Dollars at the following rate:

40 tokens = 1 dollar

At the end of the experiment your entire earnings from the experiment plus the 5 Dollar show-up fee will be immediately paid to you in cash.

The experiment is divided into two phases, and the first phase consists of 10 periods. Before the first period, the participants are matched into groups of 8. You will therefore be in a group with 7 other participants. The composition of the groups will remain the same for the 10 periods. In each period your group will therefore consist of same participants.

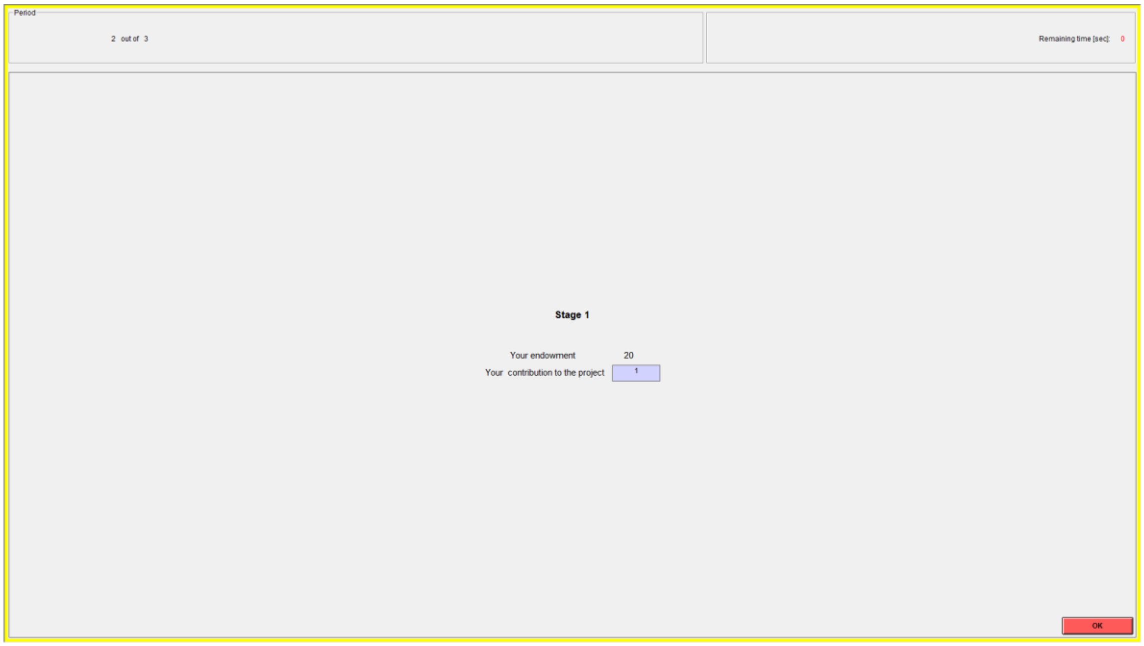
In each period the experiment consists of one decision. At this stage you have to decide how many tokens you would like to contribute to a project. The following pages describe the course of the experiment in detail.

Detailed Information on the Experiment

The First Stage

At the beginning of each period each participant receives 20 tokens. In the following we call this his or her endowment. Your task is to decide how to use your endowment. You have to decide how many of the 20 tokens you want to contribute to a project and how many of them to keep for yourself. The consequences of your decision are explained in detail below.

At the beginning of each period the following input screen for the first stage will appear:

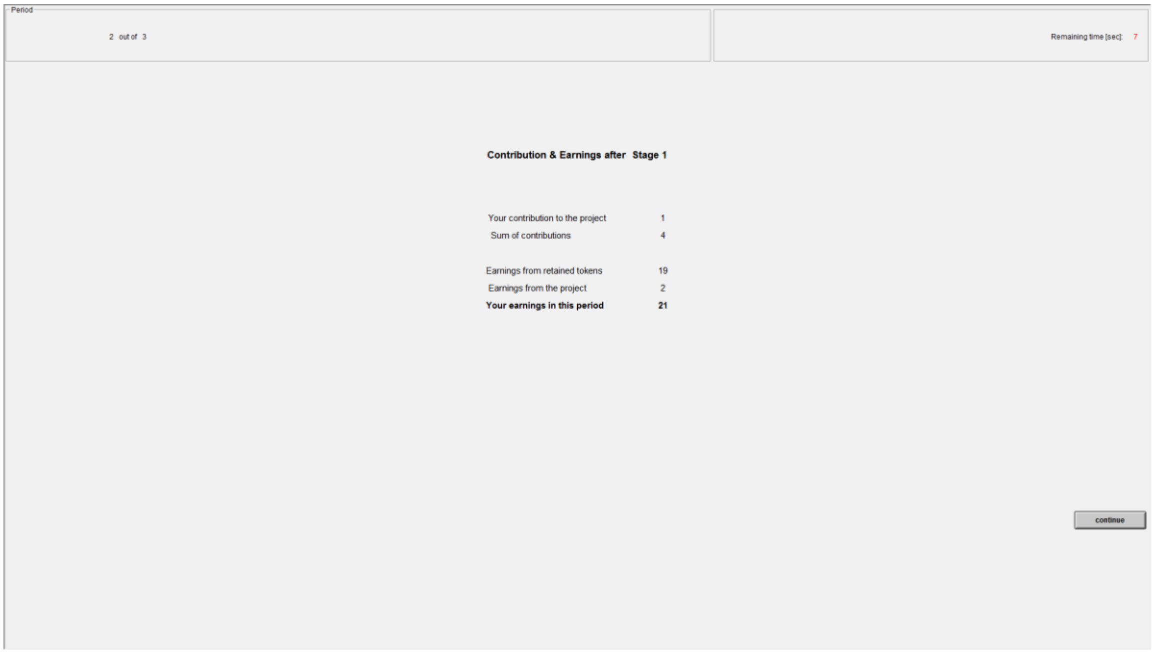


The number of periods appears in the top left corner of the screen. In the top right corner you can see how many more seconds remain for you to decide on the distribution of your tokens. Your decision must be made before the time displayed is 0 seconds.

You have to decide how many tokens you want to contribute to the project by typing a number between 0 and 20 in the input field. This field can be reached by clicking it with the mouse. As soon as you have decided how many tokens to contribute to the project, you have also decided how many tokens to keep for yourself. This is (20 – your contribution) tokens. After entering your contribution you must press the OK button with the mouse. Once you have done this your decision can no longer be revised.

After all members of your group have made their decisions the following income screen will show you the total amount of tokens contributed by all eight group members to the project (including your contribution). Also this screen shows you how many tokens you have earned in the first stage.

The Income Screen after the First Stage



Your income consists of two parts:

1) The tokens which you have kept for yourself ("Income from tokens kept")

2) The "Income from the project." This income is calculated as follows:

Your income from the project = 0.25 x total contribution of all 8 group members to the project

Your income in tokens at the first stage of a period is therefore:
(20 – Your contribution to the project) + 0.25*(total contributions to the project)

The income of each group member from the project is calculated in the same way, this means that each group member receives the same income from the project.

Suppose the sum of the contributions of all group members is 60 tokens. In this case each member of the group receives an income from the project of: 0.25*60 = 15 tokens. Now suppose that total contribution to the project is 9 tokens, then each member of the group receives an income of 0.25*9 = 2.25 tokens from the project.

For each token that you keep for yourself you earn an income of 1 token. Supposing you contributed this token to the project instead, then the total contribution to the project would rise by one token. Your income from the project would rise by 0.25*1 = 0.25 tokens. However, the income of the other group members would also rise by 0.25 tokens each, so that the total income of the group from the project would rise by 2 tokens. Your contribution to the project therefore also raises the income of the other group members. On the other hand you earn an income for each token contributed by the other members of the project. For each token contributed by any member you earn 0.25*1 = 0.25 tokens.

In every period you have 10 seconds to review the income screen. If you are finished with it before the time is up, please press the continue button (again by using the mouse).

Phase II Instructions

We will now repeat this experiment with one change. As before, the experiment consists of 10 periods and in each period you have to make a decision how many of the 20 tokens at your disposal you want to contribute to the project (and, implicitly, how many you keep for yourself)

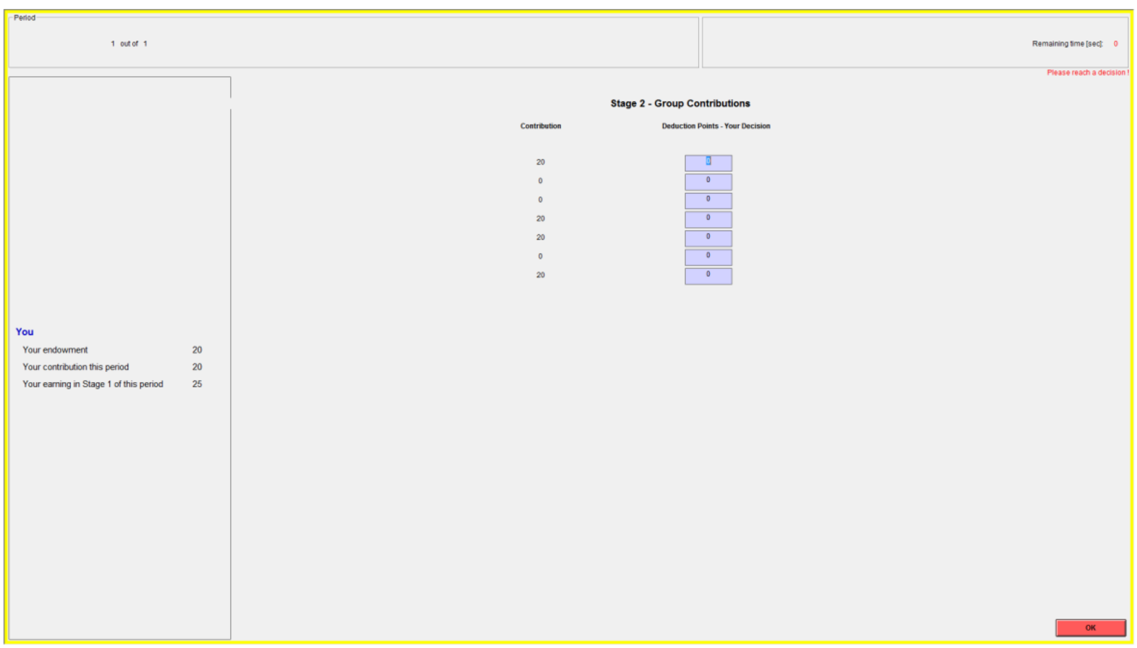
The Change

The second stage is added. In the following ten periods there will be a 2nd stage following the 1st stage. At the 2nd stage you are informed of the contribution of the other group member to the project. You can then decide whether or how much to reduce their earnings from the first stage by distributing deduction points to them. The instruction for the 2nd stage is as below:

The Second Stage

In the second stage you see how much the other group members contributed to the project. At this stage you can also reduce or leave equal the income of each group member by distributing deduction points. The other group members can also reduce **your** income if they wish to. This is apparent from the input screen at the second stage:

The Input Screen at the 2nd Stage



Besides the period and time display, you see here on the left how much each group member contributed to the project at the first stage.

You must now decide how many points to give to each of the other 7 group members by entering a number in the box. If you do not wish to change the income of a group member then you must enter 0. For your decision, you have 35 seconds in all the periods. You can move from one input field to another by using the mouse.

If you distribute points, you incur a cost which depends on the total amount of points you distribute. You can distribute between 0 and 3 total points to your group members. Note that the maximum total amount of deduction points that can be distributed is 3. The more points you give to the other group members, the higher your costs. Each deduction point you distribute to a group member costs you 1 token and deducts the group member's earnings by 3 tokens.

Suppose you distribute a total of 3 deduction points among some of the other 7 members in your group. In this case, your costs of distributing points would be 3 tokens. Your total costs of distributing points are displayed on the input screen. As long as you have not pressed the OK button you can revise your decision.

If you choose 0 points for a group member, you do not change his or her income. However, if you give a member 1 point (by choosing 1) you reduce his or her income from the first stage by 3 tokens. If you give a member 2 points (by choosing 2) you reduce his or her income by 6 tokens.

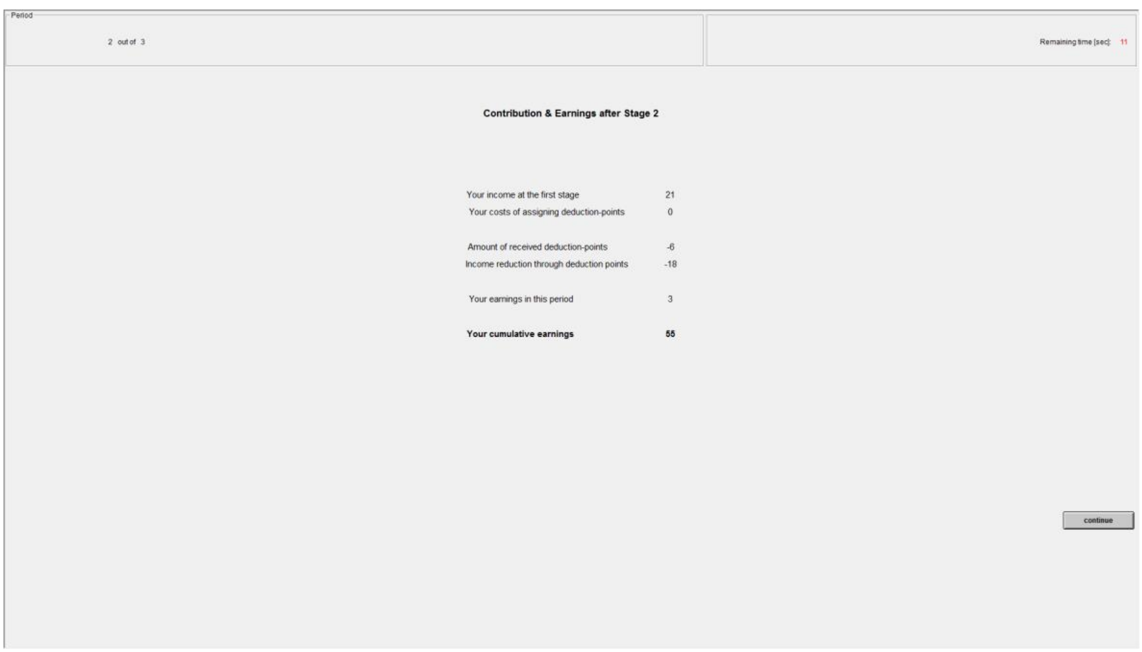
Whether or by how much the income from the first stage is reduced depends on the total of the received points. If somebody received a total of 7 points (from all other group members this period) his or her income would be reduced by 21 tokens. Your total income from the two stages is calculated as follows and the minimum income is 0 tokens.

Total income (in tokens) at the end of the 2nd stage = period income =

= income from the 1st stage – distributed deduction points*1– received deduction points*3

Remember that the maximum total amount of deduction points that can be distributed to the other group members is 3 and your minimum period income is 0.

After all participants have made their decisions, your income from the period will be displayed on the following screen:



The calculation of your income from a period, the costs of your distribution and your income in the period are as explained above. Do you have any further questions?

After the end of these 10 periods, the experiment is finished and you will get:

Your income in tokens from the first set of 10 periods
<u>+Your income in tokens from the second set of 10 periods</u>
= Total Income in tokens

(Total Income in tokens)/40 + 5 Dollars show-up fee

APPENDIX C

THE COLONEL BLOTTO GAME

C.1 General Instructions

This is a study of economic decision making. Your earnings in this study depend on the decisions that you make, the decisions of other people, and chance. You will be paid these earnings privately in cash at the end of the session today.

Please take a minute to turn off your cellphones. There is no talking during the study except to ask questions. If you have questions at any time, please raise your hand and someone will come and assist you.

C.2 Instructions, Part 1

In this game, you choose <u>one</u> of the six possible options, as shown below. Once you choose an option, the experimenter will pick up this sheet with your decision selected. (another part of the experiment may follow this) Then the experimenter will grab a paper bag with 12 poker chips inside. These poker chips are labeled with the integers 1 through 12, corresponding to your computer stations. The experimenter will randomly and blindly select a chip. The number on the chip the experimenter selects corresponds to the person responsible the picking the chosen participant. Then by the same process, this person will then randomly and blindly select one of the 12 chips in the bag. The number on the chip this person selects corresponds to the chosen participant. (Note that there is only one chosen participant and that the same person responsible for picking the chosen participant can be the chosen participant as well) Once the chosen participant is found, at the end of the session, a six-sided die will be rolled to determine whether the chosen participant receives payment A or payment B. If a 1, 2, or 3 is rolled the chosen participant receives payment A; if a 4, 5, or 6 is rolled the chosen participant receives payment B. You only play the game once.

When this game is completed, you will be paid the amount you earn in this game. Note: the dollar values in the each experiment are measured in US dollars.

Option	Payment A	Payment B
1	$12.00	$12.00
2	$8.00	$20.00
3	$4.00	$28.00
4	$0.00	$36.00
5	-$4.00	$44.00
6	-$8.00	$48.00

Examples:

If you choose option 1: If you roll 1, 2, or 3 you earn $12.00; if you roll 4, 5, or 6, you earn $12.00.

If you choose option 2: If you roll 1, 2, or 3 you earn $8.00; if you roll 4, 5, or 6, you earn $20.00.

If you choose option 3: If you roll 1, 2, or 3 you earn $4.00; if you roll 4, 5, or 6, you earn $28.00.

If you choose option 4: If you roll 1, 2, or 3 you earn $0.00; if you roll 4, 5, or 6, you earn $36.00.

If you choose option 5: If you roll 1, 2, or 3 you lose $4.00; if you roll 4, 5, or 6, you earn $44.00.

If you choose option 6: If you roll 1, 2, or 3 you lose $8.00; if you roll 4, 5, or 6, you earn $48.00.

Decision:

When you are ready please circle the option you prefer. Remember, there are no right or wrong answers, you should just choose the option that you like best.

C.3 Sample Instructions, Part 2 (8 Prizes, Repeated Matching Protocol)

For Part 2, earnings are described in tokens and at the end of the session the tokens will be added and translated into dollars at a rate of 200 tokens = 1 US dollar.

In this session, there are two types of people. Half of the people in the room will be assigned to the "Participant 1" type and the other half will be assigned to the "Participant 2" type. You will be randomly matched with another person in this room from the other type (called your "counterpart") and both of you will each make a series of decisions. You will not know who your counterpart is, and they will not know who you are. You will keep your type designation for the entirety of this session. Also, you and your counterpart will be matched for the entirety of this session. Your earnings for each decision will depend on the decisions that you make and the decisions that your counterpart makes.

Today, you and your counterpart will be facing multiple “boxes”. You have resources to spend to win these boxes. However, these resources are also valuable to you. In every period you and your counterpart will choose an action at the same time. You will not know what your counterpart has chosen before you make your own choice, and they will not know your choice before they make theirs. After both of you have chosen, you will find out what your counterpart did and the resulting earnings for you both.

If you have more resources in a given box than your counterpart, you will win the box. Each box won will have an earnings benefit. If you have fewer resources than your counterpart in a box, you will lose the box. If you and your counterpart have the same number of resources in a box, then you and your counterpart will have a 50% chance of winning the box.

Today, you and your counterpart will play 15 periods. For each period, you will have 200 tokens as resources to allocate across 8 different boxes. Your counterpart will also have 200 tokens to allocate across these boxes. Each box is worth 25 tokens to the winner and 0 tokens to the loser. For each period, you can allocate your resources in any amount in any number and combination of boxes you wish. Note that you do not have to spend your entire allotment of 200 tokens nor do you have to spend tokens in all boxes (some may be left empty). Resources that you do not spend in boxes count toward your earnings later.

In a given period, once you finish distributing your tokens to the 8 boxes, you will see the results of the number of boxes won/lost and your earnings for that period.

Once this is finished, you will move to the next period where the process starts over again with 200 tokens to distribute and 8 boxes. Earnings per period are cumulative and will be paid, in private, at the end of the session. Next are two examples to help explain the game.

Example 1:

Suppose you are Participant 1 and have allocated all of your 200 tokens across the following eight different boxes: (note, that the first column indicates the 1^{st} box, the 2^{nd} column indicates the 2^{nd} box, and so on up to 8 total boxes)

Participant 1 has 200 tokens

10	20	20	50	0	30	50	20

Participant 2 has 200 tokens

Next you will see what Participant 2 allocated across these boxes. (Remember, these decisions are simultaneous, you will not see the choices of the other participant until you have already made your choices per period and vice versa)

Participant 1 has 200 tokens

10	20	20	50	0	30	50	20
0	0	10	20	20	0	0	30

Participant 2 has 200 tokens

Participant 2 spent 80 of the 200 tokens on these 8 boxes. The middle row indicates which boxes you (as Participant 1) won.

Participant 1 has 200 tokens

10	20	20	50	0	30	50	20
Yes	Yes	Yes	Yes	No	Yes	Yes	No
0	0	10	20	20	0	0	30

Participant 2 has 200 tokens

You won 6 boxes and lost 2. Next we detail your and your counterpart's earnings for this period.

PARTICIPANT 1 PERIOD EARNINGS		**PARTICIPANT 2 PERIOD EARNINGS**	
Initial resource endowment	200 tokens	Initial resource endowment	200 tokens
minus		*minus*	
Total tokens spent	200 tokens	Total tokens spent	80 tokens
plus		*plus*	
Box winnings	150 tokens	Box winnings	50 tokens
(6 boxes x 25 tokens per box)		(2 boxes x 25 tokens per box)	
Period earnings	**150 tokens**	**Period earnings**	**170 tokens**

You earned 150 tokens and your counterpart earned 170 tokens from this period.

Example 2:

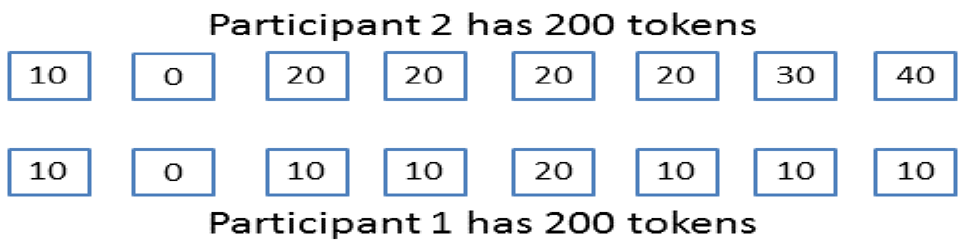
Suppose you are Participant 2 and have allocated 160 of your 200 tokens across the following eight different boxes:

Participant 2 has 200 tokens

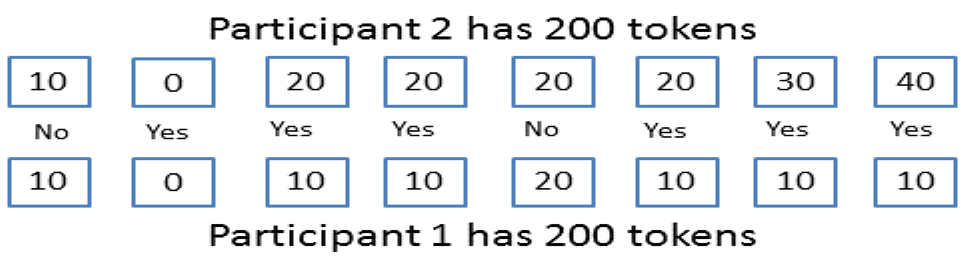
10	0	20	20	20	20	30	40

Participant 1 has 200 tokens

Next you will see what Participant 1 allocated across these boxes. (remember, these decisions are simultaneous, you will not see the choices of the other participant until you have already made your choices per period and vice versa)



Participant 1 spent 80 of the 200 tokens on these 8 boxes. The middle row indicates which boxes you (as Participant 2) won.



You won 6 boxes and lost 2. Note that you and your counterpart had the same number of tokens in boxes 1, 2, and 5. For these boxes, the computer randomly awarded box 2 to you and boxes 1 and 5 to your counterpart. Next we detail your and your counterpart's period earnings.

PARTICIPANT 1 PERIOD EARNINGS		PARTICIPANT 2 PERIOD EARNINGS	
Initial resource endowment	200 tokens	Initial resource endowment	200 tokens
minus		*minus*	
Total tokens spent	80 tokens	Total tokens spent	160 tokens
plus		*plus*	
Box winnings	50 tokens	Box winnings	150 tokens
(2 boxes x 25 tokens per box)		(6 boxes x 25 tokens per box)	
Period earnings	**170 tokens**	**Period earnings**	**190 tokens**

You earned 190 tokens and your counterpart earned 170 tokens from this period.

Once everyone has finished reading these instructions, please wait for the experimenter to reread them to you. After the experimenter has finished, you will be assigned your type and the session will start.

C.4 Sample Instructions, Part 2 (8 Prizes, Random Matching Protocol)

For Part 2, earnings are described in tokens and at the end of the session the tokens will be added and translated into dollars at a rate of 200 tokens = 1 US dollar.

In this session, there are two types of people. Half of the people in the room will be assigned to the "Participant 1" type and the other half will be assigned to the "Participant 2" type. You will be randomly matched with another person in this room from the other type (called your "counterpart") and both of you will each make a series of decisions. You will not know who your counterpart is, and they will not know who you are. <u>You will keep your type designation for the entirety of this session. Also, you and your counterpart will be a different person from the preceding period.</u> Your earnings for each decision will depend on the decisions that you make and the decisions that your counterpart makes.

Today, you and your counterpart will be facing multiple "boxes". You have resources to spend to win these boxes. However, these resources are also valuable to you. In every period you and your counterpart will choose an action at the same time. You will not know what your counterpart has chosen before you make your own choice, and they will not know your choice before they make theirs. After both of you have chosen, you will find out what your counterpart did and the resulting earnings for you both.

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In a given period, once you finish distributing your tokens to the 8 boxes, you will see the results of the number of boxes won/lost and your earnings for that period. Once this is finished, you will move to the next period where the process starts over again with 200 tokens to distribute and 8 boxes. Earnings per period are cumulative and will be paid, in private, at the end of the session. Next are two examples to help explain the game.

Example 1:

Suppose you are Participant 1 and have allocated all of your 200 tokens across the following eight different boxes: (note, that the first column indicates the 1st box, the 2nd column indicates the 2nd box, and so on up to 8 total boxes)

Participant 1 has 200 tokens

10	20	20	50	0	30	50	20

Participant 2 has 200 tokens

Next you will see what Participant 2 allocated across these boxes. (Remember, these decisions are simultaneous, you will not see the choices of the other participant until you have already made your choices per period and vice versa)

Participant 1 has 200 tokens

10	20	20	50	0	30	50	20
0	0	10	20	20	0	0	30

Participant 2 has 200 tokens

Participant 2 spent 80 of the 200 tokens on these 8 boxes. The middle row indicates which boxes you (as Participant 1) won.

Participant 1 has 200 tokens

10	20	20	50	0	30	50	20
Yes	Yes	Yes	Yes	No	Yes	Yes	No
0	0	10	20	20	0	0	30

Participant 2 has 200 tokens

You won 6 boxes and lost 2. Next we detail your and your counterpart's earnings for this period.

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Period earnings	**150 tokens**	**Period earnings**	**170 tokens**

You earned 150 tokens and your counterpart earned 170 tokens from this period.

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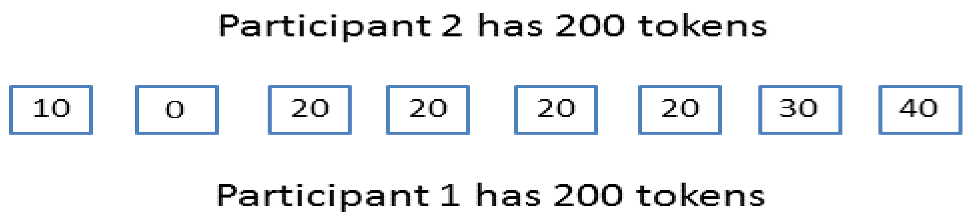
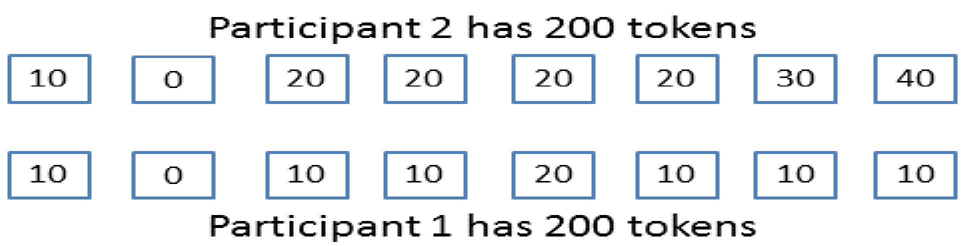
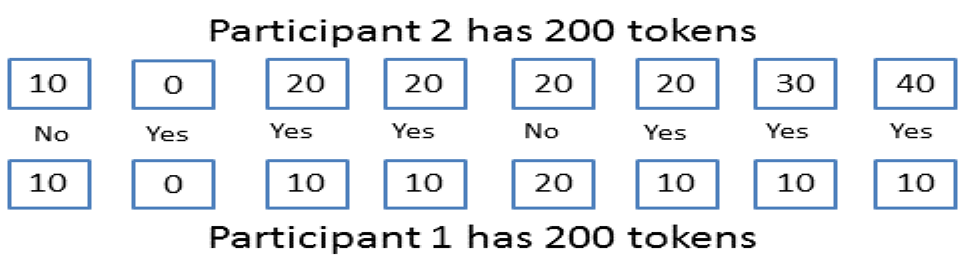
Suppose you are Participant 2 and have allocated 160 of your 200 tokens across the following eight different boxes:



Next you will see what Participant 1 allocated across these boxes. (remember, these decisions are simultaneous, you will not see the choices of the other participant until you have already made your choices per period and vice versa)



Participant 1 spent 80 of the 200 tokens on these 8 boxes. The middle row indicates which boxes you (as Participant 2) won.



You won 6 boxes and lost 2. Note that you and your counterpart had the same number of tokens in boxes 1, 2, and 5. For these boxes, the computer randomly awarded box 2 to you and boxes 1 and 5 to your counterpart. Next we detail your and your counterpart's period earnings.

PARTICIPANT 1 PERIOD EARNINGS		**PARTICIPANT 2 PERIOD EARNINGS**	
Initial resource endowment	200 tokens	Initial resource endowment	200 tokens
minus		*minus*	
Total tokens spent	80 tokens	Total tokens spent	160 tokens
plus		*plus*	
Box winnings	50 tokens	Box winnings	150 tokens
(2 boxes x 25 tokens per box)		(6 boxes x 25 tokens per box)	
Period earnings	**170 tokens**	**Period earnings**	**190 tokens**

You earned 190 tokens and your counterpart earned 170 tokens from this period.

Once everyone has finished reading these instructions, please wait for the experimenter to reread them to you. After the experimenter has finished, you will be assigned your type and the session will start.